Space Research: Directions for the Future

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SPACE RESEARCH

DIRECTIONS FOR THE FUTURE

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by the
SPACE SCIENCE BOARD

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FOREWORD

In the Fall of 1964 discussions between members of the Space Science Board and the National Aeronautics and Space Administration suggested that it was timely for the Board to undertake a study of certain principal areas of space research. Plans were made accordingly, and specialists from several scientific disciplines were convened in a Summer Study at Woods Hole during June and July, 1965. Space Research: Directions for the Future is the report of that study. Part I is devoted to planetary and lunar exploration. Part II takes up four branches of astronomy and some special topics in physics and geophysics. Part III discusses rocket and satellite research, university programs, biology, medicine and physiology, and the role of man in space research. The three parts, here combined in a single volume, were initially issued individually to make them available more quickly.

In contrast to the general review of space research in the 1962 study, the objectives in 1965 were limited: first, to develop a program of planetary exploration and to recommend priority within it; second, to determine the needs of astronomy in space; and, third, to consider the role of man in space research. All of these tasks were to be regarded in the light of the post-Apollo period, extending through about 1985.

Under the guidance and coordination of the study's general chairman, George P. Woollard, chairmen of individual working groups were encouraged to formulate their own plans early in February 1965. In some cases preliminary meetings were held in early Spring, specific assignments were made, and participants reported to the summer study with prepared papers in hand. NASA made available for advance distribution to all participants a variety of background information that provided a standard point of departure.

The study itself divided naturally into two 2-week sessions: (a) June 20-July 3: Working Groups on Optical Astronomy, Solar Astronomy, Radio and Radar Astronomy, X-Ray and Gamma-Ray Astronomy, Physical Sciences, Medicine and Physiology, and Biology and (b) July 5-July 16: Working Group on Planetary and Lunar Exploration. Related topics were covered in shorter sessions during the course of the study. On July 5 for the first session and on July 16 for the second session, the study convened in reporting sessions to discuss findings and recommendations. It would be unrealistic to attempt to reflect here all or even a large fraction of the recommendations to be found in this report. Some conclusions, however, are very clear to the Board and these merit emphasis.

We recommend planetary exploration as the most rewarding scientific objective for the 1970-1985 period. In pursuing this goal we recommend a reasonable balance between lunar and planetary programs. Within the planetary program we have established an order of importance.

All of our astronomy working groups project a need for large orbiting telescopes and anticipate the availability of man to adjust, maintain, and repair these national facilities.

All of the working group reports make clear that the exploration of space requires the utilization of both ground-based observations and studies with balloons, sounding rockets, and satellites.

The distinction between manned and unmanned programs is an artificial one; scientific objectives should be the determining factors.

The report of the Working Group on Medicine and Physiology concludes that before man can be safely included in missions of planetary duration, an orbiting research facility for the study of long-term effects of space flight is essential.

The Board wishes to acknowledge its gratitude to all those who participated in the 1965 study and in particular to its general chairman, George P. Woollard of the University of Hawaii, who also served as the chairman of the working groups on the Physical Sciences and NASA-University Relationships; and to the chairmen of the working groups: G.J.F. MacDonald (Planetary and Lunar Exploration), N.U. Mayall (Coordinator for Astronomy), Lyman Spitzer (Optical Astronomy), Leo Goldberg (Solar Astronomy), J.W. Findlay (Radio and Radar Astronomy), Herbert Friedman (X-Ray and Gamma-Ray Astronomy), W.W. Kellogg (Rocket-Satellite Research), L.D. Carlson (Medicine and Physiology), A.H. Brown (Biology), R.W. Porter (Man in Space and International Co-operation). The study was most effectively assisted by the Board's professional and secretarial staff: A.L. Carlson, G.A. Derbyshire, E.R. Dyer, Jr., A.L. Foss, T. Gikas, G.C. Marshall, H.G. Shepler, E.F. Tully, A. Wagoner, M.A. Wilson, under the direction of Hugh Odishaw. Finally, the Academy's facilities at Little Harbor Farm in Woods Hole, under the supervision of Mrs. Helen A. Barnum, provided the services so necessary to the pleasant and efficient conduct of our work.

Harry H. Hess, Chairman
Space Science Board

PART ONE

Chapter I. PLANETARY AND LUNAR EXPLORATION

PART TWO

Chapter II. OPTICAL ASTRONOMY

PART THREE

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PART ONE

I

Planetary and Lunar Exploration

INTRODUCTION

The exploration of the solar system bears on three central scientific problems of our time: the origin and evolution of the Earth, Sun, and planets, the origin and evolution of Life, and the dynamic processes that shape man's terrestrial environment. Not only do these problems directly bear on man's place in the universe, but they are closely related to the question of the origin of the universe itself. For these reasons, the exploration of the solar system has the enthusiastic support of a broad range of scientific specialists who are working toward the solution of these problems using the appropriate techniques of their disciplines. Of the underlying problems of natural philosophy the questions of the origin of life and the origin of the solar system have received the greatest attention from the public. It is highly appropriate that the prime goals of the nation's space program should be the elucidation of these fundamental cosmological and biological questions.

It is wise to plan and to execute the program of exploration in a manner that will yield the maximum of scientific data. To be sure, any such program is bound to be full of surprises, and we expect the unexpected at every step of the way. But this does not relieve us of the responsibility to plan, for only by careful planning will it be possible to exploit the new discoveries as they are made. Thus, our first task is to appraise the present state of knowledge of the solar system and to identify those problems that seem most susceptible of solution with the resources at our command. This done, it will be necessary to map a strategy for attacking those problems of greatest scientific interest, judged by their relevance to one or more of the key scientific questions stated at the outset.

The Working Group (see Appendix 1) began its work by hearing working papers prepared in advance of the study; these appear in section 9. These reports provided a basis for discussion of the various approaches to gathering data on the solar system. With this discussion in mind, the Working Group divided into panels (see Appendix 2) assigned to the different objects listed in the titles of sections 2-7; these sections comprise reports of those panels. The intent of these reports is to identify, quite specifically, the interesting problems and to indicate their scientific priority.

After considering these reports the Working Group attempted to assign over-all priorities to the various groups of objects on the basis of current scientific interest and on the bases of their relevance to the questions of the origin of the solar system, the origin of life, and the understanding of the Earth. A surprising degree of unanimity was evident. It should be emphasized, however, that these priorities are preliminary in the sense that they should be modified both by further debate among scientists in general and by the application of new data acquired. The list of priorities is given in Table I.

Table 1

Object	Rank
Mars	1
Moon, Venus	2, 3
Major Planets	4
Comets and asteroids	5
Mercury	6
Pluto	7
Dust	8

Mars was ranked first because of its relevance to all three central problems. Its relevance to biology has been documented in a previous report ("Biology and the Exploration of Mars"). In addition, it can be seen in section 2 that its similarity to Earth in many important respects will mean that study of Mars will throw much light on terrestrial geology, geophysics, and meteorology. It now appears that it has modest interest vis-à-vis the origin of the solar system, primarily because it is an object with many properties intermediate between those of the Earth and the Moon.

The Moon is naturally of great interest in its relationship to the Earth. As Earth's nearest neighbor it has been subjected to external conditions similar to those affecting the Earth. But on the Moon erosional processes may be less destructive than those on the Earth, and perhaps written on the primitive lunar surface are clues to the early history of the solar system. The Moon has undergone complex, tidally induced, dynamical interactions with the Earth These interactions have led to long-term changes in the Earth-Moon configuration; thus the Moon is of great interest in cosmological considerations.

Venus, in spite of an allegedly high surface temperature, nevertheless retains some limited biological interest, for the following reasons: the lack of complete certainty regarding the interpretation of the radio emission as thermal radiation from the surface, the possibility of elevated topography at lower temperature, and the possibility of development of life-forms suspended in the atmosphere. Furthermore, as a sister planet to the Earth, with a dense atmosphere of considerable meteorological interest, Venus ranks high in its relevance to terrestrial problems. Finally, the interior of Venus and its rotational and dynamical characteristics are of great interest in connection with the evolution of the solar system.

The major planets are assigned fourth place because of their great relevance to the creation of the solar system. There is a good chance that their chemical composition (particularly Jupiter and Saturn) represents a sample of the primitive solar nebula. Minor relevance for the origin of life is assigned because of the possibility that prebiotic molecules are generated in the reducing upper atmospheres of these planets by ultraviolet radiation.

Comets hold great interest for the origin of planets because they may provide either a chemical sample of matter from interstellar space or a sample of primitive solar system material. Investigations of asteroids are of interest in deciphering the abundant data already provided by the terrestrial investigations of meteorites. Mercury will be of some interest for the origin of the solar system, judging by its unusual orbit, rotational characteristics, and high density. Pluto's orbit is also unusual, but our ignorance of the object is so great that it is difficult to assess its importance at present. Interplanetary dust is certainly of interest, but the methods for investigating it are so different from those appropriate to the other objects that it can as well be studied as a routine part of missions to other bodies.

1. MAJOR RECOMMENDATIONS

The Working Group examined in detail the present programs of the National Aeronautics and Space Administration in lunar and planetary exploration. For administrative reasons, the manned and unmanned programs are largely separate. This division is artificial and does not correctly reflect the complementary nature of manned and unmanned techniques in studying the basic questions of the solar system. In developing our recommendations we adopted the point of view that the most appropriate and timely techniques should be used for answering scientific questions, whether these do or do not involve man in space.

STRATEGY OF LUNAR AND PLANETARY EXPLORATION

Today, approximately two-thirds of NASA's resources are devoted to lunar and planetary exploration. A substantial part of these resources are at present devoted to Earth-orbital missions, however, these are but steps in the approved Apollo mission.

1. We recommend that starting in the 1965-1975 period a shift of emphasis toward the planets and away from the Moon begin, progressing toward a roughly equal expenditure for lunar and for planetary exploration in the 1970-1985 period.

2. Within the category of planetary exploration we further recommend that primary emphasis be given to Mars, secondary emphasis be given to Venus and the major planets, and significant attention be paid to comets, asteroids, and Mercury (see sections 2-7).

3. In the period 1965-1985, we anticipate that unmanned experiments will probably provide the most significant contribution to the program of planetary exploration. We further anticipate substantial demands on NASA's limited resources for programs the justifications of which are based on considerations other than those of science. Since we believe the exploration of the solar system bears so directly on major central scientific questions of our time, we recommend that the percentage of support of lunar and planetary exploration be maintained over the 1965-1985 time period and be devoted predominantly to scientific objectives and that programs whose objectives are other than scientific be started only as additional resources become available.

MANPOWER AND FACILITIES FOR LUNAR AND PLANETARY EXPLORATION

The Space Science Board has recommended that the emphasis of the space program shift over the next few years from the manned lunar mission to a broadly based program of lunar and planetary research. This change will require a different balance of facilities with a very much stronger emphasis on the development of scientific mission capabilities.

The existing manpower and facilities assigned to the lunar and planetary programs are insufficient for the proposed program. We therefore recommend that urgent attention be given to increasing the manpower and facilities available to the lunar and planetary science programs.

ADVISORY STRUCTURE OF NASA

A program of planetary exploration involves a national commitment to scientific inquiry on a scale greatly exceeding that of the Earth satellite program, and requires deep involvement of scientists from a wide variety of scientific fields, including geophysics, meteorology, aeronomy, geology, biology, medicine, astrophysics, and engineering.

A single mission system can involve an immense effort and a long lead time. Consequently there is a need to make long-term commitments to major objectives, and to reach decisions that may overshadow development of large scientific areas for two or more decades in the future.

Both the national interest and the interests of the scientific disciplines involved demand that the maximum scientific competance be brought to bear upon the formulation and realization of long-term programs of NASA. Neither the present discipline-oriented NASA subcommittees, nor occasional summer conferences by larger groups, is adequate for this purpose, and it is essential that immediate steps be taken to rectify this weakness in the decision-making process.

We therefore recommend that an advisory committee to the Administrator be formed, largely of non-NASA scientists but including engineers, which, in collaboration with the appropriate Associate Administrators, shall be responsible to and provide the Administrator with continuing advice on current efforts and future planning in the area of planetary and lunar exploration.*

This group must be a working group willing to meet frequently, and it will need free access to all planning documents. Close collaboration with the Space Science Board and the Office of Science and Technology will be necessary. It should be free to develop such organizational forms (e.g., a subcommittee structure, continuing seminars) as it deems necessary for its effective operation.

SCIENTIFIC PRIORITIES

In sections 2-7 we list significant scientific questions relating to the members of the solar system. An attempt has been made to order these questions with respect to their relevance to the major problems outlined in the introduction. We recommend that, in developing missions and assigning payloads, appropriate consideration be given to the assigned scientific priorities.

EARLY USE OF SATURN V ON MARS MISSIONS

The Saturn IB-Centaur probably can land a small capsule on Mars on early missions but the limited payload would exclude optical image transmission, for which there are compelling arguments, and would place stringent limitations on other experiments to be carried. Later landing missions including an automated biological laboratory and a variety of physical experiments will require a larger booster, presumably Saturn V. The sooner the conversion to a Saturn V booster can be achieved the more effective will be the scientific program in the early phase and the more economical it may be in the long run.

The exobiology mission places an urgency on the Mars investigation that ordinarily would not exist. The longer definitive experiments are delayed, the greater the chances of contamination by accidental impact before they can be carried out.

The requirements of the planetary program for larger scientific payloads than will be possible in the Voyager program, if based on the Saturn IB-Centaur booster, make it imperative to introduce a larger booster as early as feasible in the program. In light of the excellent progress on Saturn V, we recommend that the Office of Space Science and

* The Space Science Board, at its meeting November 12-13, 1965, agreed to enter into discussion with the NASA Administration to explore the best mechanisms for providing such continuing advice.

Applications (OSSA) and the Office of Manned Space Flight (OMSF) jointly undertake a study of the early use of Saturn V for exploration of the planets with special emphasis on a Martian capsule landing in the early 1970's.

EARLY VENUS PROBE

The priorities of biological exploration of the solar system and the general strategy of the exploration of Venus would be profoundly influenced if the surface temperature of Venus should prove to be lower then is currently supposed. A simple drop sonde measuring at least temperature and pressure during its entry should be given high priority. A small payload could be delivered with existing boosters, and consideration should be given to doing this (see section 4) at the 1968 or 1970 opportunities.

DEEP SPACE INFORMATION TRANSFER

At present, a major limitation to planetary exploration is the inadequate capacity for the transfer of information over interplanetary distances. The proposed expansion of the Deep Space Instrumentation Facility (DSIF), at least in part, will meet the needs of the planned Voyager program. However, the expanded facility will not be able to support the planetary and lunar exploration program recommended by this working group. This working group therefore recommends that NASA consider a further enlargement of deep space communication capabilities. Five areas of possible improvement are:

1. The use of large phased arrays employing medium-sized (85-125 foot) dishes.
2. Improvements in payload power supplies.
3. Improvements in on-board storage and processing of data.
4. Communication by laser beam with the receiver placed high in the atmosphere or above it.
5. Return flights in which large quantities of information (primarily photographic) can be delivered to Earth or telemetered in the vicinity of the Earth.

ROCKET POINTING CONTROL

We recommend that immediate steps be taken to develop a fine pointing control for rockets capable of guiding the entire payload to 10 sec of arc with a maximum jitter of $\pm$ 2 sec and maintaining the orientation for 30 seconds of time. This requirement is discussed in section 4 and "Upper Atmosphere of the Planets", section 9, p. 106, and a similar requirement has been put forward by the Working Groups on Optical Astronomy and Rocket-Satellite Research.

ORBITING PLANETARY OBSERVATORY

We recommend that at least one Orbiting Astronomical Observatory (OAO) with versatile spectrographic equipment should be launched for planetary studies at an early stage in the planetary and lunar program. As the program develops it is possible that there will be further requirements for the capability. (See "Meteorological Exploration of Mars" in section 2, the Conclusion to section 4, and "Upper Atmospheres of the Planets" in section 9.)

A 100-in. telescope in far Earth orbit, preferably with manned support, could provide high-resolution data on the planets. At this stage of the program we believe that the magnitude of effort involved in the mission cannot be justified, but the problem should be kept under consideration.

OPTICAL TELESCOPES AND THE SPACE PROGRAM

The planning and execution of a scientific program of planetary exploration must utilize all available methods with proper emphasis upon those of relatively low cost which are easily accessible. Of particular importance in this context is the large, ground-based optical telescope.

There are examples in recent years of programs of planetary observations that have not had the priority they should have been accorded if they had been considered in the light of their importance to an expensive national program. Priorities in astrophysical observatories are, quite rightly, awarded on the basis of over-all contribution to astronomy. In order to introduce a different set of priorities it will be necessary for NASA to continue and, probably, expand its support of ground observatories in a few locations.

The type of observation most needed at the present time for planning future missions is high spectral resolution spectrometry and high spatial resolution photometry on Jupiter, Mars, Venus, and Mercury. Long-term programs with maximum variation of parameters should be made. A selection of the programs that we have in mind is as follows:

1. The thermal mapping of Murray and his colleagues presents the possibility of a preliminary study of some features of planetary meteorology. The Venus observations should be repeated on a day-to-day basis so that the development of atmospheric systems can be studied. Similar routine work should be undertaken on Mars and Jupiter.

It cannot be overemphasized that such observations are potentially just as valuable and may even accomplish the same results as certain space probe observations, e.g., thermal mapping from orbiters. What telescopic observations lack in spatial resolution they make up in

(a) accessibility to members of the academic community, e.g., graduate students; (b) repeatability; and (c) flexibility with which the program can be developed and improved.

2. The quantitative and routine exploration of the Venus terminator in two or three colors gives direct information about the time variation of the structure of the Venus cloud layer, and some data on composition. Combined with thermal mapping one has almost a routine meteorological sounding of the upper cloud layer. We cannot responsibly ask for deep space probes to investigate planetary structure unless these observations have been developed to the maximum extent possible.

3. Detailed spatial and spectral resolution of the different CO_2 bands on Venus on a routine basis. We can hope for pressure, concentration and temperature measurements as a function of time and related to changes in height and motions of clouds.

4. Continued spectrographic study of the CO_2 bands on Mars.

5. The terminator of Mars has not been investigated properly. It could yield a surface pressure and some information on the meteorology of dust clouds.

6. It is possible that a water-vapor feature on Mars could be used to trace water transfer; this is one of the prime questions of biological interest.

7. The Venus and Mars dayglow, nightglow, and atmospheric fluorescence (ring effect) should be thoroughly investigated.

These and other telescopic programs vital to the planetary missions will, with present facilities, either not be performed at all, or only in a perfunctory way. The expansion of facilities proposed by the Whitford report ("Ground-Based Astronomy: A Ten-Year Program," National Academy of Sciences—National Research Council, 1964) may partly relieve this problem, but it is still dangerous to leave the demands of a large national program subject to the priorities of groups which are not directly involved. The two telescopes at present under construction with NASA funds are a more direct contribution, but the entire experiment contemplated at present may be insufficient for these reasons:

a) The high spatial and spectral resolution discussed requires the largest telescopes available. Practical considerations suggest that attention should be focused upon a telescope similar to the 150-in. Kitt Peak instrument, since this is on the way to development. There are no plans to give planetary observations overriding priority for long periods on such an instrument.

b) We are dealing with a meteorological type of geophysical problem. Variability is the essence of the problem. The programs discussed, and those not listed for lack of time and specialized appreciation, all have to be repeated on a routine basis. Such programs would never be contemplated by classical astronomers for obvious reasons, for they require excessive telescope time. But, from the point of view of NASA'S expenditures, the costs are very low indeed. Thus the equipment asked for by the Whitford report will not be available for the time that we consider necessary.

c) The Whitford committee had no information on the extent of the large scientific program planned by this study group.

d) There are sound reasons for a longitudinal and southern hemisphere coverage in connection with the space program that may not be met by the instruments recommended per the Whitford report.

This group has not had the time or breadth of knowledge to formulate exact proposals for NASA. It should be pointed out that NASA is not being asked to support academic astronomy but to support its own interests. Coincidently there is some overlap.

We know of examples where existing facilities have been insufficient and we know of detailed planning based on slipshod observations leading to avoidable situations in which poor science was backed by huge federal funds: we forsee more and more acute problems in the future.

We recommend, therefore, that NASA give very high priority to the construction of ground-based telescopic equipment to the extent required to provide maximum support to the planetary flight program.

We further recommend that NASA and the Academy make a joint study of the programs that could be undertaken in planetary astronomy, with the aim of identifying the gap between existing and projected instruments and the needs of planetary exploration.

SCIENTIFIC PASSENGER FOR LUNAR EXPLORATION

In the long-range systematic exploration of the Moon the scientific fruitfulness of observational work and sampling on the lunar surface will depend heavily on the experience of the observer (see section 3). We recommend that opportunities be created as early as possible for highly experienced observational scientists to be taken to the Moon, essentially as passengers, in addition to the scientifically trained astronauts.

THEORETICAL EFFORTS IN PLANETARY SCIENCE

The present theoretical effort in support of the exploration of the solar system is inadequate when compared with the presently approved observational program. As the planetary and lunar programs further develop, the disparity between the theoretical and observational programs should be reduced. Such problem areas as multibodied gravitational interaction, turbulence, magnetohydrodynamics, and convection are fundamental to the understanding of the origin and evaluation of the solar system, yet these are questions in which only modest advances have been made in the past fifty years. In part, the lack of theoretical efforts in this area is due to a shift in emphasis in Physics and Mathematics Departments and Institutes away from those areas of classical physics and mathematics of particular concern to the space program.

We therefore recommend that NASA give increased attention to the development of both research and educational capabilities in those areas of theoretical physical sciences particularly appropriate to study of the solar system.

2. MARS

SCIENTIFIC QUESTIONS IN THE EXPLORATION OF MARS

The major scientific questions to be asked in the exploration of Mars can be grouped under six main headings:

Exobiology

Is there or has there ever been life of any kind on Mars? What is the chemistry of this life? How is it adapted to an environment quite different from the Earth? What is the past environment from which it evolved and what is the evolutionary sequence of life-forms? On the other hand, there may be no life at all, either extant or extinct, in which case the problem is one of search for proto-organic materials. The biological aspects of the exploration of Mars have already been discussed in some detail in the report of a Space Science Board group under the title "Biology and Exploration of Mars" (published by NAS—NRC, April 1965, 19 pp.) to which reference should be made.

Differentiation

Is Mars differentiated into a core, mantle, and crust and are there areal as well as vertical differences? If differentiated, then is the process still functioning or is this a feature accomplished during the original accretion of the planet or by subsequent radioactive heating and chemical processes? The prime requirements are refinements in determination of mass, shape, and moments of inertia, coupled with natural or active seismic exploration and heat flow measurements.

Activity

Is Mars now, or has it ever been an active planet from a seismic, volcanic, and tectonic point of view? The answer will necessitate search for present and past tectonic patterns; investigation of types and classes of folding, faulting, mountains, and craters; determination of seismic activity, heat flow, magnetic field, and many other standard geophysical parameters. The combination of these data with similar data about the Earth will provide a powerful couple for understanding the internal driving forces of both planets.

Composition

What is the physical, chemical, and mineralogical composition of Mars? Did Mars come from the same chemical crucible as the Earth and what physical and chemical processes have operated to modify its original constitution? The answers will require analysis of volcanic rocks, atmosphere, sediments, any volcanic gases, and determination of some of the isotopic ratios. Of considerable importance is the role of water, possibly in fossil oceans, in these processes as compared with its major role in development of Earth's geochemical character.

History

What has been the history of Mars? The time-scale and sequence of major events on the planet must come from the rock record of those events. This means crude reconnaissance geological mapping by a variety of remote image devices to work out the sequence of superposition of the various strata and their relationship to major events in the planet's history. Coupled with this must be an understanding of the interactions of the dominant surficial processes of erosion, transport, and deposition. This time sequence must eventually be keyed into absolute ages, first by estimation of rate processes and eventually by radioactive age dating of some samples.

Atmospheric Dynamics

What is the character of the general circulation of the Martian atmosphere? Is it in the symmetric or the wave regime, and does the regime change with season? If in the wave regime, what is the characteristic wave-number, amplitude, phase speed, and internal structure of the waves? What is the relation of this large-scale circulation to the thermal structure and to the cloud forms and violet layer on Mars? What nonhydrostatic or mesoscale circulations are present in the Martian atmosphere? What is the character of the surface boundary layer? How do the boundary layer motions, the mesoscale motions, and the general circulation interact to raise dust particles from the surface and to transport the suspended dust and wind-blown sand? How do these various scales of motion affect the transport of water vapor within the atmosphere, especially the cross-equatorial transport, with the corresponding geographical and seasonal variation of the flux of water vapor across the air-ground interface? The answers to these questions, which can be obtained from theory combined with observations from instrumented orbiters and entry probes, will increase our knowledge of the dynamics of planetary atmospheres and provide essential information for many aspects of the biology and surface geology of Mars.

RECOMMENDED PRIORITIES IN THE EARLY STAGES OF INVESTIGATION

An early flight planned as part of a series of probeborne investigations of Mars will necessarily have to determine certain properties of the Martian environment of critical importance to the design of subsequent, more advanced missions. We must deal with two separate considerations:

1) The need to obtain that specific information about the Martian

surface which is of most general usefulness in the design of subsequent Voyager missions.

2) The need to explore the planet in an efficient way, putting priority on the surveys and experiments that have maximal relevance to later scientific investigations.

These considerations are predicated on the importance and urgency of early biological exploration.

We have listed below the characteristics of the Martian surface environment that belong at the top of any hierarchy of relevant information. That is, we feel that the items listed here deserve priority on the basis of their importance to later missions, both in terms of specific mission-oriented data and general scientific exploration. They also reflect the needs of both biological and nonbiological interests.

Category I. Specific aspects of the Martian environment, the study of which is to be given highest priority in the early phases of the Voyager program.

1. Existence of Life

Specific life detection experiments should be incorporated in the early landers even though they may have a low *a priori* chance of success. In the event of severely limited payload capability the emphasis on life detection may have to be decreased in order to permit high-priority environmental measurements to be taken.

2. Chemistry and Geology of Surface Water

Distribution of ice and moisture in and on the soil. Rates and mechanisms of water movement, both locally and in the planetary water cycle.

3. Carbon Chemistry

Presence of carbon compounds, including elementary carbon, in the soil. Occurrence of nitrogen in carbon compounds.

4. Micrometeorology

The temperature, wind velocity and direction, humidity, carbon dioxide concentration, dust flux and particle size distribution, and their diurnal variations.

5. Surface Environments

Identification and classification of the major physiographic and surficial features of the planet, including a distinction between permanent and variable features over a significant fraction of the planetary surface. Particular attention should be paid to determining differences in environmental conditions from place to place.

Category II. Broad-band Exploration of Mars. General items of high priority as distinguished from the specific observations recommended in Category I.

1. Photographic three-color reconnaissance of surface and clouds.
2. Determination of atmospheric and surface composition.
3. Reconnaissance of thermal emissions from the surface in the infrared region.

The early missions should thus be oriented to fulfill the tasks outlined in the first paragraph above. In addition, we shall consider a series of questions that are not so urgent but the answers to which are of enormous intellectual importance. Since the study of these questions can provide an early scientific yield without necessarily interfering with the two main considerations they should be assigned the next-highest priority; in any early missions where payload restrictions are not severe, at least some of these experiments should be carried. The sequence of the following items does not imply any ranking.

1. What are the Martian polar and equatorial radii (± 1 km)?
2. What is the magnetic field strength and ion density around the planet?
3. Which noble gases are present? What is their abundance and isotopic distribution, especially A^{40} and other radiogenic species?
4. What is the degree of Martian seismicity?
5. What is the mineral composition of the surface at the landing site, especially with respect to Si, Fe, Al, K, Ca, Ni, S, U, Th, Na, and Mg?

SOME ASPECTS OF MARTIAN EXPLORATION: GEOLOGICAL, GEOPHYSICAL AND METEOROLOGICAL

Geological Exploration of Mars

The geology of Mars is particularly important to us because, probably more than any other celestial body with the possible exception of Venus, Mars may be expected to have paralleled somewhat closely the Earth in its origin and development. Thus, not only its similarities, but also such differences as exist, should provide a most valuable control on the validity of many of our tentative conclusions about the Earth and Earth processes, and should be an aid to the solution of existing uncertainties and unknowns about the Earth and its history and about the solar system in general.

Basically, the geologic knowledge we need about Mars is (1) its physical, and chemical constitution, (2) the processes which are modifying or have modified this constitution, (3) the record of life, if any, and (4) the relative and quantitative dating of the major events in the history of the Martian planet—the record of which is preserved only in the Martian rocks. In essence, therefore, the nature of what we need to know about the geology of Mars is much the same as what we have been trying more or less successfully to find out about the geology of the Earth. The two planets, taken together, would form a couple far more powerful in illuminating the general setting in which we live than the geology of the Earth (or Earth and Moon) alone.

The working paper by D. Wise and J.W. Salisbury on "Geology of Planetary Surfaces" (section 9, p. 90) discusses the importance of information on the geology of planetary surfaces in general, and many of the problems and methods involved. It provides an introduction to consideration of individual planetary surfaces. The present paper deals specifically with Mars and only with the very early approach to its geological study. A somewhat more advanced stage in planetary geological investigation is treated in section 3, pp. 20 and 26. Much of what is contained in these papers may eventually be applicable to Martian exploration also.

Information currently to be sought from Mars falls within the scope of many disciplines, but most of this information will apply directly or indirectly to the geology of Mars and most of it will achieve its greatest usefulness only when seen in the context of a proper understanding of Martian geology. The outstanding importance of a biological investigation of Mars has been rightly emphasized, but even "biological questions", as stated in the foreword to the recent report of a Space Science Board Working Group on Biology and the Exploration of Mars, "should be asked in an ordered sequence of exploration whose purpose is to understand the over-all evolution of the planet's crust and atmosphere."

Two principal lines of approach to the geology of Mars are evident. They are complimentary and should be closely coordinated. The first involves analysis and measurement—physical, chemical, and biological—of properties and components of the planet and its atmosphere:

Chemical composition and density of the Martian atmosphere. This factor may have had profound influence on the crustal rocks of Mars as well as significance regarding the history of the planet.

Surficial water content of the Martian crust—polar ice caps? permafrost? adsorbed water? hydrated minerals? Information regarding the presence of water on Mars, past or present, is important for many reasons, both geological and biological, and particularly for comparisons with Earth.

Physical, chemical, and mineralogical composition of surface rocks. Fundamentally important information which may also reflect internal composition and show geologic processes which have been in operation. Identification of carbonates and evaporites might be particularly significant of past aqueous environments. Carbonaceous sediments and hydrocarbons would be particularly important with respect to biology.

Intensity of cosmic and solar radiation. Significant with respect to environment for life. Also, radiation-formed nuclides may indicate the exposure history of planetary surfaces.

Extent of accumulation of cosmic or other extra-Martian meteoritic material. May be indicative of degree of activity of transport or of activity of other agents of surface rock genesis. May also control possibilities of finding a primordial or early surface exposed anywhere.

Composition of fluid emanations, if any, from Martian crust. Indicative of internal constitution and activity.

Heat flow and variations in thermal activity. May be significant of differences in radioactive content and related differences in distribution of lithologic types. May indicate proximity of igneous activity to surface. Infrared surveys could be particularly helpful.

Variations in radioactivity and radioactive products. May indicate distribution of acidic vs. basic rocks. Important also with respect to thermal budget and extent of degassing of interior.

Indications of organic activity, and analytical evidence of organic or proto-organic compounds. Indicative of life, past or present, or significant of biologically interesting conditions.

Age determinations by radioactivity. Important for chronostratigraphy, geological history, and recognition of early surfaces of the planet.

More exact determination of the figure of Mars. Important in geophysical calculations of internal structure of planet.

Measurement of geophysical properties and their distribution (gravity field and density variations, magnetic field and remanent magnetism, seismic velocities, and current seismic activity). Most effective means of determining internal structure of planet, degree of differentiation, and distribution of rock types in interior.

The second, and at least equally important, approach to the geology of Mars is through more conventionally geological observation and interpretation of the more prominent surface features of the planet, either directly or by means of visual-range photography and remote sensing devices. The importance of photography as an early step in the exploration of Mars, both because of its relative feasibility and because of the significance of the results, should scarcely need emphasis. In the first place, photography supplies the initial map base to which it is essential that most kinds of geological data be related if they are to have full significance. Further, it provides the readiest means of identifying the many sorts of geological surface features that may contribute to understanding the character and history of the planet. Some of the features of major significance that we might hope to determine on Mars from this second type of investigation are listed below with items that have already been mentioned under the first category:

Constructional and erosional land forms (mountains, valleys, plains, basins, dunes, etc.) Significant of past and present processes of deposition and erosion, uplift and depression. Radar surveys may be a particularly valuable supplement to conventional photography.

Provinces of distinctive physiography, color, albedo, or other physical properties. May reflect regions characterized by certain environments or certain lithologies.

Impact phenomena (impact craters, splash areas, etc.) Indicative of extent of meteor bombardment.

Igneous rock features (volcanic cones and craters, lava flows, igneous plugs, dikes, etc.) Significant of internal igneous activity, past or present.

Stratified rocks and their nature, thickness, and distribution. Significant of sedimentary processes (or igneous rock layering). Are there any water-laid strata?

Folded or tilted strata. Significant of tectonic activity.

Fault lines (and their displacements) and fracture patterns. Significant of tectonic activity.

Chronological sequence of rock units and features. Interrelations of rock units and of surface features may provide evidence of their chronological sequence and consequently of geological history.

Direct evidence of life. Identification of living organisms or their fossil remains by direct observation. Perhaps only ground observations will suffice, but photographs or remote sensing devices may pick up indicative features such as large vegetated areas.

Climatic evidence (polar ice caps, dust storms, clouds, atmospheric circulation, seasonal and longer-period changes, etc.) Significant of environments and of processes to be expected.

Features such as those listed in the above category may be of great significance not only with respect to surface character and surface environments but also with respect to the fundamental structure and state of activity, past and present, of the planet. Were there major features on Mars corresponding to our continents and ocean basins? large discrete regions of acidic rocks and of basic rocks? isostatic adjustments to relief of lithological variation? geosynclinal troughs? folded mountain belts? Does Mars show a planet-wide shear pattern? evidence of expansion or contraction? periodic deformation? All topography has an underlying reason; all uplifted or deformed strata have a tectonic significance; and such features of the Martian surface may be the most potent keys we have to the interior of the planet.

The great value of photogeological studies, even by themselves, in the exploration of extraterrestrial bodies has been amply demonstrated in the wealth of information that has been obtained about the Moon through telescopic and Ranger photographs. An essential element of geology, as contrasted with most other sciences, is its concern with time, chronostratigraphy, and the historical sequence of events. The contribution that photography has made to a study of the relative ages of materials and features on the Moon, makes it reasonable to expect that photographs of Mars will allow great contributions to the working out of Martian geological history, paving the way for more detailed work at the surface of the planet. Planning should be for an orderly, long-range, and continued program of geological exploration within the framework of facilities that can be made available, and possibly leading eventually to more comprehensive surface exploration of the Martian planet, perhaps including manned landings.

While much in Martian geology can doubtless best be interpreted from a background of the geology of the Earth, certain distinctive features of Mars (low water content, rare atmosphere, low gravity, etc.) may indicate a geology quite different from that of Earth. Many new features and processes may be encountered in Martian exploration. However, striking differences are also found between different regions on the Earth, although perhaps lesser in degree, and the adequately Earth-trained geologist is already prepared for some measure of nonuniformity wherever he goes. One of the things we seek to know about Mars is what is unknown on Earth. One of the things we must expect on Mars is the unexpected.

Interior of Mars

The most significant question to be asked about the interior of Mars is whether it has been differentiated. Does it have a core, mantle, and crust? Differentiation may have been accomplished during the original accretion of the planet or, if the internal temperature was high enough, by chemical processes after accretion, or both processes may have operated. If differentiated, is the process still functioning, or has it terminated so that the planet is essentially dead?

Refinement of the determination of mass and radius (shape) and determination of the moment of inertia from the orbital characteristics of Phobos and a transponder on the Martian surface would give information on gross internal density distribution and either body strength (departure of shape from that of an ideal rotating fluid) or internal dynamic disturbance of shape.

The absence of a sizable magnetic field on Mars is not convincing evidence favoring a liquid core. Natural or artificial seismic exploration of Mars would prove whether it was differentiated and give the thicknesses of its crust and mantle as well as whether its core was liquid or solid. It would also give its elastic properties and could lead to a refinement of its density distribution with depth.

Meteorological Exploration of Mars

1. Introduction

The ephemeral and changing cloud forms and their rapid motions indicate that Mars has a meteorologically active lower atmosphere. The Martian atmosphere and its lower and upper boundary conditions are similar enough to the Earth's atmosphere in some respects and different enough in others to make the atmosphere of Mars one of the more interesting problems in planetary fluid dynamics in its own right. However, in an assignment of priorities for the over-all Mars exploration program the initial effort must be that which helps to describe the environment for exobiology. Thus while meteorology per se occupies a secondary role it can be a powerful support for biological exploration.

The distribution and transport of water within the atmosphere of Mars and in the upper layers of the Martian soil will be a key environmental factor in the question of Martian biology. The moisture transport, both global and local, is controlled by the atmospheric circulation in response to the incoming solar energy. A knowledge of the atmospheric circulation will therefore allow a much better understanding of the broad-scale climate and fairly good estimates of the local climate for a wide range of surfaces. As on the Earth, topography, soil type, Sun angle, and albedo can influence the local climate, but the general circulation is the primary control. Knowledge of the Martian general circulation therefore holds the key to determining representative spatial and temporal variations of soil and atmospheric moisture content. Clearly, the atmosphere is the carrier that moves heat, moisture, and even the soil itself about the planet.

2. Observations required

Many of the measurements required to understand the Martian atmospheric circulation can be made initially from outside the planet. Some measurements, such as albedo and infrared loss as functions of latitude, are best made in this way. Ideally, the atmospheric composition and surface pressure should be measured by direct entry into the atmosphere, but a fly-by or orbiter might give adequate information. The amounts of the radiatively active gases—principally carbon dioxide—must be determined, but it is their radiative properties that make it possible to measure them from the outside as well. It is not necessary to know the composition of the soil even though the atmosphere is heated by contact with it; insofar as the general circulation is concerned the soil can be treated as an insulator. While existing mathematical models treat the atmosphere as a closed physical system under the influence of solar energy and predict the radiative loss to space, it is necessary to measure the radiative heat loss as a control. We can expect that the difference between the net solar input and the radiative heat loss on Mars will be significantly less than it is on Earth, and so a sensitive control observation can be made.

As the Martian exploration program develops a greater capability, it will be possible to obtain even better controls. The horizontal and vertical structure of the Martian atmosphere can be determined by infrared and microwave radiometry. Such a program is under development for probing the Earth's atmosphere. At a later stage, the direct measurement of the atmospheric circulation on Mars can be made by releasing a few small, constant-level ballons into the atmosphere of Mars and tracking those balloons from a Mars-orbiting satellite. Systems of this kind are now being built to explore the Earth's atmosphere.

Microclimatic measurements. Given the general circulation, and the local radiative heat budget, it is possible to infer a substantial amount of local climatology. It is particularly useful to have observations of the diurnal and seasonal variations of surface temperatures with as much geographic resolution as possible. Both visible and infrared measurements are required and these must be made in such a way that the total solar energy and infrared emission for each image element can be determined. If images of the reflected solar energy and thermal emission are made with different camera systems, having different observing angles so the radiative energy budget of the surface elements is lost, a large part of this type of data's usefulness to the micrometeorological picture is also lost. The diurnal and seasonal observations require an orbiter.

Some fairly straightforward observations of important meteorological parameters that can be made early in the Martian exploration program may be of considerable importance to exobiology. Further, as the technical capability of the program improves, a series of more sophisticated observations can be made that will yield fundamental understanding of planetary atmospheric motions *per se*.

3. THE MOON

SCIENTIFIC QUESTIONS IN LUNAR EXPLORATION

Major questions in the exploration of the Moon fall chiefly in three categories of basic problems: 1) structure and processes of the lunar interior, 2) the composition and structure of the surface of the Moon and the processes modifying the surface, and 3) the history or evolutionary sequence of events by which the Moon has arrived at its present configuration. These are principal categories of significant questions that can be asked of every object in the solar system of planetary or subplanetary dimensions. In the case of the Moon special interest resides in the effects impressed on the lunar surface by such general processes operating in the solar system as the impact of solid bodies and of charged solar particles and in the physical record of such effects, especially for the early part of the history of the solar system. The possibility that ancient rocks and deposits on the Moon's surface may contain a unique record of events related to the formation or accretion of the terrestrial planets gives the scientific exploration of the Moon unusual potential significance. There is also the minor possibility of finding prebiotic material, either buried or in sheltered locations.

The major questions are as follows:

Structure and processes of the lunar interior

(1) Is the internal structure of the Moon radially symmetrical like the Earth, and if so, is it differentiated? Specifically, does it have a core and does it have a crust?

(2) What is the geometric shape of the Moon? How does the shape depart from fluid equilibrium? Is there a fundamental difference in morphology and history between the sub-Earth and averted faces of the Moon?

(3) What is the present internal energy regime of the Moon? Specifically, what is the present heat flow at the lunar surface and what are the sources of this heat? Is the Moon seismically active

and is there active volcanism? Does the Moon have an internally produced magnetic field?

Composition, structure, and processes of the lunar surface

(1) What is the average composition of the rocks at the surface of the Moon and how does the composition vary from place to place? Are volcanic rocks present on the surface of the Moon?
(2) What are the principle processes responsible for the present relief of the lunar surface?
(3) What is the present tectonic pattern on the Moon and distribution of tectonic activity?
(4) What are the dominant processes of erosion, transport, and deposition of material on the lunar surface?
(5) What volatile substances are present on or near the surface of the Moon or in a transitory lunar atmosphere?
(6) Is there evidence for organic or proto-organic materials on or near the lunar surface? Are living organisms present beneath the surface?

History of the Moon

(1) What is the age of the Moon? What is the range of age of the stratigraphic units on the lunar surface and what is the age of the oldest exposed material? Is a primordial surface exposed?
(2) What is the history of dynamical interaction between the Earth and the Moon?
(3) What is the thermal history of the Moon? What has been the distribution of tectonic and possible volcanic activity in time?
(4) What has been the flux of solid objects striking the lunar surface in the past and how has it varied with time?
(5) What has been the flux of cosmic radiation and high-energy solar radiation over the history of the Moon?
(6) What past magnetic fields may be recorded in the rocks at the Moon's surface?

GEOLOGICAL EXPLORATION OF THE MOON

Major Objectives of Geological Exploration

A major objective of the geological exploration of the Moon is the development of perspective in viewing our own planet and the solar system in which it resides. We are engaged not only in the exploration of space but also in the exploration of time. Key to this perspective in time is the recognition of the stratigraphic sequence, the order in which deposits of the past were laid down. On Earth this task is firmly grounded with the landmarks in the sequence tied to isotopically determined ages of rocks. The difficulty with the terrestrial record, however, is that active mountain building, erosion, and sedimentation have destroyed any recognizable remnant of the primordial Earth. At present we know almost nothing concrete about the first billion years of the Earth's history.

The Moon is of especial interest in working out the history of the solar system for two reasons. First, the surface of the Moon may be one of the few places where a very early stratigraphic record is preserved and decipherable. Secondly, because of its proximity to the Earth, it will be possible to determine the geology of the Moon in far greater detail, for a given level of effort, than that of the next nearest terrestrial planet. The events recorded on the Moon are likely to be more closely correlated to events on the Earth than the events in the record of any of the other planets. Consequently, the history of the Moon may have the most relevance to terrestrial history.

The question of the origin of the Moon illuminates the relevance of lunar history to questions about the Earth. If the Moon was captured by the Earth, violent disturbances of both would have occurred during capture. The age of features on the Moon produced by these disturbances would provide the key to recognizing the effect of the disturbance on the Earth. If the Moon was formed by capture or coalescence of multiple satellites, there may have been a disturbance from a concomitant rain of fragmental debris on the Earth. If the Moon was formed by fission from the Earth, on the other hand, the present composition of the Moon provides direct evidence on the degree of differentiation of the Earth at the time of separation. Under this mechanism the possible presence of organic material on the Moon would provide clues to early evolutionary stages of life on the Earth. Finally, if the Moon was formed by independant condensation from a proto-Earth nebular mass, the present composition of the Moon should furnish important clues to the chemical differentiation mechanisms operating during formation of the Earth-Moon system.

Telescopic observation forms the basis of attempts to elucidate the stratographic history of the Moon. At present the sequence of deposits is known to a first order in the equatorial regions. For some of the lunar geologic units, the general mode of origin can be inferred. It is because the geological processes that change the surface of the Moon probably operate much more slowly than those on the Earth that part of the lunar stratigraphic sequence gives promise of providing a record of the early history of the solar system that we can never find on Earth.

The historical record preserved in the lunar strata bears upon several processes of the solar system. In contrasting the older with the younger deposits, we can ask what has been the distribution in time and in mass of solid bodies impacting the Moon. The history of cosmic radiation and changes in its intensity or character should be recorded in the nuclear changes in the upper part of stratigraphic units. Some of these units probably have been exposed through much of geologic time, others have been exposed for a brief fraction of lunar history. The problem of origin of planetary bodies would be greatly aided by examination of a primordial or very ancient surface of one of the bodies of the solar system, by studying its chemical and isotopic composition, or even by looking for the strength of early magnetic fields in the solar system through paleomagnetic measurements on early volcanic rocks.

Many basic problems of the Earth can be approached by comparison of the Earth with the Moon. We still do not understand the chemical evolution of the Earth's crust as complicated through reworking by surface waters. The Moon stands as an example uncomplicated in this way, possibly having evidence of protocontinents. It may be the best place to see what an early crust looks like. The processes of mountain building on Earth are only partially understood, in part because tectonically active areas are covered by oceans or thick sedimentary deposits. In the lunar environment, tectonic deformation of the surface of a planet can be examined without the camouflaging effect of erosion, sedimentation, or oceans. Similarly, volcanic products that on the Earth are contaminated by passing through the chemically reworked surface sediments, should, on the Moon, be free from such effects.

Geological Complexity of the Moon

In the light of present knowledge, the Moon is a heterogeneous body. Differences in color, albedo, polarizing, and thermal properties that are correlated with topographical differences form the basis for recognition and mapping of geologic units. Although most of the large craters on the Moon are probably of impact origin, some of the geological units exhibit topographic features closely resembling certain volcanic forms on Earth. The surfaces of the maria, for example, have clearly defined features that closely resemble terrestrial lava flows. Fields of small domes with summit craters, some of which are nearly identical in form to terrestrial volcanoes, occur irregularly over the lunar surface. These features are diverse in both kind and relative age, suggesting the Moon may have had a long and complex magmatic history.

Unlike the Earth, the Moon bears no evidence of folded mountain chains, but has a well-defined pattern of linear features that probably correspond to fractures and faults. This contrast in tectonic features should help to elucidate the mechanisms of formation of terrestrial mountain ranges when the internal structure and processes of the Moon are better understood.

Geological Exploration

The known diversity of layers of material with different physical characteristics and the observed complexity of structure of the lunar surface requires an extensive program of exploration if the broad relations of the stratigraphy and structure are to be solved. The stratigraphy and structure are solved primarily by the technique of regional geological mapping, supplemented by geophysical (principally seismic) exploration to obtain subsurface structure and by local drilling.

Geological mapping of the Moon can proceed most efficiently by systematic surveying with remote sensing instruments from lunar orbiting spacecraft, followed by local detailed studies on the lunar surface. Studies on the surface are needed at key localities where the contacts and structural relations between different mappable units are best exposed and at localities where features of special interest occur. The first step in the systematic surveying should be the preparation of topographic maps by photogrammetric methods from photographs taken from lunar orbit with cameras designed for mapping. Control for the mapping can be obtained from accurate determination of the orbit, the orientation of the spacecraft, and radar altimetry from the spacecraft to the lunar surface. Topographic maps at scales of 1:1,000,000, 1:250,000,000, and, locally, 1:100,000 will provide the base maps needed for most of the geological investigations. The distribution of different geological materials is also mapped from the photographs on the basis of differences in topography, albedo, and reflectivity and emissivity over a wide range of the electromagnetic spectrum. In addition to the photographs taken with the mapping cameras, the gamma radiation and thermal emission of surface and reflection or scattering characteristics from the ultraviolet to radar wavelengths should be measured from orbit.

Direct examination of the lunar surface should be carried out by landings at different localities and by extended traverses over the surface. This examination is needed to determine the detailed strucural relations between geologic units mapped from orbit, to obtain physical data that can only be acquired from instruments in contact with the surface, and to obtain samples for mineralogical, chemical, isotopic, and other analyses after return to Earth. Thoroughly trained observational scientists will be needed to carry out the more advanced stages of this work on the lunar surface.

Investigations on the lunar surface are needed on at least three different scales. The smallest features, ranging in size from near microscopic to hundreds of meters, the fine structure of the lunar surface, can be studied by men on foot during early Apollo landings. Very detailed investigations of features at this scale and the processes by which they are produced may require a small lunar base to sustain men over much longer periods of time than is available during the early Apollo landings. To study features ranging in size from one to many kilometers requires a vehicle to carry men over these distances from the landed spacecraft. This is the scale on which most of the contact relations of regional geologic units and mesoscale structures, such as relatively large craters, faults, folds, and possible igneous intrusions and volcanoes must be examined. Finally, surface traverses of ten to hundreds of kilometers in length are required to examine features of crustal and subplanetary dimensions, such as the basin and surrounding mountain ring of Mare Imbrium and other circular maria. These traverses are needed to obtain deep seismic reflection and refraction profiles correlated with surface gravity measurements and geology. Such traverses provide extensive opportunities to sample and study areal variations in the regional geologic units.

GEOPHYSICAL OBSERVATIONS ON THE MOON

Introduction

As explained in the section on geological exploration, the Moon is the second relatively large member of the solar system available to us for detailed study. In addition to satisfying an intrinsic interest in the constitution and history of the Moon itself, study of the Moon provides valuable insight into fundamental questions concerning the morphology of the Earth and the solar system. Study of the figure of the Moon, the distribution of matter within it, the heat flow from the interior, and the magnetic field increases our knowledge of the composition and history of the Earth-Moon system. Study of the present tectonic activity as evidenced by seismic activity, volcanism, measurable deformation (faulting), and anomalies in the gravity field, increases our understanding of similar processes on Earth. In the opening paragraph of this section is a list of fundamental questions for which we hope to obtain answers through exploration of the Moon. The following suggested geophysical observations, combined with geological and geochemical observations, are required for such exploration.

Geophysical Observations from an Orbiter

Magnetic field. Provided that earlier measurements of the magnetic field indicate an internally produced field of sufficient magnitude, this field should be mapped from an orbiter. Such a map will provide a reference for ground-based magnetic profiles obtained on traverses; for measurements of residual magnetization of lunar samples; and for studies of the origin of the main internal field. It is possible that the field induced by the solar wind will predominate. Observation of the time-dependence of this field will provide information on electrical conductivity at depth within the Moon.

Microwave temperature. Measurements of the radiation temperature, at wavelengths near 10 cm, can be used to map the steady-state near-surface temperature. This temperature will be strongly dependent upon surface thermal properties. By using more than one wavelength it may be possible to separate surface effects and outline regions of anomalously high heat flow.

Geodetic measurements. Observations of the motion of an orbiting spacecraft or special geodetic satellite, combined with measurements of the lunar librations, are needed to obtain accurate values for the principal moments of the Moon. Since the mass and the moment of inertia are fundamentally important constraints on the internal constitution of the Moon, such measurements are extremely important. The higher harmonics of the gravitational field, obtained from observations of an orbiter, contain information on the departure of the Moon from fluid equilibrium and place constraints on the symmetry of the density distribution within the Moon. When combined with radar distance measurements between the orbiter and the lunar surface, the observed gravitational field provides datum for topographic mapping and for studying the possible degree of isostatic balance between regions of high and low elevation.

Geophysical Observations from a Lander

Measurement of lunar motions. Measurement of lunar librations can be improved by radar observations from Earth of three widely separated (about 1000 km) corner reflectors. These measurements are needed for determination of the moment of inertia. In addition, such observations would help unravel the dynamical history of the Earth-Moon system.

A fixed and comparatively small optical telescope emplaced on the Moon allows measurement of the length of the lunar day and its slight variations. The absence of an atmosphere makes this measurement simpler and more precise than it is for the Earth. In the absence of the fluctuations of atmosphere and oceans the greater precision will be significant in relation to other dynamical effects. Tides raised on the Moon by the Earth and the Sun come into this category, as well as the librations. Any internal fluid motion would be expected to show an effect.

The instrument required is a telescope with a photoelectric detector behind a slit, aligned approximately along a lunar meridian. Telemetry with millisecond timing, as stellar images sweep across the slit, is all that is required.

Passive seismology. Assuming there are natural moonquakes with a temporal distribution of events and magnitudes roughly similar to that of Earth, passive seismology provides the most direct source of detailed information on the deep interior of the Moon. Implantation of at least three small remotely operating seismic observatories at widely separated (about 1000 km) locations is recommended. Such instruments should be capable of recording all frequencies between several cycles per second and tidal frequencies in three components of motion. With such a net most larger events could be located (latitude, longitude, depth, time) and active zones delineated. Once active regions are identified, additional (perhaps simple) instruments should be installed so that these sources can be used most efficiently to study different tectonic provinces; short-period instruments can be used to study smaller events within active areas, longer-period instruments can serve to study pure maria or highland paths from larger, distant events. All such instruments should be designed to operate as long as possible.

In the absence of natural moonquakes, impacts of large meteorites, large explosions, or impacting Lunar Excursion Modules (LEM) may provide sufficient energy for large-scale studies. If meteorite impacts can be differentiated from moonquakes, by focal depth or other criteria, the seismograph net can be used to monitor the distribution of larger meteorites in the vicinity of the Moon.

Active seismology. Seismic refraction and reflection techniques should be used to extend geological observations to depths beneath the surface. Refraction measurements using chemical explosives are probably most efficient for general reconnaissance since they give both average velocity and thickness of a layered structure. Under certain conditions, local reflection surveys might be useful in the study of small-scale variations. At least two scales of refraction surveys are recommended: short profiles, 1 to 10 km, using explosive charges of a few pounds or less, to determine shallow structure down to a few hundred meters; and long profiles, 10 to 100 km, using explosive charges up to several hundred pounds, to investigate the structure of the entire crust (if any) and upper mantle. An expended LEM, crashed into the lunar surface, should provide sufficient energy for the long profiles. For this purpose the location and time of crash should be known to 1 km and 0.1 sec or better.

Heat flow. Heat flow from the interior of the Moon provides essential information on the distribution of radioactive elements and the thermal history, including volcanism. It is probably necessary to use a hole 1 to 10 meters deep in order to obtain the heat flow. A considerable amount of work on theory and observational techniques is needed in this important area. Care should be taken to assure that such measurements are taken both at typical and at interesting places.

Gravity-magnetic. Essentially continuous measurement of the gravitational and magnetic fields should be obtained along all traverses for correlation with the geological and seismic data. Such observation can be highly automated. Gravity should be measured to about ±1 milligal and elevation to 3 or 4 meters, if possible. However, even cruder data would be useful for regional studies. It will probably be necessary to operate a magnetometer at a base station during magnetic traverses in order to remove the effects of temporal variations in the field.

Magneto-telluric. Information about the internal distribution of electrical conductivity may be obtained by combining the base station magnetic variations with variations in the horizontal electrical field.

GEOCHEMISTRY OF THE MOON

Introduction

Studies of the lunar surface in the visible, infrared, ultraviolet, and microwave portions of the electromagnetic spectrum show that there is much diversity in its reflective properties. Correlations between topographic features and color differences suggest that the Moon is chemically and mineralogically heterogeneous on the scale of present telescopic observations. Some of the topographic forms are suggestive of volcanic flows. There is, however, little agreement among observers about the mechanism that may be responsible for chemical differentiation.

In general, chemical differences produced on the lunar surface may be ascribed 1) to material or energy arriving from space and 2) to processes driven by energy released from the Moon's interior.

The extent to which either of these two kinds of processes predominate in determining both the chemistry and morphology of the lunar surface will be at least partially answered through studies of samples collected during the first manned lunar landings. Samples returned from these missions should also allow some characterization of the chemical nature of lunar differentiation processes. Establishing the relative importance of particular processes on the lunar surface is, clearly, of first-order importance. From a scientific point of view, however, this knowledge is only a part of a larger picture, i.e., the origin and history of the Moon. It is doubtful that answers to basic questions on the Moon's history, gross composition, and over-all degree of chemical differentiation can come from samples collected in two or three relatively small areas on the Moon, particularly if the chemical variations are large. Investigations of significant portions of the Moon's surface are probably necessary to obtain answers to these fundamental questions. An integral part of these investigations will be a more detailed study of the geochemical processes on, and properties of, the lunar surface, both to characterize the materials and to determine their place on an absolute time scale. It cannot be overemphasized that such studies must be coordinated with a larger program for investigating the structural stratigraphic and geomorphic features of the lunar surface, i.e., a geologic mapping program.

Specific Geochemical Problems on the Lunar Surface

1. Radioactive Isotopes and an Absolute Time Scale

The isotopes of uranium, Th^{230}, K^{40}, and Rb^{87}, and their stable daughter products are the basis of the most powerful methods for the determination of time on the 5-billion-year scale of the solar system. The study of these nuclides in lunar materials will be very important for estimating the time of formation of the Moon and the times during which melting and differentiation of silicate materials took place. If internally driven chemical differentiation processes are discovered, isotopic dating methods will tell us when and for how long in the Moon's history such processes were active.

It would be of great interest if we were to find portions of the lunar surface dating back to the times when the chemical differentiation recorded in meteorites took place.

2. Bulk Composition of the Moon

In order to understand the origin of the Moon, in particular, to see its relation to the Earth, the Sun, and other planets, it will be necessary to deduce the over-all composition of the Moon. If the surface of the Moon is chemically heterogeneous, only extensive sampling of the lunar surface will permit us to infer its over-all composition.

It is quite possible that a significant amount of material on the present lunar surface is foreign to the Moon in terms of its characteristic composition. The existence and identification of such foreign materials (probably similar to some meteorites) will also depend on detailed chemical and isotopic studies.

3. Lunar Magmatism

Should magmatic processes occur on the Moon on a large scale, an understanding of the chemistry and mineralogy of magmatically produced materials will be essential for understanding its geological history. It will be important to determine whether such processes have concentrated radioactive elements and volatile elements near the surface as they have on the Earth.

4. Lunar Degassing

The evolution of the Earth's atmosphere and oceans is a fundamental part of terrestrial history. These features are the consequences of chemical potential gradients that tend to drive volatile components like CO_2, H_2O, Cl_2, and H_2S from hot high-pressure interior regions to cool low-pressure surfaces. The mechanism by which these gases are transported in the Earth is not entirely understood, but it is probably associated with volcanism. It has been suggested that even in the absence of volcanic processes there will be transfer of volatiles from the interior to the surface of the Moon. Detailed studies of the gases absorbed in subsurface layers and of the transient lunar atmosphere will be necessary to delimit the nature of lunar degassing processes.

5. Cosmic Ray and Solar Wind History

In the absence of a lunar atmosphere the surface materials are constantly bombarded by energetic cosmic rays and the solar wind. The nuclear reactions resulting from cosmic-ray bombardment have been studied extensively in various meteorites. These studies suggest many important applications to the lunar surface, e.g., rates of turnover and cosmic-ray flux may both be determined. The krypton and xenon content of surface material may also be an indication of the solar wind flux averaged over the exposure time of the surface materials. Older materials that have been buried and shielded from cosmic-ray and solar wind bombardment may furnish a way by which the magnitude of these quantities in the past may be investigated. Such studies can be made only after the geological history of a particular region is fairly well understood.

The problems outlined above represent the major questions that appear interesting with our present knowledge of the lunar surface. The discovery of processes that are not now anticipated may easily lengthen the list of chemical problems on the lunar surface that will challenge our ingenuity and intellect.

Conduct of Chemical Investigations

With the sample return capabilities that are planned in both the early Apollo and post-Apollo missions, it is clear that enough material can be obtained on a scale to represent quite well those that may have been mapped or investigated during a mission. If sample consumption is optimized, all the obvious chemical and mineralogic analyses can be done on samples of several hundred grams. Hence, there is relatively little need for performing analyses on the Moon, particularly if they compete for time that could be used for observing and examining surface features. A number of cooperating analytical facilities would be required on Earth, including several laboratories equipped for modern mineralogical analyses, geochronometry, and chemical analyses for major and minor elements. The Earth-based geochemical program should be well coordinated with all other aspects of the lunar exploration program.

The success of the geochemical program is very much dependent on the skill with which materials are collected and returned to Earth. The observational skill of the scientist-astronaut on the lunar surface will be the most important factor in obtaining significant materials from the lunar surface. It seems clear that geological field experience would most closely approximate the type of situation that may be encountered on the lunar surface. Training and selection of geologist-astronauts thus would be an important part of the preparation for an extensive program of lunar exploration.

Other preparations may include the development of special tools to aid in sample collection on the surface, e.g., boring devices, sample containers, etc., and devices for rapidly differentiating visually similar materials, e.g., gamma-ray survey instruments, microscopes for examining thin and polished sections.

Studies of the lunar atmosphere and degassing products will almost certainly require some type of extremely sensitive gas analyzer on the lunar surface. Because of the difficulty of returning samples that fully preserve the lunar conditions, such an instrument should be capable of measuring a wide range of atomic masses or molecular weights (mass 12 to mass 200) and be capable of measuring extremely low partial pressures at least down to 10^{-14} atmospheres. In preparation for detailed analyses of lunar gases a simple device measuring total gas pressure may be useful on earlier missions.

Lunar orbiters present an excellent opportunity to map the chemical differentiation of the lunar surface, by surveying the gamma-ray activity of the surface: such a survey should clearly identify regions where potassium, uranium, and thorium are concentrated relative to other areas. By analogy with the terrestrial situation, such a survey should clearly distinguish differentiated regions from nondifferentiated regions.

It is not now clear whether the possibility of back-contamination of the Earth by pathogenic organisms from the lunar surface will be a serious problem in the long-range exploration of the lunar surface. If the question is not answered by early Apollo missions, the precautions that may be required by this hazard should be integrated into the analytical program in a way that will result in a minimum of degradation of the materials and methods used to analyze the lunar materials.

PHYSICAL PROCESSES ON THE LUNAR SURFACE

Geological investigation of the Moon and understanding of the surface features and their time scale will require, first of all, some understanding of various externally induced processes that may operate there. Erosion, surface transportation, impact, evaporation, and recondensation will need to be understood in addition to the externally induced changes in the appearance and physical structure of the surface material.

Erosion and Transportation

There is evidence that erosion and surface transportation of material has occurred on the Moon and it will be of great importance to understand the nature of that process. It may be simply due to the accumulated effect of meteorites, including micrometeorites, which would tend to redistribute material and cause creep. It may be that impact, evaporation, and subsequent recondensation are major factors. It may also be that other effects, such as electrostatic forces, play a part in the erosion and transportation mechanism. The detailed appearance of the surface may, of course, give an indication to help resolve these questions. Physical measurements and some simple observations carried out on the Moon may, however, be needed for better understanding.

The rate of micrometeorite bombardment and of secondary bombardment on the surface should be measured. From this, the direct rate of surface transportation and the time to produce a given degradation of a crater could be estimated. The shapes and size distribution of very small craters in the range of centimeters to millimeters will be helpful here, and if such craters can be seen, good photography should bring back the information.

The precise manner of deposition of the material will determine the way in which it reflects and polarizes light. Since returned samples are unlikely to have preserved the optical surface characteristics, it will be necessary to measure, in place, the angular scattering law and the polarization law at optical wavelengths. Variations from place to place in these optical properties, and especially between fresh craters and other surfaces, should be observed. Since these properties are well known for the Moon as a whole, a study in one locality will also serve to establish whether the particular site is a representative one.

If electrostatic effects are significant in the transportation of small particles, perhaps after they have been loosened by micrometeorites, such particles would probably accumulate to form distinctive structures of small scale that could not be ascribed simply to meteoritic action. Any such shapes should, of course, be carefully photographed for subsequent study. They might appear as small-scale waviness of the surface or any other near-periodic irregularities. They might also involve preferential attachments of small particles to points or edges of other material.

For understanding such electrostatic processes, measurement of the daytime electric field above the surface on a scale of millimeters is essential. The photoelectric effect is expected to produce an electric field above the surface in the first few centimeters and of the order of a few volts. Observation of a low-energy electron beam at various heights close to the surface would be one way of studying such electric fields. Perhaps measurements of electrostatic force are another possibility.

The possibility of chemical differentiation being produced or maintained during erosion processes should be studied.

A simple observation of how a handful of dust picked up from the surface and allowed to fall back to it will distribute itself is also most important. If the particles do not generally appear to fall on straight ballistic orbits back to the ground, their motion should be photographed and that should be done both in the sunlight and in a locally shaded region or the lunar night.

Thermal Properties

The thermal properties of the surface material are also critically dependent on its detailed fine structure and therefore not likely to be preserved in a sample returned to Earth. An understanding and the detailed interpretation of thermally oriented maps will require a study of the regional relations and correlations with other observable features. Again, this will also serve to establish the degree to which the landing site is representative of other areas. The thermal property best measured would be the rate of cooling, as observed at infrared wavelengths, in a region shielded from sunlight.

Proton Bombardment

The intensity of the solar wind proton bombardment and alpha particle bombardment of the Moon's surface is not necessarily the same as that which would be deduced from interplanetary measurements. It will, therefore, be necessary to make a direct determination of the bombardment rate on the Moon's surface. The study and precise photography of regions where the local topography prevents most or all of this proton bombardment from reaching the ground will be of particular value in defining the nature of the effects produced.

Gases and Volatiles

The percolation of gases and volatile substances through the lunar surface is a matter of great interest. First, such substances may contain important information concerning the interior of the Moon and its mode of derivation. Second, they may have affected and helped to shape the present surface. Some knowledge of these processes is necessary for understanding the geology.

The returned samples may contain condensates of some substances that have come from the Moon's interior and have frozen out at a temperature near that of the subsurface. Many other substances may have condensed at lower levels where the temperature is higher, and only traces of them may reach the surface and may not be discernible in an analysis of the sample; substances that do not condense, even at the subsurface temperature can, of course, not be found in the returned samples except possibly in very small amounts on surface nightime samples that have reached an extremely low temperature. Analysis of returned samples is thus not likely to yield the required information concerning such exhalation of gas. An investigation and instrumentation on the surface of the Moon will be needed to tackle the problem.

There are likely to be very great regional variations in the outflow of gases and possibly some of the known features are related to these processes. The dark-haloed craters have been mentioned in this connection. Since the rates of flow are likely to be very small and detection therefore difficult, one would be tempted to consider first locations that look particularly promising in this respect.

Instrumentation will have to be devised that is suitable for the detection of water vapor, carbon dioxide, nitrogen, argon, methane, ammonia, helium, and perhaps several other gases. Cryogenic capture and return will be suitable for some of these, ion pumping and capture or direct mass spectrography may also be appropriate. Optical spectrographic methods, such as observation of the temporary lunar atmosphere in grazing sunlight, may also be useful, especially in connection with the possibility of water vapor and resulting OH.

4. VENUS

UNCERTAINTIES AND SCIENTIFIC QUESTIONS

Our understanding of Venus is at present limited by the complete cloud deck that obscures the underlying surface.

While some physical measurements can be made, it is remarkable how little certain knowledge we have because of complexities and ambiguities in the interpretation of observations. Although it is our sister planet our knowledge is extraordinarily slight and paradoxes abound. A brief review will illustrate these statements.

Even the pole and rate of rotation of Venus are not certainly determined. The obliquity of the orbit is probably not great and radar observations suggest a 200-day retrograde rotation.

The cloud itself may well be of ice-crystals, as Strong's infrared measurements indicate, but the complete coverage then suggests an absence of downward motion of the atmosphere, which is, of course, impossible. The upper 10 to 30 km seems to be extremely finely dispersed and is in a state of violent and rapid motion.

The presence of a fine, scattering cloud makes the interpretation of certain measurements very difficult. Thermal emission (of which excellent maps can be made) may involve both scattering and absorption, and cannot be related to temperature with certainty. Spectral absorption lines of water vapor and carbon dioxide show the presence of these gases and indicate temperatures from 200°K upwards and pressures of the order of a few atmospheres, but we are unable to say where the spectral lines are formed, and therefore to which level the measurements refer.

The thermal field poses some difficult problems. Why does the planet appear to be hottest at the antisolar point? Some combined measurements suggest a deep isothermal layer in the upper parts of the cloud. How can active dynamical processes not destroy such an isothermal region?

Finally, radio emission measurements at wavelengths from 3 mm to 40 cm suggest the possibility of a surface whose temperature is near 600°K, while the infrared emission suggests that the clouds are closer to 250°K. These measurements in particular have unleashed a train of speculation, but we have no really satisfactory explanations for a temperature of 600°K, nor has a good nonthermal alternative been proposed. Recent studies suggest the possibility of a deep adiabatic layer generated by an atmospheric circulation heated along its upper surface. The model further suggests that upwelling currents may indeed cover all but a small fraction of the planet's surface, but if this should be so we are dealing with an atmosphere utterly unlike the Earth's.

About the surface and the interior of Venus there is no information. We do not even know whether the surface is solid or liquid or neither one nor the other, like Earth. Our ignorance makes it impossible to point to specifically interesting features, such as can be done on the Moon and Mars, but nevertheless we may expect that exploration of Venusian geology and geophysics will be as stimulating and valuable as that of any object in the solar system.

THE IMPORTANCE OF INVESTIGATING VENUS

The most striking challenges presented by Venus lie in the totally different character of the atmosphere as compared with Mars and Earth; our lack of understanding of the atmosphere, surface, or interior of the planet; and the apparent paradoxes suggested by our present knowledge.

It can be argued that Venus is almost as likely an abode of extraterrestrial life as Mars, and the motivations behind the biological exploration of Mars therefore apply equally strongly to Venus. Let us briefly examine this point, for there is a possibility that Venus has been too hastily rejected as a biological objective in earlier discussions.

On the positive side, the pressure and temperature near the cloud top, and the presence of large quantities of carbon dioxide and moderate quantities of water vapor all favor Venus strongly. The main objection has been to the supposedly high surface temperature, but we should note:

(a) That the interpretation of the radio-emission is, at least, questionable. Few planetary physicists would be surprised to hear that a nonthermal source exists.

(b) The thermal maps offer evidence of large topographic features. High mountain ranges might exist with summit temperatures far lower than the general basal temperatures.

(c) Is it not conceivable that a form of life could develop in suspension in a very dense atmosphere?

Even if we concede that Venus should be investigated as an abode of life, the priority accorded to Mars is not called into question. Because we can see directly that Mars has a solid surface, and because we know the temperature and pressure (approximately) at the surface, engineering design of landers can proceed in a way that is not possible for Venus. Nevertheless, we should proceed with a systematic program for studying Venus.

We therefore strongly urge that NASA support a program of exploration of the planet Venus over the next ten years on a level comparable to, but perhaps slightly below, that proposed for Mars.

The emphasis of this program should be on systematic investigation of the surface, the cloud layer and the possible subcloud region, and particularly on those observations that require the least effort in new developments, or have an unusually effective exploratory character. Here also, much information can be obtained from telescopes on the ground, in balloons and rockets, and in near-Earth orbit.

A TENTATIVE PROGRAM

An essential aspect of the Venus program should be flexibility; a hard-and-fast program should be avoided. However, in order to indicate the way in which the investigation may develop we draw attention to the following areas:

Theoretical

It is difficult to show that any major area of theoretical investigation into Venus has been exhausted and some have hardly been touched. Examples are: possible planetary atmospheric flows; interpretation of the twilight zone in terms of atmospheric structure; line formation in the cloud deck; models of the cloud layer and its relation to the planetary dynamics; theoretical structure of the upper atmosphere. In view of the relative ease with which theoretical studies can be undertaken and their vital significance in all stages of planetary exploration, their neglect is difficult to understand.

Ground-based

It is important to bear in mind that astrophysicists have not in the past devoted much effort to planetary studies. There are investigations that would add greatly to our knowledge and might re-orientate our planning of some space missions, but they are not being undertaken. And there is every reason to suppose that, even without the proposed space missions, rapid advances in planetary studies could take place by conventional means.

Optical. Investigation of the Stokes parameters in the twilight zone with high spatial resolution; relationship of such observations to the fine thermal radiation structure. High-resolution spectroscopy of carbon dioxide at many points of the disk, as a function of time.

Radar. Detection of surface features. Determination of the depth of the atmosphere. Better determination of rotation rate.

Radio emission. Improvement of photometric accuracy and spatial resolution at all accessible radio wavelengths, and particularly in water vapor absorption lines.

Balloons, Rockets, and Earth-Orbiting Observatories

A hierarchy of experiments is contemplated with increasing spectral and spatial resolution and decreasing interference from terrestrial absorption. Large infrared spectrometers could be carried by balloons for general spectral exploration and the thermal emission region near 10 μ should be investigated at highest spectral resolution. The ultraviolet spectrum could yield valuable information about the nature of the cloud layer and of oxygen and ozone concentrations. For this the stabilized rocket platform is desirable. Infrared spectroscopy could also be performed from rockets with adequate stabilization. An absolute accuracy of 10 sec with $\pm$ 2 sec random motion would provide a useful capability for general planetary work on Mars, Venus, and Jupiter and to a lesser extent on other planets. A very similar requirement has been put forward by the Working Group on Optical Astronomy.

Much of the balloon and rocket work can perhaps be superseded ultimately by an Orbiting Astronomical Observatory with flexible instrumentation assigned to planetary observations. Off-set guiding would be a desirable facility. This Orbiting Astronomical Observatory, Planetary (OAOP) should be devoted to spectrographic investigations at all wavelength ranges with some resolution of the disk. Really high spatial resolution would be possible only with a larger telescope orbiting at several Earth's radii to avoid perturbations from the Earth's gravitational field. Such a concept (the OPO, or Orbiting Planetary Observatory) probably requires a man in space and expenditures that would give a lack of balance to the proposed program. Thus, we do not recommend the OPO at this stage, but see great value in the simpler OAOP.

Space Probes

Geodetic measurements. The maximum geodetic data should be obtained from satellite systems since any more direct exploration of the planet is far in the future.

Surface profile. A radio altimeter would give invaluable data on the thickness of the clouds, the existence of mountains and the possibility of oceans. Spectral reflectivity measurements may lead to information on the nature of the surface. While such data are partially obtainable from Earth-based radar, detailed surface resolution requires a Venus orbiter.

Cloud structure. High-resolution scans with optical and thermal radiations are invaluable for exploratory studies. Thermal maps or cloud photographs, as have been obtained from terrestrial satellites, would be extremely exciting and would undoubtedly lead to the formulation of many pertinent questions.

Upper atmosphere. Experiments on the upper atmospheres of the planets are discussed elsewhere in this report (cf., section 9, p. 105) . Such experiments (and we may add magnetic field measurements) are generally relatively easy to perform from orbit and do not require transmission of large amounts of data. They all add to the general understanding of the atmosphere of Venus and should be given a good priority.

Drop sondes. These are highly desirable in an early stage of Venus exploration. The aim should be to determine pressure and temperature as a function of height. Since programs of biological exploration of the solar system might be modified if a nonthermal source should be responsible for the high temperatures now estimated from radio emission measurements, the earliest possible use of a simple drop sonde is strongly urged. The payload required does not require the use of Saturn launchers, and there is a strong case for attempting this particular measurement with an existing rocket. Some aspects of the chemical composition should not be difficult to determine, particularly water vapor, and consideration should be given to the use of a simple mass spectrometer. From such data we may hope to infer the nature of the clouds; it is doubtful whether a more direct method could be designed for this purpose.

The intensity of solar radiation and re-radiated infrared radiation could also be measured with a drop sonde. This would provide data for dynamical studies and give a direct test of the "greenhouse theory," if the latter should survive the direct temperature measurements.

Finally, the first drop sonde should carry some kind of penetrometer, designed to distinguish between solid and liquid surfaces. A seismometer would also yield valuable geophysical data.

We note that it would be valuable to repeat almost all the investigations outlined above on planets other than Venus. It is to be hoped that such explorations will be coordinated and that instruments and procedures will not necessarily have to be proved again for each mission.

Recommendations

We recommend that a program be undertaken, integrating theoretical investigations, ground, balloon, and rocketborne and Earth orbiting instruments with space probes in a unified, systematic approach to the exploration of Venus. This program should involve:

(a) Approximately five missions to Venus during the next 10 years. For most of the contemplated experiments orbiters are preferred to fly-bys, and for some they are necessary. Two missions should include the capacity to drop a small sonde onto the surface. One of these two missions should be given very high priority because of its relevance to the biological exploration program. An individual mission with an existing launcher may be justifiable. No space-probe experiments of the kinds that we have discussed require the use of very large payloads.

(b) Priority access to at least one 100-in. ground telescope with versatile spectrographic equipment.

(c) Immediate development of a fine-pointing control capable of stabilizing an entire rocket to a precision of 10 sec of arc with a random motion of not more than ± 2 sec of arc.

(d) Extension of the national balloon astronomy program to the maximum extent that it can contribute to planetary exploration.

(e) Extension and improvement of theoretical programs of planetary research within NASA and in universities and research institutions. Better correlation and integration of theoretical investigations with flight and observational programs.

(f) Orbiting a flexibly instrumented Orbiting Astronomical Observatory, Planetary for general planetary investigations.

Equipment and techniques developed for (a) should, where possible, be related closely to those required for other planets. The need for optical telescopes expressed in (b) should also be related to a picture which includes exploration of all the planets; a larger requirement would certainly emerge which should be planned as a whole.

Item (c) should be related to the needs of optical astronomy. With respect to item (d) the suitability of the ballooning facility of the National Center for Atmospheric Research as an integral and essential part of the space program on a long-term basis should be critically assessed.

The importance of (e) cannot be overestimated. Unfortunately the recommendation is particularly difficult to carry out, but this should not deter a continuing effort to increase the theoretical effort in the field of planetary exploration.

5. THE OUTER PLANETS

INTRODUCTION

The outer planets—those beyond Mars—are Jupiter, Saturn, Uranus, Neptune, and Pluto. The first four of these are the so-called major planets, which are characterized by large masses and small mean densities. The fifth outer planet, Pluto, is small, and has a mean density comparable with that of the Earth, Mars, Venus, and Mercury, the so-called inner planets.

The salient features of the outer planets are outlined below, and numerical values of some of their properties are given in Table 1.

Table 2. Some Properties of the Outer Planets (CGS units throughout)

Table 2. Some Properties of the Outer Planets (CGS units throughout)

	Jupiter	Saturn	Uranus	Neptune	Pluto
Radius R	6.98×10^{9}	5.82×10^{9}	2.38×10^{9}	2.24×10^{9}	3×10^{8}
Uncertainty mass M relative to solar mass	1.90×10^{30} 0.003%	5.58×10^{29} 0.008%	8.67×10^{27} 0.03%	1.05×10^{28} 0.4%	5×10^{27} factor of 2
Moment of inertia	2.31×10^{49}	4.23×10^{48}	1.62×10^{47}	1.53×10^{48}	$<3 \times 10^{44}$
I/MR^2	0.25	0.22	0.23	0.29	?
Mean density	1.33	0.684	0.60	2.25	?
Inclination of rotation axis	3°05'	26°44'	97°55'	28°48'	?
Angular rotation speed Ω	1.77×10^{-4}	1.71×10^{-4}	1.66×10^{-4}	1.16×10^{-4}	1.14×10^{-5}
Angular momentum $I\Omega$	4.10×10^{45}	7.2×10^{44}	2.69×10^{43}	1.77×10^{43}	$<x\ 10^{39}$
Surface gravity	2000	1120	940	150	?
Velocity of escape	6.1×10^{6}	3.7×10^{6}	2.2×10^{6}	2.5×10^{6}	?
Magnetic field near planetary surface	50 gauss	?	?	?	?
Rotational energy $I\Omega^2/2$	4×10^{41}	6×10^{40}	2×10^{39}	1.1×10^{39}	2×10^{34}
Gravitational energy GM^2/R	-3×10^{43}	-4×10^{42}	-2×10^{41}	-3×10^{41}	5×10^{39}?
Magnetic energy	10^{33}?	?	?	?	?
Total insolation	1×10^{42}	2×10^{41}	9×10^{39}	3×10^{39}	3×10^{37}
Heat capacity	5×10^{38}	1×10^{38}	2×10^{37}	3×10^{37}	1×10^{36}
Radioactive energy	10^{39}?	10^{38}?	10^{38}?	10^{39}?	10^{38}?

Jupiter. This object contains two-thirds of the mass of the planetary system and most of the angular momentum. Owing to its high surface gravity, it may have lost very little of its original material by evaporation; if this is so it is good sample of the primordial material of the planetary system. It is the nearest of the major planets and, owing to its large angular size, Jupiter has long been the object of fruitful ground-based studies. It is a very active planet; its atmosphere is in continual motion and exhibits a strong equatorial current; it possesses a magnetic field of internal origin and is surrounded by Van Allen radiation belts; it is a strong emitter of nonthermal radiowaves. Jupiter also possesses one of the solar system is curiosities—the Great Red Spot—an apparently unique feature. Finally, the planet is surrounded by the richest collection of satellites in the solar system.

Of the many important reasons for wanting to study Jupiter, one of the foremost arises from the fact that in the investigation of planetary systems of other stars Jupiter-like objects are likely to be encountered first.

As a result of what we already know about Jupiter, many of the important scientific questions one would now ask about the planet (see Scientific Questions listed below) are what might be termed "first-order questions," relating directly to processes taking place within the planet, or in its near vicinity (are, for example, the particles in the radiation belts energized by the solar wind?) The case of Jupiter contrasts sharply with that of Saturn, Uranus, Neptune, and Pluto, scientific questions about which are still largely of "zeroth order" (e.g., does Saturn possess radiation belts?)

Saturn is the only planet known to possess rings. It also appears to have a strong equatorial current, which may be more rapid than the corresponding current on Jupiter.

Uranus is unusual in that its rotation axis is almost in the plane of the ecliptic.

Neptune has a large satellite, Triton, with a retrograde orbit.

Uranus and Neptune have mean densities greater than those of Jupiter and Saturn (though much less than those of the terrestrial planets). Unlike Jupiter and Saturn, hydrogen may not be their main constituent.

Pluto has an anomalously large orbital eccentricity and inclination, and, as already noted, a mean density closer to those of the terrestrial planets than to those of the major planets.

It is possible to formulate a list of important and, in some cases, crucial scientific questions about the outer planets; such a list is given below. It is strongly recommended that the final strategy for investigating the outer planets should be designed with at least all of these questions in mind.

On the basis of these scientific questions it is possible to formulate a number of scientific investigations; these are discussed below under Proposed Investigations.

Brief reviews and references to some of the considerations that underlie these discussions and recommendations are given in the sections on chemical composition, thermal history, magnetic fields, the solar wind, atmospheric circulation, satellites, and strategy for the study of the outer planets (pp. 40 to 51).

SCIENTIFIC QUESTIONS

Composition, Structure, Chemistry, and Thermal History

1. Is the composition of any substantial part of Jupiter the same as that of the Sun?
2. Is Jupiter chemically differentiated vertically or horizontally; are there physical interfaces within the planet; is there a solid interface underlying the atmosphere? Is metallic hydrogen anywhere present within Jupiter?
3. Are the clouds on Jupiter composed of ammonia crystals, and are the color variations of the surface of the planet reflections of temperature variations? Are the colors due to free radicals or to the presence of small traces of metals?
4. Is gravitational or nuclear energy being released in substantial amounts within Jupiter? Is Jupiter a star?
5. Is there life or protobiotic material on any of the major planets?
6. Is the composition of Saturn, Uranus, or Neptune nonsolar and is Pluto chemically similar to the inner planets?

Dynamics of Atmosphere and Interior

7. Is the average temperature gradient in Jupiter's atmosphere sub-adiabatic or superadiabatic, and is there substantial mass transport within Jupiter's interior?
8. Is there a Jovian or Saturnian cycle; is Saturn's equatorial current stronger than Jupiter's and, if so, is it because Saturn has a deeper atmosphere than Jupiter; is the Spinrad effect real; is the Murray effect real; is the long life time of the South Tropical Disturbance (and similar phenomena) a direct consequence of the highly supersonic rotation speed of planet?

Magnetic Fields and Radiation Belts

9. Is the magnetic field of Jupiter due to a homogenous dynamo in the lower atmosphere or to a fluid core? Is 50 gauss the approximate field strength near the surface; is the field outside the planet that of an eccentric dipole nearly parallel to the rotation axis, and does the internal field undergo substantial variations in one rotation period of the planet; is there a strong toroidal magnetic field within Jupiter?
10. Are the radiation belt particles on Jupiter energized by interactions with the solar wind?

Curious Features

11. Is the temperature, composition, magnetic field, or any other property of Jupiter's Red Spot anomalous; is there a correlation of variations in the Red Spot period with variations in the radio period, and are these variations manifestations of hydromagnetic torsional oscillations of Jupiter's interior?

12. Is the Red Spot due to a Taylor column type of mechanism?
13. Are the rings of Saturn the remnants of a broken up asteroid?
14. Are features such as Saturn's rings, Jupiter's Red Spot, Jupiter's decameter radio bursts unique?

Satellites

15. Did the massive satellites have the same origin as the light ones?
16. Can the effect of Io and Ganymede on Jovian radio emission be interpreted in terms of their tidal action or is it necessary to ascribe magnetic fields to these satellites?
17. What processes may have been responsible for the anomalies in the orbits of Pluto and Triton?

Environment

18. Does the solar wind reach beyond Jupiter and is Jupiter's magnetopause about 7×10^6 km from the visible planet; does the solar-cycle impress its influence on processes taking place on any of the outer planets?

General

19. What is the radius of Pluto; do Saturn, Uranus, Neptune, Pluto, or any of their satellites possess magnetic fields or radiation belts; do they emit nonthermal radiation; what is responsible for the anomalous direction of Uranus' rotation axis?
20. Were the major planets formed by accretion?

PROPOSED INVESTIGATIONS

In the present section we list investigations that should be undertaken in the study of the major planets. They are grouped into seven areas corresponding to certain key questions.

Chemical Composition

Key Question. Are Jupiter and Saturn composed of a representative sample of the solar nebula, and are Uranus and Neptune depleted in hydrogen?

General Considerations. Present knowledge is direct only concerning the atmospheres above the clouds. This knowledge should be refined, and attempts made to ascertain the effectiveness of vertical mixing in order to permit extrapolation inward. Gravitational and materials data should be assembled to permit refined interior models.

Specific Investigations

i) High-resolution spectroscopy using large telescopes.
ii) Extension of spectroscopy into infrared and ultraviolet using balloons, rockets, and Earth satellites.
iii) Radio studies between 0.1 and 10 cm wavelength (sensitive to NH_3).
iv) Consideration of ortho-para H_2 as possible temperature trace in a convective atmosphere.
v) Infrared spectroscopy using satellites as source.
vi) Occultations to obtain scale height.
vii) Laboratory studies of equations of state and phase transitions in H_2, He, CH_4, NH_3, H_2O near 0°K, with particular emphasis on metallic hydrogen; also studies at elevated temperatures.
viii) Refinement of theoretical equations of state, with some emphasis on effects of temperature.
ix) Refined interior models with emphasis on effects of temperature.
x) Improved determination of the orbits of inner satellites for gravitational moments and tidal accelerations.
xi) Special attention to satellites of Uranus during orbit crossing of 1966.
xii) Possible geodetic (gravimetric) orbiters to refine gravitational data.
xiii) Refined determination of radii.
xiv) Atmospheric floats for measuring thermodynamic variables in deep atmosphere.
xv) Active acoustic experiments using floats, to detect interfaces.

Thermal Regime

Key Question. Are the planets hot inside because of the release of gravitational, rotational, chemical, or nuclear energy?

General Considerations. Internal heat would be significant in three ways. It would relate to the mode of formation of the planet; it could be the driving source for several of the active phenomena (magnetic field, atmospheric turbulence, etc.); and its proper evaluation may be necessary to the construction of interior models from which firm conclusions regarding chemical composition may be drawn.

Specific Investigations

i) Ground-based studies of infrared in the 8 - 13 μ and other spectral windows using large telescopes and resolution of the disk.
ii) Extension of infrared measures up to 100 μ using balloons, rockets, or an Orbiting Astronomical Observatory.
iii) Detection of infrared from back side using fly-by.
iv) Study of the effects of shadows (Wildey-Murray effect).
v) Evaluation of atmospheric motions for signs of internally driven convection (photography).
vi) Verification of the deep atmosphere that is required by recent interior models (Peebles, 1964).

Magnetic Field

Key Question. Is there indeed an eccentric magnetic dipole on Jupiter, and do the other planets have magnetic fields?

General Considerations. The extraordinary magnetic field structure indicated by the radio data demands explanation, perhaps in terms of a homogeneous dynamo. Its great strength, its eccentric position, and its variable rotation period suggest that clues to the dynamical processes involved in generating such a field may be forthcoming in an extensive study in situ, which is not possible on the Sun, where comparable dynamical phenomena are evident.

Specific Investigations

i) Continue and extend ground-based radio studies. Search for circular polarization at high frequencies.
ii) Search for extension of low-frequency spectrum below 5 MHz using locations of low critical frequency and/or rockets.
iii) Detect Jovian magnetic field with reconnaissance fly-by. Observe boundary of magnetosphere, orientation of magnetic equator. Same for other major planets.
iv) Advanced magnetometer in planetary orbiter to observe detailed structure and both short and long period changes.
v) Investigate magnetic field of Red Spot or other surface features.
vi) Check magnetic fields of satellites, particularly those correlated with decameter emission.

Atmospheric Dynamics

Key Question. Are the extraordinary surface features of Jupiter and Saturn (Red Spot and equatorial accelerations, for example) due to the effects of high rate of rotation on the atmosphere?

General Considerations. No completely satisfactory interpretation of the Red Spot exists. Hide has suggested that its presence is connected with the enormous Coriolis forces present. If observations can be made to provide a definitive interpretation, we are sure to learn something about hydrodynamics and possibly about the conditions in Jupiter's deeper layers. In any case, the Red Spot is an indicator of unusual atmospheric circulations.

Specific Investigations

i) Refined high-resolution photography and multicolor photometry from the ground.
ii) Obtain optical and ultraviolet polarization measurements with resolution of the disk, to define cloud structure.
iii) Time-lapse photography with high resolution from an orbiter to obtain cloud structure, motions, evidence for vertical convection, shear turbulence etc.

iv) Laser reflections from low orbiters to obtain the vertical distribution of reflecting material.

v) Concentrated optical, infrared, ultraviolet, radio study of the Red Spot and other anomalies using orbiters.

Radiation Belts

Key Question. Do all of the major planets with magnetic fields have radiation belts, and are these belts energized by the solar wind?

General Considerations. There is strong evidence from the radio emission that Jupiter possesses such belts, while they appear to be much weaker, if present at all, in Saturn. This could be because Saturn has no magnetic field, because the solar wind does not reach it, or because the rings sweep the belts free of particles. A comparative study is indicated. Moreover, a comparison of Jupiter's belts with the Earth's may elucidate the little-understood acceleration processes that are common to many astronomical systems.

Specific Investigations.

i) Continued study of nonthermal radio emission.

ii) Search for precipitated particles (aurora) with narrow-band filters from the ground.

iii) Simple particle detectors on a fly-by to measure changes in intensity with radial distance. Synchrotron theory can be checked by combined measurements of particle energies and magnetic field.

iv) Detailed study of particle energy spectrum, pitch-angle distribution, and time variations using orbiter.

v) Search for interaction of satellites with particle fluxes. Correlate with radiometers both in orbit and on Earth.

Satellites

Key Question. Did any of the satellites originate by capture?

General Considerations. Several lines of thinking suggest that there may at one time have been many Moon-sized objects in the solar system, not necessarily satellites of other planets. Some of these might have been captured, including the Moon, and the massive satellites of the major planets. Furthermore, the orbits of the outer satellites suggest that they may have been captured from a population of nonsatellites such as the asteroids, still found in the solar system. It will be of great interest to test these ideas.

Specific Investigations.

i) Photography to ascertain possible nonhydrostatic shape of J V and other small satellites, and general appearance of major satellites, from orbiter, bearing in mind possibility of exposed primitive surfaces, and possibility of evaluating collision rates near Jupiter.

ii) Magnetic fields from eccentric orbiter.
iii) Masses of satellites from refined astrometry, and possibly orbiter.
iv) Particles in Saturn's rings and very possibly in equatorial regions of other planets, as results of possible collisions.
v) Solution of many-body capture problem (mathematical).
vi) Isotope determination on J V using satellite lander.

Photochemistry

Key Question. Are interesting substances such as free radicals or prebiotic compounds being formed in the upper atmospheres of the major planets?

General Consideration. Both ultraviolet and hydrogen-rich atmospheres are present as required in some theories of the origin of life. Jupiter's colors may be due to chemical processes occurring under these conditions.

Specific Investigations.

i) Study aurorae from Earth.
ii) Topside sounder to obtain electron density from orbiter.
iii) Luminescence of dark side from fly-by.
iv) Simulated studies in laboratory.

SELECTED TOPICS

Chemical composition and internal structure of the outer planets

When it is realized that there is no complete agreement among geophysicists as to the more detailed chemical composition of the Earth, whose internal structure is known, the subject of the internal composition of other planets with unknown internal structures seems somewhat moot. We are at present in the stage of ascertaining their chemical composition by the use of one fundamental parameter, their mean density, coupled with spectral data (on some integrated level in the planets' atmosphere). Under these conditions only bounds can be sought and it is completely necessary to view measurements in this light. A laboratory experiment or theoretical study may be equally effective in providing bounds when used with planetary parameters. Such laboratory or theoretical studies may be absolutely necessary to understand and interpret data obtained at the planet itself.

Two bounding parameters, which have been indispensable in the study of the chemical composition of the Earth's interior, are its density distribution, including surfaces of singularity, and the variation of its electrical conductivity with depth. While the density distribution is best obtained from seismic data coupled with a knowledge of layering, planetary mass and higher-order gravitational terms, seismic data for the outer planets seem inaccessible at present. A practical substitute is to calculate a self-consistent density distribution using a density measured at a level sufficiently high to enclose most of the planetary mass and sufficiently low to allow the assumption that the material from that point inward obeys, approximately, a known equation of state.

This approach has been used in several recent papers (see References at the end of this section) to estimate the chemical composition of the outer planets. The results indicate a hydrogen abundance of 70-85% for Jupiter, about 60% for Saturn, less than 23% for Uranus, and less than 14% for Neptune. The calculations for Uranus and Neptune are based on only second-order gravitational terms (J) and could be improved somewhat by determination of the fourth-order term.

At the heart of all of these determinations lies the knowledge of the equation of state of hydrogen. Its transitions of state, fluid to solid and solid to solid, are of great interest. In particular, the physical characteristics of each phase (including also their electrical conductivity) are of large-scale importance in determining our picture of these planets. To ask a few specific questions:

1. In what regimes of temperature and pressure do solid metallic or other possible solid phases of hydrogen exist, as distinct from solid molecular hydrogen?
2. Does liquid hydrogen show metallic properties under any conditions of temperature and pressure?
3. Does the application of sufficient pressure cause the appearance of a semiconducting or (poor) metallic phase of helium? These questions will have to be answered by either laboratory or theoretical research. Their answers can be obtained well before flights are attempted to any of the major planets and, indeed, should be obtained, since many of our initial measurements and measuring instruments will be determined by our preconceived notion of what parameters are important to measure.

We are now able to state what direct contributions planetary exploration may make toward determining the density distribution within a planet. Parameters such as radius, planetary mass, and terms in the gravitational potential may or may not be improved in precision by initial space studies (as compared to ground studies). But it is difficult to see how any ground measurements can give the other two fundamental parameters, namely the location of the significant interface and the density at that level. Equally so, the variation of electrical conductivity with depth can only be a space measurement. To a person not accustomed to thinking of matter in a discrete (atomic) fashion this use of the electrical conductivity as a bounding parameter may be obscure, but the fact is, combined with our present laboratory and theoretical understanding of the conduction mechanism in gases, liquids, and solids, it is quite sensitive. When combined with a knowledge of density, many other related physical parameters, such as thermal conductivity, may be estimated. In principle the variation of electrical conductivity with depth may be obtained by measurement of the electric and magnetic field at a point. This method seems quite attractive from a space technology standpoint. An alternative method, the long-term measurement of the magnetic field only, while much in use on Earth, seems attractive only to manned landing missions (where feasible) or to orbiters or landers that store information for release at a later time. Of particular interest in the study of the electrical conductivity with depth are its possible surfaces of discontinuity which may or may not coincide with those of density distribution. (A surface of high metallic conductivity would be the deepest recognizable due to the shielding effect. Even if deeper layers were present they would not affect the observations).

So far as measurements are concerned, there is little to distinguish one outer planet from the other. The pressure regime of Earth-bound interest ranges up to 100 megabars for Jupiter and over 10 megabars for the others. Direct measurements of the equation of state of hydrogen are definitely possible to 0.2 megabar and over a wide range of temperatures (with explosive techniques). This is a factor of 10 higher in pressure than current measurements. Further values must be obtained theoretically as must also information about the type of conductivity of helium or liquid hydrogen. The actual space experiments called for are as follows:

An orbiting vehicle for each planet including among its experiments:

(a) Ejection of a geodetic satellite data from which may be relayed to Earth by the orbiter.

(b) Measurement of electrical and magnetic fields at the orbiter. The geodetic satellite could conceivably do this also if its geodetic mission were not affected. It would be valuable to have two measurements.

(c) A drop sonde measuring temperature, pressure, etc. and perhaps locating a fluid-solid interface by its destruction. If possible this portion of the orbiter task should not be performed until data obtained in (b) had been analyzed for the location of discontinuities.

Thermal History and Theories of Formation of Jupiter

Let us take 1000°K as an interesting number for the central temperature. Very probably lower temperatures would have little effect on the structure and would not be noticed on the surface. The energies required to heat the planets to these temperatures are 41.7, 41.0, 40.3, and 40.5 on a logarithmic scale (ergs) for Jupiter, Saturn, Uranus, and Neptune from Table 2. In each case insolation would just make a dent in this, provided there were a very efficient greenhouse. A few years ago this would have been thought ridiculous, but we now believe that Venus' surface is 700°K, so that this possibility must be kept in mind.

More likely, gravitational energy or rotational energy would be responsible for high temperatures if they were to be found. Let us consider gravitational energy. If the planets formed by a long steady accretion of material, at first by sticking of colliding ice masses, and later by infall of gases, the gravitational energy could have been dissipated at the surface, permitting the internal energy to remain low. If, on the other hand, the accretion was rapid (perhaps as fast as the free-fall time scale of a few hundred years in the solar nebula) each accreted layer would be "hit from behind" by a succeeding layer before it could cool. If the planets are cool, a slow accretion is indicated.

What about rotational energy? So long as the rotation is solid-body, there is no way for it to dissipate as heat. The most extreme case we can think of requires that the interior now be rotating twice as fast as the exterior, say, as a result perhaps of slowing of the exterior by magnetic torques in the early history of the solar system. The energy of differential rotation would be dissipated as heat as the system moved back toward solid-body rotation. That there is differential rotation today cannot be disputed, but the apparent values are small: 1/2000 from the radio versus optical period of Jupiter, and 1/120 from the equatorial accelerations. There is not yet any evidence from the models (Peebles, 1964) that the gravitational moments disagree with the assumption that the observed surface rotation periods are valid throughout Jupiter and Saturn.

How can we determine whether they are hot?

(a) If evidence can be adduced to show that they radiate more than they receive from the Sun (Öpik, 1962) we have positive proof. Unfortunately, the present data on infrared emission and optical solar absorption are too limited, although there is a hint of such an effect in Jupiter according to Öpik. Detailed infrared studies, including observation of the back side, are needed.

(b) Convection in the atmosphere must be driven by heat from below. While the observed billowing clouds could be due to solar heating, the adiabatic atmosphere down to several thousand kilometers depth that is needed by Peebles (1964) to explain the observed gravitational moments probably could not be solar-driven, and so their existence points to internal heat. Clearly, further observations of the gravity field, and Earthbound studies of pressure-density relations, and theoretical models are needed to settle this point. Ultimately, a lander, equipped to sample temperature at various depths, would give useful information. In the meantime, further efforts to probe beneath the clouds by radio methods are worthwhile.

(c) Conceivably, the planets are hot enough to affect the equation of state of the relevant materials (>5000°K, say). This would show up as a disagreement between more precise gravitational data and cold equations of state found in the laboratory.

In the above discussion we have ignored two sources of energy: gravitational energy released as the planet undergoes a phase transition and contracts, and nuclear energy of fusion, produced as deuterium burns. The latter is 10^{45} ergs in Jupiter, if H/D = 6600, enormously exceeding every other source. To be released, however, temperature must exceed 2×10^{50} °K, and there is no evidence for such enormous temperatures. Nor can one see how they could be produced during accretion, for even if all the gravitational energy were kept in, the temperature would only be 60,000°K, according to Table 2. However, further studies of deuterium-burning stars should be conducted to find out how close Jupiter is to such a configuration.

Chemical energy is not so easy to dismiss, since appropriate data are not available. As things stand, observations confirming that the planets were hot would indicate either rapid accretion or that the interiors are solid—both interesting.

Magnetic Fields of the Outer Planets

For a discussion of this subject see the review by R. Hide "Planetary Magnetic Fields" in section 9, p. 85.

Interface between Solar Wind and Interstellar Space

It would be desirable, in a trans-Martian mission, to provide instruments for a search for the outer edge of the solar wind. We are fairly confident that it extends to Jupiter (5 AU) at least, because of the radiation belts of that planet. But Saturn (9 AU) has no detected belts, which might be because it is beyond the solar wind, or alternatively, because it has a small magnetic field, or because its belts are swept clean by the rings.

Since the density of the wind decreases with increasing distance from the Sun, there must come a point where the back-pressure of the interstellar gas is sufficient to stop it, perhaps at a shock front. One might expect a rather sharp transition at the edge, with possible turbulence present. Merely to detect that the solar wind had stopped would be of great interest. Plasma probes, magnetometers, and particle detectors (for solar and nonsolar cosmic rays) would be required. Since these instruments would be useful near the target planets, proper design would meet both requirements.

Once out of the solar wind, one is in interstellar space, in a sense. It has been suggested by Parker that the galactic cosmic-ray flux may be 10 times larger out there, owing to the excluding effect of the solar wind. If so, there would be interesting implications for astrophysics.

The edge of the solar H II region (where hydrogen is ionized) is also uncertain but may be in the same region. This could be detected by a detector looking at the intensity of a strong solar emission line beyond the Lyman limit, to detect variations from the inverse-square law.

Atmospheric Circulations of Jupiter and Saturn

An account of selected observational and theoretical aspects of the atmospheric circulations of Jupiter and Saturn is given in R. Hide, "On the Circulation of the Atmospheres of Jupiter and Saturn", in section 9, p. 107. Not mentioned there are the Spinrad effect and the Murray effect, both of which have one feature in common—they seem to be unobservable at the present time.

Spinrad discovered that, according to the Doppler shift of ammonia lines in spectra of Jupiter and Saturn, the material giving rise to these lines appears to rotate at several kilometers per second relative to the visible planet.

Murray and his collaborators found that the temperature in a region of Jupiter's surface occupied by the shadow of a satellite appeared to be much higher than the surrounding regions.

Satellites

The study of the satellites of a planet yields information on the planet's gross properties, including internal structure, and past history, and cannot, therefore, be divorced from the study of the planets themselves.

Some of the properties of the satellites can be found in Astrophysical Quantities by C. W. Allen (1963, Athlone Press, University of London).

The theory of the processes by which the planets acquired their satellites raises fundamental questions about the origin of the solar system. For example, the capture mechanism of J V, (the small innermost satellite of Jupiter); and the retrograde orbit of Triton (the large satellite of Neptune) bear directly on the history of the outer planets.

There is evidence that at least two of the satellites of Jupiter, Io and Ganymede, interact strongly with the radiation belts of Jupiter, a discovery so recent that its implications have hardly been explored by theoreticians (see R. Hide, "Planetary Magnetic Fields" in section 9, p. 87).

Strategy for the Study of the Outer Planets

The sequence of study of the major planets might take this form:

1) ground-based observation
2) studies from balloons, rockets, and satellites
3) fly-by
4) orbiter (general purpose)
5) specialized orbiters
6) lander

Some notes on these possibilities are given below.

1) Ground-based observations comprise visual and photographic study, spectroscopy, infrared studies, and radio studies. There are very worthwhile projects in each of these areas. Some of the more fruitful at the present time include analysis of cloud structures using high-resolution photography and polarization techniques (used together with the theory of inhomogeneous atmospheric absorption and scattering), study of the Murray-Wildey effect (Murray et al., 1964), extension of the infrared spectrum into other windows, and attempts to gain higher resolution at decameter wavelengths. High-resolution spectroscopy over the whole accessible range is important.

2) Studies from balloons, satellites, and rockets include infrared photometry in the 10-100μ range, studies of ultraviolet albedo and spectrum (Stecher, 1964), high-resolution ($0\overset{''}{.}1$) photography (Danielson, 1964), and extension of the decameter spectrum below 1 Mc/s.

3) A fly-by could perform a number of measurements vital to the success of later missions, including assessment of the magnetic field, charged particle density, and amount of particulate material. The latter is obviously crucial in Saturn's rings, but should be considered for the other planets, whose equatorial regions may also contain particle populations. In addition, a fly-by could assess the infrared emission from the back side, and perform other simple remote measurements, provided

the instruments could be protected against intense radiation fluxes (for Jupiter, especially).

4) An orbiter could carry out a great many measurements on the planet itself, including photography with 10 km resolution (from synchronous orbit; see below) detailed infrared studies as a function of local time and latitude, laser reflection studies of cloud heights, spectroscopy over the 0.1 - 100 μ range, radiometry over the 0.1 - 10 cm range, topside ionospheric sounding, detailed mapping of fields and particles in space and time, and detection of nonthermal radio emission from such particles. The following table gives characteristics of synchronous orbits for each of the planets:

Planet	Rotation Period (h)	Orbit Radius / Radius of Planet	Orbit Radius (km)	Altitude (km)
Jupiter	9.83	2.30	1.6×10^5	9.3×10^4
Saturn	10.23	1.93	1.2×10^5	5.6×10^4
Uranus	10.82	2.59	6.2×10^4	3.8×10^4
Neptune	15	3.60	8.0×10^4	5.8×10^4

A synchronous orbiter of Jupiter would see J V as an object 2×10^4 km away, 12 inches in diameter, and Io, 2.6×10^5 km away, 43 inches in diameter. Such an orbiter for Saturn would be in Cassini's division of the rings, and would be useful in studying the rings.

5) Specialized orbiters would include low-altitude orbiters for geodetic purposes, eccentric ones for encountering satellites, and close inclined orbiters for study of special areas like the Red Spot.

6) A lander would be a very ambitious undertaking. For example, about 50,000 ft/sec would be necessary to obtain a low orbit above Jupiter, and the vehicle would still differ by 100,000 ft/sec from the atmosphere rotating below it. If it were possible to bring a payload into a terminal free fall in the atmosphere, one could obtain decisive information obtainable in no other way, such as the pressure-density relation deep in the atmosphere. Special interest would attach to acoustic sounding to locate possible interfaces.

The schedule might go as follows if there were one vehicle available per year starting in 1975. Fly-bys could either be sent to all four planets early in the period or the effort could concentrate on Jupiter, several fly-bys being succeeded by one or more orbiters. It should be emphasized that Uranus has considerable interest because of its unusual obliquity and its apparent deficiency of hydrogen, yet a minimum energy orbit is 14 years to encounter. It may be decided to send one of the earliest fly-bys to it, in order to obtain data on a reasonable time scale.

It should be recognized that data transmission from such vehicles will be 12, 40, and 160 times more difficult from Jupiter, Saturn, and Uranus, respectively, than from Mars (see Deep Space Information Transfer, section 8, p. 59).

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6. SMALL OBJECTS IN THE SOLAR SYSTEM

MAJOR QUESTIONS: PROPERTIES AND ORIGINS

1) Which of the objects in this group, if any, contain primordial planetary material?

Planetary material still in the state it assumed when the planets first formed would contain valuable information about processes that operated at that time. The Earth contains none: it is a very active planet, and all the crustal material has been repeatedly reworked by melting, weathering, differentiation, etc. One object of space exploration is to recover material of this type. The larger satellites may or may not contain primordial surface material—they may have been geologically active enough to destroy it all. Small satellites and asteroids are more likely sources, since geological activity is a function (among other things) of planetary size. There is evidence in the meteorites, however, that even objects of a few hundred kilometers dimension were geologically active. Comets may offer the possibility of sampling primordial material more comprehensively (since frozen volatile compounds are present as well as the earthly materials that make up meteorites and, presumably, asteroids and satellites) and in a better preserved state (because of their apparently low temperatures) than do asteroids or satellites.

2) How stable is the present orbital configuration of comets, asteroids, and satellites?

Most asteroids and satellites are in orbits of small inclination and eccentricity, the latter extremely small, for some satellites, suggesting a long history of tidal interaction. Several pairs of Saturn's satellites are commensurable (i.e., of integral ratio in period), but form a stable pair because of phase relationships in their motion. On the other hand, there are some orbits that appear to be very unstable with high eccentricities and inclinations. This is true of nearly all comets, particularly those with the most pronounced physical characteristics of comets. It is also true of a few asteroids, such as Hidalgo (eccentricity 0.65), and even one satellite of Neptune, Nereid (eccentricity 0.74). It therefore is of cosmogonical interest to ask how far back the present configuration can be carried, and, if such extrapolation is not very far on a geologic time scale, whether the present configuration can be considered a sample of the situation which has existed throughout the evolution of the solar system: i.e., a population of largely slowly changing members, with a few erratic newcomers.

3) Where do comets come from?

Comet orbits fall in two classes: some have extremely eccentric, almost parabolic orbits, of semimajor axis more than 10^4 AU, while a few hundred have more or less elliptic orbits that cause them to return periodically. The latter group appears to be depleted compared to the former, which suggests that they have been perturbed from a near parabolic to an elliptic orbit. The question thus appears to be whether comets can be considered as always having been members of the solar system—in which case they would constitute samples of the primordial outer parts—or are they interlopers from interstellar space (as suggested by the variety of their inclinations) that chanced to have a small enough difference in motion from the Sun to be captured by the solar system?

4) How frequently do collisions and near encounters occur in the asteroidal belt, and what are their effects on rotation and orbital motion?

The periods of the asteroids are distributed in such a way that none are found to have certain integral ratios to Jupiter's period (2:1; 3:1; 5:2; 7:3), (as would be expected from stability considerations), but instead they have values close to certain other ratios (3:2; 4:3; 1:1). The latter nearly commensurable cases suggest that the accumulated effects of near encounters are significant. The periods of rotation of the 27 asteroids for which it has been measured with good assurance are all short: 2.1 to 16.8 hours, and most of them lie between 5 and 10 hours. This amount of angular momentum also suggests that collisional processes have been important. Correlations between chemical compositions and cosmic-ray exposure ages of some meteorites suggest that they have been formed by the breaking up of asteroid-like bodies. A better understanding of collisions and near encounters is needed.

5) Why did the process of planetary formation yield a multiplicity of objects in only one region of the solar system (so far as we know), namely the asteroid belt, and a small number of relatively massive, widely separated planets elsewhere?

The searches that have been made appear to justify a confidence that nothing of comparable albedo could exist larger than 100-200 km at Jupiter's distance; 320-640 km at Saturn's distance; 660-1320 km at Uranus' distance. If the albedos were comparable to those of the major planet satellites, these figures would be halved. Hence a considerable second "belt" of asteroids is possible only beyond Saturn.

7) What is the nature of tidal interaction between satellites and planets?

Secular accelerations are estimated for seven satellites, but are firm only for the Moon and Phobos. However, the closeness to the planet and the very small eccentricities of many satellites suggest that tidal interaction may be significant, and hence that better determination of their orbits would yield information about the planets. In the case of Jupiter and its satellites there is also a possibility of significant electromagnetic interaction.

COMETS: SPECIFIC QUESTIONS

Most Important

Are cometary nuclei single bodies of icy material?

If so, what compounds do the ices comprise?

What part do frozen free radicals play?

What is the structure of cometary dust? Meteor observations suggest a highly skeletal nature.

Do stable isotope ratios in icy compounds or dust differ from the usual terrestrial and meteoritic values?

What is the age of the dust (K/A, U/He methods)?

Can nuclear material be identified as primordial solar system condensate? Accumulated interstellar dust grains?

Important

Can the present distribution of elliptical comet orbits be explained as perturbations of parabolic orbits?

How long, on the average, have the present elliptical comets been in orbit?

What is the composition (major and minor elementary abundances) and size distribution of cometary dust?

Is the dust composed of discrete minerals (if so which), or essentially amorphous material?

How are tail gases ionized?

How are they accelerated away from the Sun; if by the solar wind, what is the coupling mechanism?

Are there great intrinsic differences between comets?

From meteor observations, dust from old comets seems coarser than that from new comets—what are detailed differences?

Less Important

How fast do comets rotate?

What is the ice:dust ratio in nuclei?

What is the nuclei temperature?

Are nuclei homogeneous or stratified?

Do complex organic compounds play any part in comets?

What causes cometary bursts?

What are the compositions of coma and tail gases?

What reactions are involved in producing the gases from parent compounds in the nucleus?

ASTEROIDS: SPECIFIC QUESTIONS

Most Important

Are some or all meteorites derived from asteroids?

What is the composition, structure, mineralogy, and age of asteroidal material?

Do asteroids vary in composition, and if so, is it related in any regular way to position in the solar system?

Important

What shapes do asteroids have? Is there a correlation between shape and (1) size, (2) rotation, (3) orbital characteristics—i.e., with the probability that close encounters or collisions have occurred in the past?

What can be inferred from shape and rotation about mechanical strength and past history?

Are comets and asteroids discrete classes of objects?

What is the total population and size distribution of asteroids?

Are compositional layerings or other variations revealed on fracture surfaces of fragmental asteroids?

Are asteroid surfaces relatively stable, or are they being eroded away at a significant rate by collisions with debris of small dimension?

Less Important

How many asteroids are still intact (most are thought to be fragments)?

Do any asteroids have dust layers on their surface?

Are there systematic compositional differences between belt, Mars-crossing and Apollo-group asteroids?

SATELLITES: SPECIFIC QUESTIONS

Which satellites are captured asteroids, and, conversely, which asteroids are detached satellites? (Of the 31 satellites identified, 21 have periods less than 30 planetary days; one has a period of 79 days; and nine have periods of more than 250 planetary days. Five of the last group have retrograde revolution. Of the close group, six are comparable to the Moon in size; all other satellites are very small. Hence there is a strong suggestion that the distant group is made up of captured asteroids. On the other hand, there are interactions between asteroid orbits and major planet orbits that suggest detachments have been possible, such as Pluto from Neptune—a possible explanation of Triton's retrograde revolution, as well).

What are the orbital accelerations of the various satellites?

What is the stability of the orbit of each satellite to nontidal perturbations?

What are the densities and surface compositions of the various satellites?

Do they contain evidence of formative processes?

What are the ages of their surface materials?

Are there systematic differences in composition from one satellite system to another?

What are Saturn's Rings? (The rings are at 1.6 to 2.2 planetary radii, with 2 gaps corresponding to satellite commensurabilities. The obvious suggestion for their origin is that they represent the fragments of a former satellite, but a complete explanation involves tidal effects on this satellite, the process of collision, and the interactions of the resulting fragments with the atmosphere, the inner satellites, and each other.)

DUST: SPECIFIC QUESTIONS

Where does the interplanetary dust come from? (The zodiacal light indicates that most of the dust is in the outer regions of the solar system, where solar effects are weaker. Its distribution is little known, however, so there are questions about its source and the rate at which it is replenished. A high rate for the latter would be an indicator of higher masses for comets as a source or a greater density of small asteroid collisions.) Specifically, we need to know the size, velocity distributions, mass density, optical properties, and electrical charge of interplanetary dust.

SUMMARY

These considerations indicate that, among the smaller objects in the solar system, the comets would be the most interesting to investigate. Not only do they differ markedly from the more accessible objects but their composition and structure seem most likely to reflect conditions that existed when the solar system was formed.

The most valuable method of investigation would consist in landing instruments on a cometary nucleus to determine the local conditions and to recover a sample of its material. However, a much simpler fly-by passing through a coma would also be extremely interesting. The inner coma appears dense enough to make mass spectometry feasible, and knowledge of comets would also be greatly enhanced by micrometeorite counters, magnetometers, and measurements of gas density, ion density, etc.

Return of samples from several asteroids would settle some questions of meteorite origin and history, and afford further evidence of the relationship of asteroids to comets and satellites. A fly-by passing several asteroids and making photographic, photometric, and polarization measurements would improve knowledge of their geometry, dynamics, and surface characteristics.

Determination of natural satellite orbits, with particular emphasis on tidal phenomena, would be most improved by placing artificial satellites in orbit around the planet. Information regarding the chemical composition of satellites would be of similar value to that from asteroids.

The amount of dust in accessible regions of the solar system appears to be too small to justify the design of a probe especially to study it, but devices should be flown in probes on other missions in order to measure polarization and brightness of the zodiacal cloud at different points in the solar system. Presumably particle impact counters will continue to be carried on all vehicles for engineering reasons.

7. MERCURY

PRESENT KNOWLEDGE

The only parameters that are known at all satisfactorily are the elements of Mercury's orbit. The high eccentricity and inclination are singular in comparison with any other planet but Pluto, and they imply uncertainties about the past history of the planet. The mass is known to no better than 5% and the radius to about 10%, and so the density may lie anywhere between 3.6 and 6.6. The rotation period, long thought to be synchronous with the 88-day revolution, is given by radar observations as 57 days. No satisfactory determination of the rotation axis is available. Before the radar work, Mercury was studied visually and features of the image that seemed to be permanent markings were sketched. The only firm conclusions from this work are that Mercury has a system of large-scale markings and that the theoretical prediction of a significant libration is consistent with the apparent movements of the markings with respect to the terminator. The same kind of visual analysis had also been used previously to support the idea of the synchronous rotation period, now contra-indicated by the radar observations.

1. What is the nature of the surface? In which respects is it identical to the lunar surface? What are the relative roles of volcanism and crater impact in forming surface features? By the time a probe is sent

around Mercury, we will have a fairly good knowledge of the lunar surface, and Mercury would then be an interesting object to study on a comparative basis.

2. Is there an atmosphere, and what is its composition? How is it maintained?

The question of the existence and properties of any atmosphere is open at present, although the surface total pressure cannot be greater than a few millibars. If an atmosphere exists, it is of interest to know the balance of the processes that maintain it—thermal escape, solar wind stress, and outgassing from the interior.

3. What are the gross parameters of the density and density distribution of Mercury? Is it differentiated, and is it spherically symmetrical in internal structure? A lack of spherical symmetry would be a strong indication that this or any other planet was formed from agglomeration of two or more smaller objects, with strong implications for the problem of explaining the present orbit. The most relevant single parameters that tell something about this are the mass, geometrical figure, and the moment of inertia (which are not satisfactorily known).

4. Does Mercury have a permanent magnetic field?

5. What are the parameters that describe the tidal interaction of Mercury with the Sun? These include the amplitude and phase of the tidal bulge, as well as the details of its liberation due to tidal forces. Besides giving information about the mechanical properties of the interior, elucidation of this question will enable the tidal interactions to be adequately included in the equations of motion to assess possible mechanisms for the evolution of Mercury's orbit and rotation.

EXPERIMENTS ON MERCURY

It is feasible and highly desirable for the planetary program in the next twenty years to go a significant way toward answering these questions. The amount of effort in the way of probes to Mercury does not seem to be unreasonable. Recommended experiments include:

1. Continued astronomical examination from the Earth. Further spectroscopic measurements will help to settle the questions about the atmosphere.

2. Telescopic observations from a planetary Orbiting Astronomical Observatory or balloonborne telescope can give more complete spectroscopic information and, given adequate stabilization, will give significantly better pictures than are available from the ground. More work on the photometry of the surface with this increased resolution will be useful.

3. A close fly-by mission will answer many of the first-order questions posed above:

(a) Photography of the surface (overall), including optical determination of the geometrical figure of Mercury.

(b) Tracking of the spacecraft orbit will improve the mass determination significantly.

(c) Determination of the magnetic field in the neighborhood of Mercury, including the effect of the planet on the solar wind.

4. A lander would answer certain questions:

(a) Emplacement of corner reflectors or transponders for optical or radar tracking would enable the forced libration and rotation to be determined accurately enough to give some idea of the moment of inertia.

(b) A vertical seismogravimeter will determine the gravity tides directly, measure the seismicity of the planet, and under favorable circumstances enable the internal structure of the planet to be determined.

(c) Direct determination of the chemical composition of the atmosphere and surface could be made.

5. A geodetic satellite would give the most direct evidence on the question of spherical symmetry (aside from the optical figure from a fly-by).

Mercury is an object unique in the solar system, and should be included in a comprehensive program of planetary exploration. On the one hand, it can be classed as a terrestrial planet; on the other, it can be classed with the major satellites such as the Moon, Io, Triton, etc. From either point of view, comparative study of surfaces, atmospheres, and interiors is relevant to the more general problem of the evolution and origin of the planets and the solar system as a whole.

8. DEEP SPACE INFORMATION TRANSFER

The problem of transferring information from distances like those to Venus or Mars is a major one and is likely to limit very severely the types of investigations that can be conducted, or determine largely the payloads that need to be carried. Very low rates of information transfer, such as are now used or proposed in immediate planning, place too great a handicap on the investigations and every effort should be made to discover better solutions to this problem. The following three possibilities have received discussion:

Laser Communication

Signaling by means of modulated laser beams is a technique that will undoubtedly be developed and which may have to be considered in competition with radio. In the comparison between the two methods a large factor is lost in the sensitivity of reception for light as compared with radio, both on account of photon statistics and on account of the smaller size of optical receiving instruments compared with radio antennas. On the other hand, a large factor may be gained from the greatly increased transmitter antenna gain. The destruction of coherence of the beam through the atmosphere of the Earth is a severe handicap and may result in the need to observe from high up in the atmosphere or above it.

Photographic Return

The total information content that can be inscribed on photographic film is so great that in any case where a return flight can be contemplated this method of storage is likely to be much superior to any telemetry over big distances that could be achieved with the same payload. The information so returned to the vicinity of the Earth could either be delivered in the form of a package that is re-entered or be read out by a mechanism when the vehicle is in the vicinity of the Earth. In defining a mission profile this possibility should be kept in mind, especially where much information can be gathered in the remote location. In-flight film processing provides the additional advantage of accepting a very high data rate at the planet which can be read out at an arbitrarily low rate subsequently. Finally, total data storage by means of film recording can eliminate storage as a bottleneck in any data return system.

Improvement of Present Radio Methods

Radio methods will undoubtedly continue to be used for a long period to come. It is therefore worthwhile to plan for a substantial improvement in them. Improvements in payload power supplies are in progress and will contribute significantly. Ground-based equipment for reception can only be improved significantly by increasing the antenna area.

In deciding on the expenditures on ground-based antennas in comparison to those on the space vehicles it must be kept in mind that the ground-based antenna systems will service a large number of such vehicles over the years, and that the cost of the ground-based equipment should thus be appropriately matched to the cost of all proposed deep space investigations. It is thought that an appropriate match would be provided if a much larger proportion of the cost of the deep space investigations were expended on the receivers on the ground, thereby making each dispatched vehicle much more effective. The 210-ft dishes now under construction should not be considered as a final answer for this purpose.

The present state of the engineering of steerable large-area radio antennas greatly favors the construction of densely spaced arrays of parabolic dishes. Cost optimization studies have indicated that a size between 85 ft and 120 ft is likely to be cheapest per unit area of effective receiver surface. Since technical difficulties with arrays have largely disappeared, consideration should be given to greatly increasing the ground-based receiving area by these means.

The most practical and economical way would seem to be to design and construct a dish in this range of size, carefully devised to be suitable for mass production. Such a standard mass production type of dish, once produced, would be of great value for many purposes and it would allow receiving systems to be built up gradually and at a predictable cost. This cost may well be very much lower than the present costs per unit area of steerable surface of an antenna receiving 10-cm waves as a result of the advantages of mass production. While this approach is not likely to solve completely the deep space communication problem, it is likely to make a real and useful improvement that will persist many years to come.

We recognize, of course, the basic correctness of over-all cost-effectiveness analysis of the elements of deep space information systems as a means of determing the appropriate scale of those elements. We do feel, however, that present planning may be based on too conservative a goal for total information return and may not be taking advantage of unit cost reductions in antennas that may be possible if the total information goal is increased to the level we feel is required.

9. WORKING PAPERS: TOPICAL REVIEWS

ORIGIN AND EVOLUTION OF THE SOLAR SYSTEM

George B. Field

Stars with planetary systems like the Sun's do not appear to be isolated freaks among the millions of stars known to astronomy. Brown (1964) has summarized the evidence for believing that they are very common: detection of planets by astrometric means, the high frequency of multiple stellar systems, and the correlation of stellar rotation rate with type of star, suggesting transfer of angular momentum to neighboring bodies at previous times. Theoretically, it is expected that the formation of a star by condensation from the interstellar gas will be accompanied by the formation of a flattened disk of gas containing much of the angular momentum of the system (cf., ter Haar and Cameron, 1963 for a review). Indeed, there are at least superficial resemblances between the disk of the solar system and the disks of the giant spiral nebulae. Hence, the origin of the solar system may be connected not only with the origin of stars, but with the origin of galaxies as well.

Planetary systems are of greatest interest because of their ability to support life. But an almost equally important reason for studying them is to assess the process of star formation, which even now is apparently occurring everywhere around us in the vast reaches of interstellar space, and transforming the Milky Way from a system of gas to one of stars and planets. One class of stars in particular, the T-Tauri stars (Herbig, 1962) shows signs of being very recently formed. It is interesting that these stars, independently known to be young, seem to be surrounded by clouds of dust and gas rather like the disks expected on theoretical grounds.

Theory indicates that the disk in the solar system (often called the solar nebula) contained largely hydrogen and helium, with small amounts of particles composed of H_2O, NH_3, and CH_4, together with lesser amounts of heavier elements, admixed.

It is believed that the Sun, planets, satellites, and other bodies condensed from this disk. One view is that the Sun and perhaps the major planets were formed by gravitational instability of the disk, and that all portions of particularly dense regions rapidly fell inward under the influence of self-gravitation. On the other hand, the particles could conceivably have agglomerated into larger masses by means of molecular binding forces. These later accreted hydrogen and helium by gravitation. Certainly the homogeneity expected on the basis of gravitational instability is not now evident in the solar system, but this may be because of the difficulty in disentangling the effects of later processes, such as evaporation of light gases, chemical differentiation, and particle accretion.

The key to deciding these matters may well be placed in our hands by a well-executed program of lunar and planetary exploration. First, we can hope to assess the chemical composition of the major planets (from which evaporation was probably relatively slow) for comparison with that of the Sun. Next, we can test the equality of isotope ratios for all accessible bodies (now validated only for the Earth and the meteorites). Current theories of nucleogenesis (Burbridge et al., 1959) suggest that these ratios reflect conditions in the supernova explosion in which all of the heavy elements found in the solar system were formed, and so discovery of anomalous ratios would indicate that the body in question may have been captured from an outside source. Third, we can search the system for exposed surfaces of relatively small bodies (like the Moon and other major satellites) that have not been internally differentiated. Such surfaces could yield information on primitive material. Fourth, we can assay the abundance of elements, like deuterium and lithium, that are thought to have been created by energetic particles from the Sun early in the history of the system. If differences are found, it may be a clue to the spatial variations in density of such particles.

Other questions revolve about the dynamical properties of the solar system. While it is widely assumed that the system has remained relatively unchanged since its origin, there are various suggestions to the contrary. Both the comets and the outer satellites of Jupiter occupy orbits of high eccentricity and random inclinations. Possibly either or both kinds of objects were captured: the comets from interstellar space and/or the satellites from debris left early in the history of the solar system. Similarly, it has been pointed out that Pluto could have originated as a satellite of Neptune. The barrier to evaluating these changes appears to be primarily theoretical, and more effort in the general area of n-body problems will be necessary here. MacDonald (1965) and his colleagues have stressed the influence of tidal torques in affecting the orbits of satellites and the rotation periods of planets over long periods. If these dynamical properties can be further evaluated, we may find important clues about the state of the system at earlier times.

Finally, there is at present little evidence that the process of formation was simultaneous for all of the bodies in the system. Isotope abundances will, of course, be evaluated for all samples of the solar system that can be acquired, perhaps elucidating this point.

In summary, a good working hypothesis is that the solar system formed by gravitational instability or accretion, or both, in a solar nebula. This hypothesis can be tested by chemical analysis, isotope chemistry, geophysical studies of primitive bodies, and theoretical studies of dynamical processes. Whatever the outcome, we will learn things that apply not only to the solar system, but most probably also to uncounted other planetary systems throughout the Milky Way.

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DYNAMICS OF THE SOLAR SYSTEM

W. M. Kaula

Summary

The dynamical experiments that contribute the most to planetary exploration are close artificial satellites, preferably in orbits of high inclination. Such orbiters about the Moon, Mars, Venus, and Mercury will provide much information about the departures from hydrostatic equilibrium of these planets. The existing Doppler tracking at frequencies above 2×10^9 cps is amply accurate for this purpose, although a transponder placed on the planetary surface would help to determine the perturbations better. The relatively high upper atmospheric density of Mars and Venus makes it desirable that orbiters around these bodies be instrumented to distinguish surface from body forces and to be kept in a purely gravitational orbit.

Orbiting artificial satellites also provide the best means of improving determination of orbits and masses of natural bodies in the solar system, in furtherance of the present progress using interplanetary probes and radar ranging. The principal dynamic value of an orbiter about Jupiter would probably be to improve determination of the masses and orbits of its four large satellites.

The determination of the moments of inertia of Venus and Mercury appears unpromising, because their slow rotations make very small both their hydrostatic flattening and their responses to any torques. In the case of the Moon, if results from a lunar orbiter indicate that existing estimates of the physical libration are in error, then it would be worthwhile to place laser reflectors or radar transponders on the lunar surface to be tracked from Earth.

Discussion

The scope of this paper is the use of motions of natural and artificial bodies to determine characteristics of planetary structure and to indicate the origin and evolution of the solar system.

The dynamical properties of the solar system have always been the most important data available pertaining to the origin of the solar system. These data comprise the general properties, such as the nearly coplanar, counterclockwise character of the revolutions and rotations of planetary and satellite systems; the distribution of planets, as expressed by the Titus-Bode law; and the concentration of angular momentum in the major planets, as well as several exceptions to these properties.

A more recent application of dynamics has been the use of artificial satellites and planetoids to determine more accurately the masses, orbital elements, and variations in the gravitational fields, size and shape and the upper atmospheric densities of the natural bodies. This application takes advantage of two characteristics of artificial vehicles: much more accurate tracking, and orbits that are much more sensitive to perturbations (and hence much shorter-lived) than the orbits of natural bodies.

Another use of dynamics, the determination of fundamental properties of gravitation, is discussed in "Gravitation and Relativity", in Chapter 6 of this Study. Relativistic experiments will therefore not be discussed here. However, several of the experiments we shall discuss should be performed either as forerunners to, or concurrently with, relativistic experiments, since the success of the latter usually depends on the removal of a good deal of Newtonian clutter.

The principal dynamical experiments that have been performed fall into two categories: (1) close Earth satellites and (2) planetary probes.

The close Earth satellite orbits have two dominant characteristics: (1) a steady motion of the node and perigee, due to the Earth's oblateness, or second-degree zonal harmonic J_{20} of the gravitational field; and (2) a decrease in the semimajor axis and eccentricity due to atmospheric drag (or, occasionally, an increase, if circumstances allow radiation pressure to be dominant). The smaller even zonal harmonics $J_{2n,0}$ of the gravitational field also contribute to the secular motions of node and perigee, $\dot{\Omega}$ and $\dot{\omega}$, while the essential effects of other variations $J_{\ell m}$ in the gravitational field are sinusoidal forced oscillations about the precessing Keplerian ellipse of the type

$$\Delta_{\ell m} S_i = J_{\ell m} \sum_{p,q} \frac{f_i(a, e, I)}{\left[(\ell - 2p)\,\dot{\omega} + (\ell - 2p + q)\,\dot{M} + m(\dot{\Omega} - \dot{\theta})\right]} \begin{pmatrix} \sin \\ \text{or} \\ \cos \end{pmatrix} \left[(\ell - 2p)\,\omega + (\ell - 2p + q)\,M + m(\Omega - \theta - \lambda_{\ell m})\right] \qquad (1)$$

where a, e, I, ω, M, and Ω are the Keplerian elements, θ is the Greenwich Sidereal Time, and S_i is any one of the orbital elements. The order of magnitude of the perturbations $\Delta_{\ell m} S_i$ for Earth satellites is about $\pm$ 5 km or less for zonal harmonics (m = O) and $\pm$ 300 meters or less for nonzonal harmonics ($m \neq O$).

Perturbations of an orbiter about another planet will be similar in character to those of an Earth satellite, but differ in magnitude because of differences in (1) the rate of rotation $\dot{\theta}$, and (2) the magnitude of the coefficient $J_{\ell m}$. To scale the coefficients $J_{\ell m}$, we can use the fact that the principal characteristics limiting their magnitude are the shearing stresses required to support the density irregularities they entail. Such stresses are the result of the mutual gravitational attraction of the density irregularities and the entire mass of the planet, and hence the gravitational term $J_{\ell m}GM/R^2$ should be expected to vary inversely as $g \approx GM/R^2$, or the dimensionless coefficient as $1/g^2$. Hence, assuming comparable strength of materials, larger coefficients should be expected for the smaller bodies the Moon, Mars, and Mercury, and larger perturbations of an orbiter about Venus because of its slower rotation $\dot{\theta}$; however, for the major planets, Jupiter, Saturn, etc., the situation is discouraging because of their large acceleration g and rotation $\dot{\theta}$.

To determine a particular harmonic $J_{\ell m}$, the ideal orbit would be one for which the denominator in Eq. (1) approaches zero: i.e., a resonant effect, in which (since $\dot{\omega}$ and $\dot{\Omega}$ are small)

$$(\ell - 2p+q)\dot{M} \approx m \; \dot{\theta}, \tag{2}$$

or using Kepler's law and replacing (ℓ - 2p+q) by I,

$$a \approx \left[\left(\frac{GM}{a_e^3} \right)^{1/2} \cdot \frac{1}{\dot{\theta}} \; \frac{I}{m} \right]^{2/3} a_e \tag{3}$$

A family of such resonant satellites appears to be the best means of improving knowledge of the Earth's gravitational field after upper atmospheric densities increase with solar activity. However, such a system would be too costly for other bodies. Instead, the principal requirement is for an orbiter high enough to avoid drag effects, but low enough to be sensitive to gravitational variations. The first orbiter should be of high inclination, since orbits of high inclination are the most sensitive to perturbations, just as they are the most unstable; subsequent orbiters should be of differing inclination, to eliminate ambiguities due to different harmonic terms causing perturbations of the same periods.

The appreciable upper atmospheric densities of Mars and Venus make it desirable that orbiters about these planets incorporate the device of a null-type accelerometer: i.e., an outer satellite that is kept centered on an inner satellite by use of gas jets. The orbit can thus be calculated as one affected solely by body forces; the surface forces are measured by the gas expended; and the lifetime of the satellite is prolonged in proportion to the gas supply. Such a device is small, and requires a surprisingly little gas supply. A prototype is being perfected by Cannon and Lange of Stanford, and drag-free satellites should be readily engineered after 1970. Although not essential to prolong lifetime, the drag-free characteristic would also be desirable for interplanetary probes for determining relativistic and other small defects.

The tracking by the Jet Propulsion Laboratory Deep Space Network—radio Doppler at frequencies in excess of 2×10^9 cps—is amply accurate for the foreseeable future. For the orbiter of another planet, however, it would be desirable to have it tracked by transponders or beacons on the surface of the planet, in order to obtain better conditioning of the orbit as well as positions on the surface relative to the center of mass.

The minimum satellite for gravitational purposes could be quite simple: only such stabilization as to point the antenna toward the Earth is required. The accuracy of gas control to be incorporated in the lunar orbiters is adequate for gravitational satellites, if they are also to be used for purposes requiring orientation toward the surface of the planet.

Radar altimeters and gravity gradiometers have been proposed as supplementary instruments for satellites.

The range of accuracy of the altimeter should, of course, be small compared to the variations to be measured by it: e.g., ± 5 meters for the ocean surface, or ± 100 meters for the lunar surface. The present accuracy of tracking the best Earth satellite orbits is about ± 40 meters in the radial direction. However, the results obtained should not be much affected by orbital error, since it is of a considerably different wavelength from the variations in the planetary surface.

The range of accuracy of a gravity gradiometer should be small compared to the average anomalous gradient expected in the gravitational field. In the Earth's field, the rms change in 100 km at the surface is about ± 20 milligals, or $\pm 2 \times 10^{-9}$ sec^{-2} is the average gradient. For a longer smoothing distance or for a satellite at altitude, the gradients will be even smaller. For example, for an altitude of 1000 km, if we assume that only those harmonics are significant that are not damped to less than 0.05 of their surface magnitude (i.e., of wave number less than 20), then the rms gradient is $\pm 5 \times 10^{-11}$ sec^{-2} (based on a power spectrum of surface gravity of about $130/\ell$ milligals^2). Hence the range of accuracy for an Earth satellite gradiometer should be at most $\pm 10^{-11}$ sec^{-2} and, allow for $1/Rg$ scaling, and the possibility of lower altitudes, about $\pm 5 \times 10^{-10}$ sec^{-2} for a lunar satellite gradiometer.

In addition, if satellite photogrammetry is planned—as is necessary to apply effectively the gravitational variations—it should be carried out from a satellite whose orbital perturbations by gravitational variations are accurately calculable.

Any planetary orbiter will furnish an accurate determination of the oblateness, or J_{20} gravitational coefficient. Assuming hydrostatic equilibrium, the moment of inertia of the planet can be calculated. A measure of the error of this assumption is the ratio of nonhydrostatic terms J/m to J_{20}. For the Earth, this ratio is about 1/200. Applying the equal stress scaling $1/g^2$ to J/m, the ratio is about 1/45 for Mars and 1/26,000 for Jupiter. However, the Moon, Venus, and Mercury all rotate so slowly that the hydrostatic assumption is useless, and the moment of inertia must be measured. The moment of inertia is directly involved in the response of the planet to a torque. Since the only torque that is sufficient to move a planet is the gravitational attraction of another celestial body, the differences in moment of inertia appear in the torque, and for example, Euler's equations assume the form for torque about the Z or polar axis.

$$\frac{d}{dt} C\omega = 3 (A - B) \frac{M}{\gamma 5} xy \quad (4)$$

where A, B, C, ω are moments of inertia and rotation of the disturbed planet and M, r, x, y are the mass and coordinates relative to the planet of the disturbing body. The response of a planet not rotating with respect to the disturbing body, such as the Moon, is a libration, the amplitude of which depends on the magnitudes and rates of cyclic changes in the orbit. The principal response of a planet rotating with respect to the disturbing body is a precession proportional to the relative rotation rate times the sine of twice the mutual inclination, I. Since the hydrostatic J_2, or $(A-B)/MR^2$, is proportionate to $\omega^2 R/g$ the quantity $(A-B) M \sin 2 I/r^3 C$ seems hopelessly small, certainly for Venus, and probably for Mercury.

In any case, determination of the moments of inertia for the Moon seems to be feasible by placing laser reflectors or radar transponders on its surface, preferably at three well-distributed points. At present, the moment of inertia obtained by a combination of the libration measurements and the motions of the lunar node and perigee is unreasonably large. Whether the error is in the latter will soon be resolved by a lunar orbiter.

The principal application of planetary probes made thus far has been to improve determination of the masses of the Earth, Moon, Venus, and Mars. The corrections to orbits and masses of the inner solar system made thus far by probes and radar ranging of the planets have generally been on the order of five times the uncertainties ascribed to the old determinations. This ratio is apparently a consequence of too small uncertainties resulting from applying simple least squares and incomplete dynamical theory to inadequately distributed camera observations subject to systematic star catalogue error. However, aside from the previously mentioned oblateness J_{20} of the Moon, there appear to be relatively few of the anomalies in the solar system pertinent to evolution or planetary structure that might be explained, or explained away, by improved data, either from probes or satellites or Earth-based observations. These comprise: 1) Mars' radius and geometrical flattening; 2) the accelerations (and orbital elements in general) of the satellite Deimos of Mars and the five inner satellites of Jupiter; and 3) the orbit of Mercury. 1) might be improved by new telescopic techniques, and certainly would be improved by use of an orbiter or an impacting probe. 2) would benefit from a more careful analysis of observations from Earth, as well as in some cases, orbiting artificial satellites. 3) is currently being improved by radar, and would be most improved (as would any planetary ephemeris) by an orbiter.

Most of the exceptions to the rules in the dynamics of the solar system appear to be well established, and improved theories of planetary constitution or nonelastic planetary interaction or orbital evolution are required to account for them:

Slight retrograde rotation of Venus
Irregularities in the Earth's rotation
Acceleration of Phobos' orbit
Rate of tidal dissipation deduced from the Moon's deceleration
Small eccentricities of the inner satellites of Jupiter and Saturn
High obliquity of Uranus

Intersection of the orbits of Pluto and Neptune
Retrograde resolution of Triton and high eccentricity of Nereid (satellites of Neptune)
Cometary orbit parameters
Gaps in distribution, and clusters near gaps, of mean motions of asteroids and of Saturn's rings
Origin of asteroid orbits of high inclination and eccentricity

As emphasized in "A Review of Space Research" (NAS—NRC Publication 1079), the greatest need to improve understanding of solar system dynamics is theoretical. This need exists because any reasonably satisfying explanation involves considerations of cosmogony and a time scale well beyond conventional computing techniques. Hence, within the bounds of conventional Newtonian mechanics, improvement is needed in solving problems of convergence and boundedness, simultaneous small divisors, capture, stability, integrals of motion, many-body systems, etc. The benefit of such development will be to limit the bounds of the cosmogonical problem, which unavoidably comprises more complex magnetohydrodynamic effects, mechanisms of selective escape, turbulence, and the like.

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CHEMISTRY OF THE SOLAR SYSTEM

P. W. Gast, L. H. Aller, and G. Kullerud

The Sun

It is usually supposed that, except for the elements affected by nuclear transformations, the average chemical composition of the Sun represents the primordial composition of the solar system. The only elements affected by nuclear processes in the outer layers of the Sun are deuterium, lithium, beryllium, and possibly boron. The theory of stellar structure predicts quite definitely that the outermost layers of the Sun are in convective equilibrium but that this convective envelope cannot extend to a depth of greater than about 0.2 of the radius. The transformation of hydrogen into helium occurs only in the innermost layers that are in radiative equilibrium, suffer no large-scale mass motions and do not mix with the surface. The fraction of the solar mass amenable to spectrochemical analysis is extremely small, about 2 grams above each cm^2.

It does not seem likely that the composition of the surface layers has been modified by accretion of other material, nor that they have been much affected by nuclear processes occurring within the surface layers themselves.

First of all, solar abundance data are limited to those elements whose lines can be identified in the solar spectrum and whose intensities can be measured. Furthermore, one must know certain atomic parameters—namely, f-values or transition probabilities and collisional damping constants. The spectroscopic term analysis must be known; one must be sure of energies and term assignments for the levels involved and know the ionization potential of the atom or ion. At the present time, f-values and damping constants are imperfectly known for many elements of great importance.

Additional observations of solar line intensities are required both in the normally accessible regions and in the infrared and ultraviolet. The latter spectral regions can be studied with high dispersion spectrographs flown above the Earth's atmosphere. In the ultraviolet region of the spectrum, blending is so severe that resonance lines of many rare but geochemically important elements are mostly masked by those of more abundant elements, such as iron; consequently an intensive study of such blending effects and a theoretical synthesis of the spectrum will be required.

The results obtained so far show that although there is generally a good agreement with abundances obtained from chondritic meteorites (provided one excludes volatiles like carbon, nitrogen, oxygen, sulfur, etc.) a number of marked discrepancies exist. Iron (for which the data appear to be more reliable than for most other elements) appears depleted in the Sun. Some other elements, possibly lead, are enhanced in abundance in the Sun but most elements, which are presented by few lines, are affected by much larger observational errors than is iron.

Data are particularly uncertain for the second most abundant element in the universe, helium. Not a single line of this plentiful element appears in the dark line spectrum of the Sun! Estimates of the He/H ratio made from spectroscopic studies of solar prominences or deduced from models of the Sun's interior are in disaccord with one another. The one estimate is affected by large deviations from local thermodynamic equilibrium, the other is affected by uncertainties in the theory of stellar structure.

Recommendations for further study of the chemical composition of the Sun are:

(1) Better laboratory data on atomic and molecular spectra and theoretical calculations of transition probabilities and damping constants of spectral lines are urgently needed. Newer experimental techniques such as lifetime measurements of excited levels and the plasma torch, should be exploited as well as older mthods such as the conventional arc,

atomic beam, electric furnace, and shock tube. Advantage should be taken of new advances in quantum mechanical calculations of atomic wave functions and transition probabilities.

(2) Detailed observations of the shapes and intensities of many spectral lines, both in conventional spectral regions available to ground-based instruments and in the infrared and ultraviolet, are indicated. We refer here to the dark absorption lines in the solar spectrum. The emission lines that are observed in the spectra of the chromosphere and corona at the times of solar eclipse, and which dominate the spectrum of the Sun even in integrated light in the far ultraviolet, will provide valuable insights into the structure of the outer solar envelopes. They may shed some light on the abundance problem when the detailed characteristics of these envelopes have been worked out.

(3) An improved knowledge of the structure of the solar photosphere is also required. (The photosphere is the bright surface of the Sun which produces both the continuous spectrum and the dark lines that are superimposed upon it.) Furthermore, we must have a better understanding of the transfer of radiation through the photosphere layers and the formation of spectral lines. We must evaluate to what extent our abundance determinations are affected by deviations from local thermodynamic equilibrium.

For a very modest expenditure of funds, already available observational data could be analyzed by modern methods and many of the necessary atomic parameters could be measured or calculated.

(4) More attention should be paid to isotopic abundances. Unfortunately, the atomic isotope shifts are extremely small; they are measurable for deuterium, helium, and lithium. It has been established that deuterium is much less abundant in the Sun than on the Earth, although it may be produced in flares. Molecules offer a more tractable possibility in that, for example, the $C^{12}N$ lines are substantially displaced from the corresponding $C^{13}N$ lines of the same Cyanogen band. It appears that the C^{12}/C^{13} ratio is probably about the same as on the Earth. Other possibilities may be offered by the OH bands.

(5) It is very important to establish as accurately as possible the chemical composition of the solar wind and of solar cosmic rays. Presumably the solar wind reflects the composition of the corona, while plasma clouds associated with flare activity (e.g., solar cosmic rays, severe magnetic storms) may show the effects of spallation processes.

Does the composition of the corona and of the wind associated with it differ from that of the solar photosphere? Do the chemical compositions of solar cosmic rays differ from event to event?

Because of the great spread in elemental abundances, one recognizes that it will present a real challenge to secure accurate values. In view of the difficulty in establishing accurately the ratio of protons to alpha particles, ratios such as He/O, He/Fe, etc., which are much larger will be very difficult.

The detecting devices must be flown in space platforms that avoid the magnetosphere (where ion collection will be greatly complicated). It is also necessary to avoid the surface of the Moon, where secondary particles, particles from radioactive decay, etc., would complicate the picture.

Composition of the Inner Planets

The average chemical compositions of the inner planets and the Moon will be essential information in any theory of the origin of the solar system. Even if detailed chemical compositions are unobtainable it is very important to know if and how the planets differ in their chemical compositions. In order to determine these differences it will be necessary to know how far the bulk composition of a planet can be inferred from surface observations. Both atmospheric processes and the release of energy from the interior of the planet have made the Earth a very complex chemical system, nevertheless some significant boundary conditions on its composition can be inferred from surface observations. These limitations and the compositions of meteoritic matter are the only factual information available on the chemical composition of the inner planets.

Meteorites and the Sun. A most important source of information on the composition of the solar system is the large number of meteorites that has been collected in the last several hundred years. Even though there is no agreement on their point of origin they are widely accepted as the most nearly primordial material available for direct analysis. The similarity of the relative abundance of many elements in the Sun and in the chondrites is highly significant and strongly supports the primordial nature of some types of meteorites. Recent studies of different types of meteorites have shown: (i) that there is considerable diversity in the composition of these objects; (ii) that one class represented by less than half a dozen known specimens appears to resemble the Sun composition more closely than any of the other types; (iii) that the differences in composition between meteorite groups can be understood in terms of differential volatility or different condensation temperatures of different elements and compounds, and (iv) the primordial nature of chondritic meteorites has been further supported by the discovery of extinct radioactive iodine in some of these objects.

The parallelism in chemical composition of the carbonaceous chondrites and the nonvolatile elements in the Sun suggests that at least some planetary material in the solar system accumulated under conditions that did not involve extensive chemical differentiation.

Planets, particularly the Earth. It has been widely and frequently assumed that the planets have similar over-all compositions to the nonvolatile fraction of the Sun or similar to the chondrites. This assumption is often made even when different densities (uncompressed densities) for the planets are accepted. Even for the Earth there is almost no factual evidence supporting this assumption.

Recent isotopic and geochemical analyses have given some indication, consistent with earlier observations based on planetary densities, that the Earth is not chondritic in composition. The differences are similar to some of the differences observed for chondritic meteorites, i.e., appear to be related to volatility or condensation temperatures. In particular the Earth seems to be depleted in elements such as In, Hg, Cd, Cl, Tl, Pb, relative to the most Sun-like meteorites (Carbonaceous Chondrites Type I). Furthermore, there is strong evidence that Rb and Cs are depleted in the Earth relative to Si by more than a factor of 10. Finally, it appears that higher abundances of some refractory elements, such as U and Th, are required on the Earth as a whole to account for their abundance in the surface regions and for the observed terrestrial heat flow. The latter observation, when combined with the observed abundance of these elements in chondrites, requires a mechanism that enriches these elements by removing some other elements that occur in great abundance (probably Mg and Si). We are left, then, with a planet that may have little resemblence in composition to the chondritic meteorites or the non-volatile fraction of the Sun, and at best we may be able to suggest the direction in which the concentrations of particular elements differ from those of the Sun.

If this is true for the Earth it seems very likely that it may be true for some of the other inner planets. Evidence in support of this supposition comes from the differing densities of the inner planets and the Moon. If the high density of Mercury is validated by further work, it seems quite likely that the bulk composition of this planet will be quite different from that now determined for chondritic meteorites.

It is clear that there are strong reasons for investigating the compositions of all the inner planets and the Moon. Moreover, existing ideas and hypotheses are sufficiently well developed to furnish some rather specific guidelines for exploring planets other than the Earth.

Recommendations

A. We recommend that chemical and mineralogical studies of meteorites continue, at least at the level of the past five years. We also feel that synthetic studies of systems related to the textures and compositions found in meteorites be given further emphasis. Particular attention should be paid to the physical conditions responsible for the observed chemical and mineralogic differences.

B. We feel that exploration of the planets should place nearly the same emphasis on all of the terrestrial planets.

(i) Precise determination of over-all densities and moments of inertia for Mercury, Venus, and Mars should be given very high priority.

(ii) Probes for determining atmospheric compositions and missions using these probes should be scheduled for all of the terrestrial planets.

(iii) Devices for determining the abundance of diagnostic elements in the surface materials of other terrestrial planets need to be developed, e.g., K/U ratio, and K abundance. K/Ca ratio, Fe/Ni ratio, S/Si ratio, Al/Si, etc.

Composition of the Jovian Planets

The outer planets are characterized by large masses, relatively low densities, and strong mass concentrations toward their centers. Spectroscopic observations have identified hydrogen and methane in their atmospheres; ammonia appears in the spectrum of Jupiter. Various authors have constructed models for these objects, using plausible ratios of hydrogen to helium. It is not possible to construct models for Uranus and Neptune unless it is assumed that the ratio of carbon, nitrogen, and oxygen to hydrogen is greater than the solar value. The core of Jupiter is presumed to be of metallic hydrogen. The structure of a given model depends on assumptions about the internal temperature distribution. If the center is presumed hot, a considerably different choice of elemental abundances is obtained.

Ground-based spectroscopic observations give important clues to the temperatures and pressures above the cloud layers. It is possible to examine temperature as a function of latitude and also differences between the morning and evening limbs of the planet.

The most prominent bands in the spectra of these outer planets are those of methane. It should be possible to deduce the C^{12}/C^{13} ratio and establish an upper limit to the deuterium abundance. A pressure-induced quadrupole absorption line of hydrogen has been used to obtain the hydrogen abundance and estimate the C/H ratio.

Recommendation

Much more intensive spectroscopic observations of these objects should be undertaken with ground-based telescopes provided for the purpose. These data should be supplemented by observations secured in the infrared from above the Earth's atmosphere and from balloons. Careful, intensive studies from the Earth are particularly important because the time and cost requirements for probes are so great.

THE SUN IN RELATION TO PLANETARY AND SOLAR SYSTEM INVESTIGATIONS

G. J. F. MacDonald

The following areas of solar researches have recived discussion in relation to problems of the origin of the solar system, or problems related to planetary investigation.

Composition of the Solar Wind. In addition to hydrogen and helium the solar wind should be studied for its content of heavier elements. The abundances so determined may, at times of solar quiet, relate to the corona where some selective evaporation may have taken place. At times of chromospheric outbursts, however, the solar wind composition must represent closely that of the well-mixed outer convection layer of the Sun. (This is not true for the high-energy particles generated in flares, which may well have suffered a selection related to charge or mass.)

The instrumentation to be devised for the purpose may take the form of mass spectrographs or of nuclear emulsion recording - the latter possibly with the use of slits and magnetic fields for excluding the main proton beam and the sunlight. The possibility of spectroscopic investigation of the interplanetary plasma should also be investigated further.

The shape of the Sun. The presence of strong magnetic fields or intense motions in the Sun would be important in the discussion of theories of the origin and evolution of the solar system. The shape of the Sun would indicate the presence of such effects, if it differed significantly from the hydrostatic equilibrium shape.

The terrestrial atmosphere limits the precision of any measurement of solar oblateness. Telescopic observations from above the atmosphere are required, possibly with spinning telescope or shutter and the recording of time-differences between limb crossings of a slit.

History of solar outbursts. The magnitude of solar outbursts is not known to be limited to that of present-day flares. Any possible record of greater intensity outbursts in the past should be investigated. Lunar surface investigations may reveal such a recognizable record. Mars may also contain such information.

Magnetohydrodynamics of the interplanetary plasma. A better understanding of the large scale magnetohydrodynamic flow in the solar system would help to enlighten discussion of the flow in the assumed early solar system gas before and during the formation of the planets. The present processes may provide guidance though not a close analogy.

INTERIORS OF THE INNER PLANETS

J. C. Jamieson, G. H. Sutton,
R.A. Phinney, and S.P. Clark

The problem of determining the interiors of the inner planets is so similar to the case of the Earth that techniques of terrestrial geophysics give us a good starting point in a review.

Importance of Interiors

One wants to know the interior state of the planets to answer questions that relate directly to the origin of the solar system:

1) Total abundances of elements in the solar system and their distributions.

2) Density structure of planets may reflect either major differentiation processes in their evolution or initial conditions. Data on surface geology or interiors of the planets will lead, also, to a better understanding of the importance of various long-duration processes in the evolution of the Earth.

3) The thermal structure of the planets, namely, the distribution of sources, temperature, and heat flow, is an essential parameter in studying the energetics of planetary evolution. Since the thermal picture is relatively decoupled from the density-elasticity variables, any direct thermal data from a planet is *per se* of greatest importance in putting bounds on the energy budgets of the planets.

Geophysical study of the Earth has led to a first-order model in which the elastic wave velocities are given as a function of radius. With one or two special exceptions, it is unlikely that we shall go beyond this one-dimensional model in looking at the interiors of planets in the foreseeable future.

Seismology

Seismology provides the nearest thing to a direct probe of the interior. Given a number of seismic events, one constructs the relation between (angular) distance from source to receiver and the time delay of the first arriving signal; provided the elastic velocities generally increase with depth, one can calculate them from this data. Discontinuities, such as a core boundary, produce propagation effects that are both characteristic and, at the same time, possibly confusing.

Theoretical solution of the elastic-wave boundary-value problem for a layered sphere provides a basis for interpreting the spectrum of a single signal; in most circumstances one still wants to know the source location and time. An optimum recording system for this type of analysis would include three components of motion in the passband 0 + to 1.0 cycles/second. The signals for spectrum analysis include various types of surface waves and body wave groups, with various degrees of dependence on the different depth regions. In the best of circumstances, one or two large events could give us the entire velocity structure of the planet. In practice, one must record (on Earth) about 100 events to find the one or two high-information signals.

Any seismic experiment on a planet requires that:

1) Of the order of 10-100 events should be recorded. Given the frquency of earthquakes on Earth, this would take about 6 months of recording, from which one needs about 1 hour of data per event.

2) Sources can be located in space and time. If there are enough events a single station may suffice, but information rate is greatly increased with a net of three or more instrument locations because of the greater ease of source location.

Wind-induced noise may be a serious problem. We are ground-noise limited on the Earth over much of the frequency band of interest. A much higher noise level would seriously restrict our rate of information retrieval.

Density

Deduction of the density structure is an essential prerequisite for extrapolating surface chemical data to the composition at depth. Thermodynamics gives us a differential equation by which we can extrapolate the surface density downward, using the elastic velocities as parameters. Certain ambiguities in the method can be reduced by the necessary constraints of fitting the mass, radius, and moment of inertia. These parameters are therefore the most important, since they provide significant information at every stage of the seismic investigation.

Composition

The Earth has provided a case study in the deduction of composition from density and elastic velocity. Increasing confidence in the class of models is being achieved by direct measurements of the equations of state of terrestrial materials at kilobar and megabar pressures. We are limited by a severe nonuniqueness in the inference of composition and will perhaps never be able to verify directly our latest model. Most important to study of the other planets will be high-pressure studies on supposed planetary materials.

Gravity Perturbations

Low- and high-order global constraints on the density and rigidity distribution in a planet are obtained by measuring dynamical perturbations (1) of a planet on orbiting satellites, and (2) of a solar or lunar gravitational field on the rotation and figure of the planet.

Class (1) experiments can be arranged by planning satellite orbits and tracking. Class (2) experiments are not under our control and we must be content with the available perturbing bodies. The great importance of moment of inertia requires that we must attempt the relevant precession measurement [Class (2)]. For Mercury, Venus, and Mars, the principal perturbing torque is solar.

Thermal Methods

No method comparable to seismology exists for inferring temperatures at depth. We are constrained to making surface measurements of heat flow and concentration of radioactive heat sources. First-order extrapolation of the thermal gradient downward depends on tenuous assumptions regarding the degree of upward concentration of sources. In the Earth, the temperature at 100 km is uncertain by 200°K and the temperature at the core boundary is considered to be uncertain in the range between 1800°K to 5000+°K. Despite the limitation of our models to about 1 decimal place, the importance of knowing the present heat flow on each planet overrides the uncertainty due to technique, sampling problems, or lack of uniqueness.

Present heat flow measurements involving measuring temperature and thermal conductivity at various depths in a hole that extends below the surface layer subject to diurnal and annual heating. (The problem of longer-period random variations is, of necessity, ignored.) Surface gradients are typically 2-8°C/km. No proven alternative method exists; it will probably be necessary to drill a hole to make a measurement.

Electrical Conductivity and Temperature

Indirect determination of temperature may be possible through knowledge of the composition and electrical conductivity. The latter may be determined by synoptic analysis of the secular variation of the magnetic field (global—deep mantle) or by cross-spectrum analysis of horizontal components of the time-varying E and M fields from external (ionospheric or solar wind) sources, at a single site. This method has not received sufficient attention in terrestrial problems to assess its immediate practicability for planetary exploration. It is felt that, since it is the only short-wavelength geophysical probe other than elastic waves, it deserves further attention and development. The existence of oceans and water-saturated surface layers has complicated the interpretation of measurements on the Earth; in the absence of such conductive surface layers it is clear that the electrical conductivity-depth function is closely correlated with temperature.

Magnetic Field Measurements

It is generally considered that an internally generated magnetic field is due to magneto-fluid coupling in a convective region of the planet. For the inner planets one would assume that this implies a liquid core. Since the existence or nonexistence of a liquid core is such a basic question, early probes to a given planet normally include a magnetometer. It is assumed that this situation will continue.

The spectrum of changes in various spherical harmonics of the internally generated field is determined by the screening effect of the most conductive part of the mantle. For the Earth, the relevant frequencies are about 0.05 to 10 cycles per year.

Small-Scale Variations

Near-surface variations in density or magnetization are normally studied by occupying stations on a grid with a gravimeter or magnetometer and interpreting (nonuniquely) the anomaly map. This scale of investigation does not appear to be relevant for a first-order examination of a planet, especially since measurements cannot practically be made from a satellite.

Velocity-depth relations at shallow depths (0.1 to 50 km) are made on Earth by active seismic experiments in which the source of elastic energy is artificial. Active seismology must be ranked fairly low in priority, due to the mechanical complexity, unless no natural seismic sources are present. In that case it will be necessary to re-examine the importance of seismic exploration of a planet.

Short List of Priorities

1. Mass, Radius, Moment of Inertia
2. Dipole Magnetic Field
3. Surface and Atmospheric Investigation (especially composition)
4. Passive Seismic Experiment. Heat Flow
5. All Others

INTERIORS OF THE MAJOR PLANETS

George B. Field

The low densities of the four major planets, ranging from 0.68 for Saturn to 2.25 for Neptune, point to appreciable light-element contents. A recent theoretical study by Peebles (1964) of the interiors of Jupiter and Saturn shows that the observed radii and gravitational moments J and K can be satisfied with the composition, 80% hydrogen, 18% helium, and 2% heavier elements by mass, provided the material is homogeneously distributed outside a core of dense material, and that both planets have deep convective atmospheres.

The comparison of the models with observation determines the hydrogen-helium ratio but not the heavy-element abundance. The helium to hydrogen ratio, 1 to 20 by number, is consistent with recent estimates for the Sun based on models of the solar interior. Direct spectroscopic observations of Jupiter (Spinrad and Trafton, 1963) suggest a ratio of hydrogen to carbon equal to 400 by number, compared to the solar ratio of 1900.

Several phenomena point to the correctness of the assumption of a deep convective atmosphere: the equatorial acceleration of atmospheric features, the suspected presence of planetary heat which exceeds insolation (Öpik, 1962), the existence of a 50-gauss magnetic field on Jupiter which would require internal energy sources for its regeneration, and the observation by Murray et al. (1964) of anomalous infrared emission by the area of Jupiter's disk shadowed by the satellites.

The chemical composition of the major planets is important in that it may be closer to that of the cloud out of which the solar system formed than that of the inner planets. A direct sample of the atmosphere of Jupiter would yield abundances of hydrogen, deuterium, helium, and carbon, which could be compared with solar system cosmic-ray data for the helium to carbon ratio and solar model calculations on hydrogen to helium. Since helium is very difficult to observe spectroscopically, this would yield an important datum concerning the material out of which the solar system formed. It is believed that any fractionation in hydrogen relative to helium would have increased helium to hydrogen in the atmosphere (but see Öpik, 1962), so that such an atmospheric determination would yield a firm upper limit on helium to hydrogen throughout the planet and perhaps in the solar nebula. This will be of interest to cosmogony. Similarly, confirmation of the high carbon to hydrogen ratio would suggest selective evaporation during the formation of the planet which would constrain theoretical models.

Infrared observations from space vehicles will doubtless provide more data on the planetary heat of Jupiter and Saturn long before probes reach those planets, but there appears to be no substitute for an infrared flyby to observe the dark sides of these planets, since the range of phase angles accessible from Earth is limited. Öpik (1962) has suggested an internally generated heat flux comparable to insolation. This would call for a powerful internal energy source—some 100 times that of the Earth, per unit mass. If confirmed, this would point to an extraordinary degree of radioactivity in the relatively small proportion of heavy elements contained by the planets, or to storage of the gravitational energy of accretion, either of which would throw light on the processes involved in the formation of the planets.

Deuterium may be present in the same proportion as on the Earth (1/6400) or in interstellar matter (<1/15,000). Present spectroscopic limits (Owen, 1965) are <1/1300 (uncertain) and probably cannot be extended much further. Which alternative is correct will throw light on the origin of deuterium in the solar system, and hence on the particle fluxes in the early history of the system.

The magnetic field configuration of Jupiter promises to be spectacular. Present indications from the radio emission are that the field about three radii from the planet is approximately that of a dipole, tilted about 10° to the rotation axis and centered about half a radius from the center of the planet. Interior models suggest a composition of impure metallic hydrogen (a fairly good electrical conductor) out to about 0.8 of the radius, which could support the electrical currents generating the field. The decay time to Joule dissipation does not exceed 10^7 years (Field, 1965), however, and a dynamo must operate, presumably drawing on the same energy sources as drive atmospheric convection. There is some indication of a magnetic cycle like that of the Sun, in which internal differential rotation leads to slippage between the magnetic and non-magnetic domains. The Red Spot acceleration may be connected with these phenomena (Hide, 1963). If these phenomena are confirmed by detailed in situ mapping of the field we may have additional data about convection and shear in the interior—of importance for the thermal history of the planet and for fundamental understanding of hydromagnetic dynamos generally.

The apparent absence of a strong magnetic field on Saturn has uncertain significance. As yet no observers have reported the tell-tale nonthermal radio emission such as originates in the radiation belts of Jupiter. While this could be due to a small magnetic field, it could also be because Saturn's rings, which extend from 1.2 to 2.3 radii, sweep out charged particles that may be present, or because the solar wind, which energizes Jupiter's belts, does not reach Saturn (9.5 AU). Again, there is no substitute for direct measurements of the field.

Uranus (19 AU) and Neptune (30 AU) are much less understood because of their small angular sizes. For example, measurements of thermal emission by the atmosphere or nonthermal emission by radiation belts have not yet been obtained by radio astronomy.

It is well known (Wildt, 1961) that both planets are ten times denser than pure hydrogen configurations of the same mass. De Marcus and Reynolds (1963) have constructed interior models on the assumption that elements heavier than helium are in cosmic proportions, while the abundances of hydrogen and helium are introduced as free parameters. It was found that Uranus cannot contain more than 23% hydrogen, nor Neptune more than 14%. Uranus could be almost pure helium, although it could also be an appropriate mixture of hydrogen, helium, and heavier elements. Neptune, on the other hand, must be at least 36% heavier elements. These conclusions point to substantial differences in the way the Jupiter-Saturn and Uranus-Neptune pairs formed. It remains to be seen whether these differences sprang from thermal or chemical differences in the solar nebula.

The above considerations suggest that the interiors of the major planets are of particular interest because of their chemical composition. Relevant measurements on Jupiter include: visual and infrared spectroscopy to obtain atmospheric composition, far infrared observations to obtain the planetary heat (if large), and studies of the magnetic field structure, including temporal behavior. The only thing inhibiting the first activity on the ground is availability of large telescopes. The second should be pursued from the ground and Earth-orbiting space vehicles, but an infrared fly-by would be of great interest. The last is being studied indirectly by ground-based radio astronomy. However, the interpretation of such data is treacherous, and a regular fields-and-particles satellite orbiting Jupiter would permit detailed field maps which might indicate short period (a few years) changes which would be of great interest for the study of the interior. Moreover, such a satellite might throw light on the acceleration processes in radiation belts, particularly inasmuch as Jupiter's radio emission is affected by its satellite Io.

Penetration of the atmosphere for mass spectroscopy and thermal measurements in the deeper layers would be of very great interest. It seems that breaking a probe from the parabolic velocity of 61 km/sec would pose tremendous problems, however.

The Red Spot could be observed in a variety of ways over a long period from a close satellite. The possible connection of its hydrodynamics with that of the interior has been emphasized by Hide (1963). An instrument designed to do this would also yield much information about the general meteorological features of the planet and would no doubt reflect the presence of internal heat sources if they were comparable with insolation.

There seems to be no qualitative difference in the experiments to be assigned to Saturn, Uranus, and Neptune. Distance will obviously make them more difficult, and our ignorance makes surprises seem more likely.

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PLANETARY MAGNETIC FIELDS

Raymond Hide

Introduction

The evidence for significant magnetic fields of internal origin is compelling for only three objects in the solar system - the Sun, the Earth, and Jupiter.* Since this paper is concerned only with the planets, solar magnetism will not be discussed here.

Although the first attempts to explain the Earth's main magnetic field go back a century or more, only in the past twenty years have important theoretical advances been made. As a result, though the details are lacking it is now generally accepted that hydromagnetic (magnetohydrodynamic) processes in the Earth's liquid, metallic core are responsible for the Earth's magnetism.

That no completely acceptable theory of the Earth's magnetism has been proposed is the direct consequence of (a) incomplete knowledge of the Earth's deep interior, (b) incomplete knowledge of the behavior of the Earth's magnetic field in the past (in spite of great progress in paleomagnetism during the past decade), and (c) mathematical difficulties typical of all realistic studies in hydrodynamics and hydromagnetics. Hence, while the above-mentioned theoretical advances have led to insight into hydromagnetic processes in the core, the subject is still mainly qualitative, despite any superficial appearance to the contrary.

This working paper will (a) sketch our present knowledge of the Earth's main magnetic field, (b) outline modern theories of the origin of the Earth's field, and (c) discuss briefly the circumstantial evidence suggesting that some of the other planets possess magnetic fields of their own.

The Earth's Main Magnetic Field

Description. The present magnetic field at the surface of the Earth may be broken down into two parts: the main field, of internal origin, and the small (less than one percent of the total field) remaining part, of external origin. The external part, which is subject to changes on time-scales ranging from fractions of seconds (subacoustic oscillations) to several days (magnetic storms), is due to electric currents flowing in regions well above the Earth's surface, in the ionosphere and beyond. This part will not be considered further here. The main field, as we shall see, is probably due to electric currents flowing at great depths within the Earth, in the liquid metallic core, which lies between 1400 and 3470 km from the Earth's center. (The radius of the Earth is 6370 km.) Variations in the main field occur on time-scales ranging from a few years upwards.

Observatory data, on which most of our knowledge of the main field is

* This paper was written in June 1965, before the Mariner IV space probe magnetometer measured the magnetic field strength near Mars; see, however, footnote on page 86.

based, go back only a few hundred years (i.e., 10^{-7} of the accepted age of the Earth). These data suggest the following (Fleming, 1939; Chapman and Bartels, 1940; Vestine et al., 1947; Runcorn, 1956; Hide and Roberts, 1961; and Jacobs, 1963):

(1) The main field is predominantly that of a hypothetical axial (i.e., parallel to the rotation axis of the Earth), centered magnetic dipole with a moment of 8×10^{25} emu, giving a surface field of about 0.5 gauss.

(2) It also possesses small but significant equatorial dipole and nondipole components.

(3) The main field undergoes secular changes with the following properties:

(a) they are mainly nondipolar in character;

(b) their rms amplitude is 5×10^{-4} gauss/year, with maximum amplitude about 1.5×10^{-3} gauss/year, (the corresponding dipole changes being of the order of 3.5×10^{-4} gauss/year);

(c) on typical magnetic maps lines of equal annual change of any element (isopors) form a series of sets of oval curves surrounding points at which the changes are most rapid (isoporic foci);

(d) at any epoch, the sets of isopors cover areas of continental size, and are separated by regions over which changes are small;

(e) isoporic foci migrate westward (as does the field itself) at a fraction of a degree of longitude per year (Bullard et al., 1950);

(f) in addition to the westward drift (e), the pattern of the secular variation field may alter considerably in a few decades;

(g) compared with the world-wide average, the secular changes are systematically low in the Pacific hemisphere and high in the Antarctic.

Paleomagnetic studies based on measurements of the magnetic properties of igneous and sedimentary rocks and of human artifacts (see review by Irving, 1964) have extended our knowledge of the main field as far back into the geological past as the pre-Cambrian (10^9 years ago). According to these studies:

(4) The main field has been largely that of an axial centered dipole for the past 5×10^7 years (during the quaternary and tertiary periods).

(5) If the field was that of a centered dipole prior to the tertiary, the angle between the dipole axis and the present geographic axis may have varied more or less systematically by large amounts (up to 90°).

(6) The polarity of the (assumed) dipole may have reversed several hundred times since the pre-Cambrian (Cox et al., 1964; Ade-Hall, 1964; Blackett, 1965).

(7) The intensity of the dipole has not changed much in the past 5000 years.

(8) 10,000 years ago, the time-scales associated with the secular variation (see property 3 above) were similar to those revealed by observatory data.

(9) The secular changes over the Pacific hemisphere have been weaker than the world-wide average for the past 10^6 years (Cox and Doell, 1964) [cf. property 3(g) above].

The magnetic field within the Earth. The poloidal field within the Earth cannot be determined uniquely from the field at the surface without making further physical hypotheses. Moreover, lines of force of any toroidal magnetic fields inside the Earth cannot, by definition, penetrate to the surface. Modern theories of the main geomagnetic field invoke toroidal fields of several hundred oersted within the Earth's liquid core (see below).

Theories. The main geomagnetic field could in principle be due to:

(a) permanent magnetism,
(b) the rotation of an electrostatically charged Earth,
(c) ordinary electric currents flowing within the Earth,
(d) some new physical property of gravitating bodies.

Theories under categories (a) and (b) are mainly of historical interest only. In addition to the qualitative difficulties of accounting solely in terms of these processes for a surface magnetic field with the spatial and temporal variations observed, there are serious quantitative difficulties. Our knowledge of the properties of materials at very high pressures is still very poor; we cannot, for example, be sure that the temperature of the central body of the Earth everywhere exceeds its Curie point (Weiss, 1963).

Theories under category (d) are hard to discuss briefly, for obvious reasons. Attempts to account for cosmic phenomena in terms of new effects that would be very difficult, or even impossible, to measure in the laboratory are not uncommon. The most recent attempt of this kind to account for the Earth's magnetism is that by Blackett (see review by Runcorn, 1956), which was subsequently disproved, by delicate laboratory experiments carried out by Blackett himself, and by a geophysical test involving the determination of the radial variation of the Earth's magnetic field from measurements made in deep coal mines.

Nowadays, most geophysicists believe that ordinary electric currents inside the Earth [category (c)] are largely responsible for the main geomagnetic field and its secular variation. That these currents flow mainly in the liquid metallic core is shown by the short (geologically speaking) time-scale of the secular variation and the high electrical conductivity ($10^5\ \Omega^{-1}m^{-1}$) of the liquid core as compared with that of the surrounding solid mantle (less than $10^3\ \Omega^{-1}m^{-1}$).

Because the time of free decay of currents in the core is only 10^5 years, electromotive forces (emf's) must exist in the core that are capable of sustaining, against ohmic losses, the electric currents responsible for the geomagnetic field. These emf's could be due to:

(a) thermoelectric effects,
(b) chemical effects,
(c) motional induction.

Lack of detailed knowledge of the Earth's deep interior precludes serious consideration of possibilities (a) and (b). However, it may be shown that fluid motions in the Earth's core of the magnitude suggested by the geomagnetic secular variation could, by motional induction, interact with the magnetic fields there to produce electric currents of the strength required to sustain the magnetic fields present. This is the rough quantitative basis of the so-called "homogeneous dynamo theories," first introduced by Bullard and Elsasser, on which there is now an extensive literature (see review by Hide and Roberts, 1961).

If a self-maintaining dynamo is to occur in the core, the system of fluid motions there must be quite complicated, and characterized by a very low degree of symmetry. The interaction of horizontal motions with a dipole magnetic field leads to a toroidal magnetic field. Interaction of vertical motions with these toroidal and poloidal fields then leads to the regeneration of the original dipole field. With such a system either direction of dipole field could be maintained. For a given toroidal magnetic field the dipole field depends mainly on the particular form of the velocity field; reversal of the external dipole field may involve comparatively minor changes in the magnetic field and the field of fluid motion in the core.

A quantitative requirement for dynamo action is that a magnetic Reynolds number,

$$R \equiv \mu L \sigma U \tag{1}$$

should exceed about 10, where μ and σ denote, respectively, the magnetic permeability and the electrical conductivity of the core, and L is a length scale associated with the fluid motions, typical speeds of which (relative to the rotating Earth) are of order U. Taking $(\mu\sigma)^{-1} = 3\ m^2/sec$, $L = 3 \times 10^6$ m and $U \sim 10^{-4}$ m/sec for the core, $R \sim 1000$, which should suffice for dynamo action.

The secular variation is due, presumably, to fluctuations in the hydromagnetic flow in the core. The extent to which these fluctuations involve mass motions rather than hydromagnetic wave motions has not yet been ascertained. Most theoretical work has been based on models involving mass motions (see review by Hide and Roberts, 1961). However, one recent study suggests that if the toroidal magnetic field in the core is about 100 oersted, free hydromagnetic oscillations there could account for the observed geomagnetic secular variation (Hide, 1964, 1965). According to the free oscillations theory, the westward drift of the magnetic field at the Earth's surface is due to a slow westward-propagating wave in the core. Associated with this wave would be an eastward-propagating wave moving so rapidly that the electrical conductivity of the Earth's mantle, though weak, is sufficient to suppress from the magnetic record any direct manifestation of its presence. The same theory also predicts that in a sufficiently thin fluid layer, the slow wave would probably move eastward, a result that may be of interest in the study of the magnetic fields of other planets.

Hydromagnetics of the Earth's core. The magnetic energy of the Earth far exceeds the kinetic energy of core motions. The source of energy responsible for these motions must be capable of overcoming dissipative agencies, the most important of which is ohmic heating, amounting to 4×10^9 watts for the whole core.

The nature of the energy source has not yet been established. According to several recent discussions of the problem, radioactive heating within the Earth, the Earth's precessional motion (which is due largely to the action of the Moon upon the equatorial bulge), and gravitational energy released if the Earth is still condensing, may contribute significantly to the stirring of the core (Bullard, 1949; Elsasser, 1950; Hide, 1956; Verhoogen, 1961; Malkus, 1963; Toomre, 1965).

The transmission of magnetic energy between different parts of the core is due mainly to hydromagnetic waves, diffusive processes being entirely negligible. The Earth's rotation makes these waves highly dispersive (Hide and Roberts, 1962; Hide, 1964).

Owing to their low speed, hydrodynamical motions in the core are very strongly influenced by the Earth's rotation, the axial nature of the dipole field being almost certainly a direct consequence of the action of Coriolis forces on core motions (Elsasser, 1950; Bullard and Gellman, 1954; Hide and Roberts, 1961; Hide, 1964). The most obvious role of Coriolis forces is the alignment of core eddies by gyroscopic action, giving the magnetic field rough symmetry about the axis of rotation of the plant. Less obvious, but probably at least equally important, is the role Coriolis forces play in ensuring that, irrespective of the size of the energy-producing eddies in the core, the system can produce the large eddies required to give the low degree of symmetry necessary for efficient dynamo action to occur. These large eddies gain kinetic energy by nonlinear interactions with smaller eddies, a process which, though impossible in isotropic homogeneous turbulence (where energy cascades in the opposite direction, from large to small eddies), is probably quite common in more realistic systems, where isotropy is the exception rather than the rule.

Magnetic Fields of Other Planets

Three kinds of observational evidence from which the existence of a planetary magnetic field may be inferred are:

(a) space-probe magnetometer measurements,

(b) nonthermal electromagnetic radiation from the planet with appropriate frequency, polarization, and intensity characteristics, and

(c) evidence that in modulating the solar wind the effective planetary cross section exceeds the cross section of the visible planet.

For most planets this evidence has not yet been sought; planets in this category will simply be designated "unknown" in the summary to follow. [Kern and Vestine (1963), who have recently written a useful review of certain aspects of planetary magnetic fields, present a table containing estimates of the surface magnetic fields of all the planets and several satellites. In preparing the present paper, however, it seemed preferable to avoid making speculative estimates, since one of the purposes of this paper is to expose areas of ignorance.]

Mercury. Unknown.

Venus. Magnetometer data, obtained as Mariner II flew past the planet at a distance from its center of about 7 planetary radii, gave no evidence of a Venusian magnetic field at any point on the trajectory (Smith et al., 1963, 1965). The upper limit thus set on the (hypothetical) magnetic dipole moment is less than 8 x 10^{24} emu, only 10% that of the Earth. The field may, of course, be highly nondipolar in character.

There is no evidence for electromagnetic radiation of nonthermal origin, observations having been made at wavelengths up to 11 meters (Smith and Carr, 1964).

Mars. Mariner IV will fly past Mars on next July 14, 1965 (see above). There is no evidence of nonthermal radiation from Mars (see Smith and Carr, 1964; Davies and Williams, 1965).*

Jupiter. Nonthermal electromagnetic radiation on decimeter and decameter wavelengths is so strong that Jupiter is one of the brightest radio sources in the sky. Since this was first discovered ten years ago by Burke and Franklin, numerous papers, both observational and theoretical, have appeared on the subject (for reviews see Burke, 1961; Gallet, 1961; Roberts, 1963; Franklin, 1964; Warwick, 1964, 1965a; Smith and Carr, 1964; also Field, 1960; Warwick, 1963; Ellis and McCulloch, 1963). Spectrum and polarization studies indicate that electrons in the radiation belts of Jupiter may be 10^3 more energetic than those in the Earth's radiation belts, and that Jupiter's magnetic moment is about 8 x 10^{30} emu, 10^5 times that of the Earth, and inclined about 11° from the rotation axis. Asymmetries in the total radiation, together with dynamic spectra of radiation bursts on decameter wavelengths, suggest that the magnetic field is much more complicated than that of a centered dipole (Warwick, 1963, 1964).

Radio-astronomical observations indicate a definite rotation period for the magnetic field. (This has led to the introduction of System III, with a period of 9h 55m 29s.37, for convenience in measuring the longitude of radio sources on Jupiter.) The motion of the magnetic field corresponds, presumably, to the motion of material within the planet at the lowest depth at which the magnetic Reynolds number [see Eq. (1) above] exceeds about 10. Though the radio period was apparently constant when first determined, changes have now been detected (Gallet, 1961; Douglas and Smith, 1963). The reconciliation of these changes with the motion of other internal parts of the planet, as evinced by the excursions in longitude of the Great Red Spot, is a fascinating problem (see Hide, 1961, 1963; Warwick, 1964). As Warwick has remarked, "...this is the first instance in astronomy when the distribution of angular momentum within a rotating cosmic body (perhaps even including the Earth) manifests itself in observational effects measurable within a short time-scale." The theoretical exploitation of these observations will bear both on the internal structure of Jupiter and on the origin of its magnetic field.

The possibility that a homogeneous dynamo mechanism is operating in the lower reaches of Jupiter's atmosphere is not inconsistent with the (admittedly inadequate) theoretical and observational evidence. If this is

* Magnetometer data, obtained as Mariner IV flew past Mars at a distance from its center of about two planetary radii, gave no evidence of a Martian magnetic field at any point on its trajectory.

indeed the case, variations in the motions of the Red Spot and of the radio sources could be manifestations of hydromagnetic torsional oscillations of the planet. Moreover, the "topographical feature" to which the Red Spot may be due (Hide, 1961) might be magnetic in nature (Dicke, unpublished).

Saturn. There have been several recent reports of weak and fleeting radio bursts on decameter wavelengths from Saturn. If these weak bursts are real, they are far less frequent and much less intense than those on Jupiter (Smith and Carr, 1964). The equivalent black body temperatures of Saturn at wavelengths of 9.4 cm and 21.2 cm are 180°K and about 300°K, respectively (Rose, 1965; Davies and Williams, 1965), suggesting that weak nonthermal emission may arise at larger wavelengths.

Uranus. Unknown.

Neptune. Unknown.

Pluto. Unknown.

Satellites. The Lunik II magnetometer showed that the surface field of the Moon may be less than 10^{-3} gauss, the corresponding magnetic moment being 2×10^{21} emu, some 2×10^{-5} that of the Earth (see Smith et al., 1963).

Last year Bigg (1964) reported strong modulation of Jupiter's decametric emission by the innermost Galilean satellite, Io, suggesting that Io affects Jupiter's radiation belts (Warwick, 1965b). Ganymede evidently produces similar effects(Lebo et al., 1965). Whether or not the quantitative interpretation of these discoveries will require Io to possess a magnetic field of its own has not yet been ascertained.

Some Additional Remarks

When the solar wind encounters a magnetic body in the solar system, a bow wave and a stand-off shock wave are formed at distances that depend on the momentum of the solar wind and the strength of the magnetic field with which it interacts. These distances may be many times the dimensions of the body itself. On the other hand, if the body has no magnetic field of internal origin (e.g., the Moon), as T. Gold has pointed out, the solar wind, together with its associated magnetic field, would reach the surface of the body. Any magnetic properties of the body would be due to induced eddy currents resulting from this interaction. The investigation of these properties might elucidate the history of the solar wind and thus provide information of the greatest importance in solar physics and in the study of the solar system as a whole (Gold, private communication).

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GEOLOGY OF PLANETARY SURFACES

D. U. Wise and J. W. Salisbury

Introduction

Intellectual curiosity is reason enough for looking to the geology of other planetary surfaces, but there are also some very practical reasons bearing back on the Earth. The surface geology and landforms of the Earth are dominated by a dynamic balance between the constructional forces of tectonic and volcanic activity versus the destructive agents tending to return the Earth's surface to sea level either by erosion or deposition. These destructive processes, mostly involving water in the form of oceans, rivers, or glaciers, operate very rapidly in terms of the age of the Earth. They quickly modify the tectonically produced landforms and obscure their fundamental patterns, shifting materials to new areas where tectonic changes will again deform them. The result is a steady tectonic plowing of the Earth's surface, progressively destroying evidence of older events and precluding hope of ever finding the primordial surface of our globe or the early deformational and chemical patterns that developed on it.

On adjacent terrestrial planets, with little water present to drive the surface changes so rapidly, we have a much better opportunity to see the early crustal features, or the present fundamental tectonic elements, unobscured by oceanic sediments or the blanket of oceans that covers two-thirds of our planet. These adjacent planets should have many of the same internal tectonic driving forces as our own, but a surface whose type and rate of change is dominated by atmospheric erosion, transport, and sedimentation. Consequently, we can begin to study the isostatic adjustments of a body whose surface is not so closely tied to sea level; a body on which the pure end product of arid region landforms may develop; a body on which we can ask whether there were raft-like masses of primitive proto-continents; or where we may examine a geosynclinal downbuckle in which the trough is not immediately inundated by water and sediments.

Water also dominates much of the Earth's surficial and near-surficial geochemical development. On another terrestrial planet without abundant surface water, we can begin to ask: how much chemical fractionation on Earth is the result of internal versus external processes? must mature sediment be formed and metamorphosed to create granitic continents? what happens in the metamorphism of sediments without abundant water?

Thus, the examination of the surficial geology of other planetary surfaces will permit us to see the Earth as part of a spectrum of planetary surface environments, to examine the early history and development of these environments more fully, to separate from other geological processes the features of the Earth's surface environment that are the result of our apparently unique water-dominated environment, and to understand more clearly the deeper planetary processes now largely camouflaged in the geological evolution of our own planet. On the other hand, the Moon, or a planet like Mercury, with little or no atmosphere, may provide information on geological effects of cosmological processes relatively unknown on Earth.

Geological Problems

Understanding the geology of a planetary surface involves integration of a vast number of complexly interrelated facts, which must first be sifted from an even more vast amount of redundant or superfluous information. Ideally, these data should be collected in some orderly way that makes maximum use of our most efficient techniques. Certainly, a duplication of the fumbling history of development of geological investigation on Earth would be a poor model for approaching the problem. Among the best models for thinking of the requirements and sequence of steps is the method we would now use to understand the geology of the Earth's ocean basins: that is, the geology of two-thirds of a planet involving a comparatively new and inaccessible environment. The sequence of priority for reasonable completion of different classes of problems on other planetary surfaces, as presented in the following sections, is somewhat similar to this oceanographic model.

Geographic and Spatial Control. A prime and early requirement is regional map control to define the major physiographic and geologic provinces and to provide the basis for future detailed maps and proper location of sampling sites. The present broad base of development of remote sensing devices gives promise of yielding much more than simple aerial photography of the planet, although these photographs will be an important component of the data. Gamma-radiation mapping will delimit distribution of potassium-40, uranium, and thorium nuclides to separate areas of basic from acid rocks; ultraviolet mapping can produce some elemental analysis of surface materials; infrared maps will provide maps related to thermal regions, surface roughness and some chemical parameters; radar maps will indicate distribution of smooth surfaces, possibly of surface layer thicknesses, or particle sizes, and provide also general topographic and fracture-orientation data. With the exceptions of atmospheric windows, the greater the mass of planetary atmosphere, the more closely constrained to longer wavelengths will be the measurements made from orbital altitude. For instance, Venus will be limited almost entirely to longer wavelengths; Mars will be similar to the Earth but not quite so severely limited in its atmospheric constraints; Mercury, the Moon, and other moons can be examined with the full spectrum from gamma rays to radar. The possibilities of these sensors are broad enough to permit some form of geologic mapping from orbit of all the inner planets and their satellites. For Venus, the attenuation problem will restrict the quality of geologic mapping to that obtainable from radar, although use of several wavelengths and of polarization will permit definition of some geologic units.

The point of the above discussion is that both geographic, topographic, and a variety of crude (by Earth standards) geologic maps of the planets will be possible from orbital altitude in addition to detailed maps of some selected areas. The limitations will be those of power supply both for any active illumination (probably radar) that is done as well as for the transmission of the huge volume of data back to Earth. Nevertheless the production of a reconnaissance geologic map of the surfaces of the inner planets with definition of the major provinces and a more detailed map-sample of a small area within each of these provinces is the major initial step in the geological study of their surfaces.

Sample and Site Examination. Once the geologic provinces have been defined from orbital sensing or concurrently with that mapping, sites must be visited to define chemical and physical properties of the surface, of a province or the boundaries of two provinces or of given geological units. This work would presumably be done with soft landing vehicles somewhat like Surveyor and would carry other experiments of a meteorological, geophysical, and biological nature.

Process Studies. The data from remote sensing, mapping, and landing-site examination will not be fully interpretable until some understanding of the dominant surficial processes of the planet is obtained. Largely this will result from the combination of regional image patterns with detailed site knowledge. A good example of the difficulties in interpretation without knowledge of dominant processes is the present state of the interpretation of lunar photos. Admittedly, more remote sensing of the Moon would help, but landing vehicles are necessary to get some feeling for the surface before interpretation is on any sure footing. Many surficial processes operate periodically so that a long life expectancy would be most desirable for the landing vehicles, in effect making them long-term weather stations for the planetary surface.

The first remote sensing or detailed photographs will indicate by the visible landforms the relative efficiency of constructional tectonic processes versus destructional erosive processes just as simple photographs of the Moon indicate a dominance of constructional craters and maria with relatively ineffectual destruction of them.

The constructional tectonic landforms should be more readily understandable on the basis of terrestrial familiarity. Volcanoes, their debris aprons, volcanic chains, and the associated zones of weakness, if present, should be detectable and interpreted. Fracture patterns and lineaments on the planet as a whole should be relatively easily mapped and for some classes may be readily interpreted. For instance, major strike-slip fault zones such as the San Andreas would offset structural elements. They would indicate deeper lateral shiftings beneath the planetary crust and by their over-all pattern could give evidence of whether differential zonal rotation of the planetary crust drives them or whether a less uniform convective overturn is operating in the planetary interior. Any system of global fracturing like that connecting the mid-oceanic rises and the African Rift Valleys on Earth would be quickly detected and checked against maps of volcanic activity or infrared thermal anomalies for associated features that might indicate the driving forces of the deformation.

The destructional processes, with their interrelated sedimentary and geochemical side effects, may be much more difficult to understand. In the complete absence of an atmosphere, as on Mercury, the surficial destructional processes will operate extremely slowly and many of the ideas currently being developed concerning slow surficial changes on the Moon will be applicable there. This characteristic of Mercury in being the most likely planet to preserve with little change its early surface features makes it a tempting object for at least one remote sensing probe. However, proximity to the Sun will probably complicate the otherwise slowly operating surficial processes.

On Venus and Mars, supposedly having little surface water, the dominant agent of surficial weathering, erosion, transport, and deposition will be atmospheric. The speed and direction of sediment movement will be controlled by meteorological patterns which may be worked out by meteorological techniques or possibly by radar maps of the orientations of dune complexes. These atmospheric agents will not suffer the same dependence on sea level as the Earth's watery agents. Instead, if continually concentrated in one area, wind erosion will cut to such a depth as to be balanced by the rate of isostatic rise. Likewise, the sediment deposition could continue accumulating in a region, the elevation of which would be determined by the rate of isostatic sinking under the load. On a planet like Mars, surface winds should blow towards the hotter equatorial zone and might produce an ingestion of sediments in an equatorial geosyncline. Isostasy dictates a subcrustal movement of material to complete the cycle for any unidirectional transport of surface sediments. The darker equatorial band of Mars may well represent just this type of zone ingestion. (This is certainly not the only possible explanation of the equatorial band. Vegetation, or lack of seasonal changes like those associated with the polar caps are also possibilities.) One goal of the investigations must be the understanding of the surface energy gradients and sumps for this energy.

The chemical effects on a water-deficient planetary surface, probably with a reducing atmosphere, would stand in sharp contrast to familiar terrestrial surface chemistry. Many of the commonly extensive marine rock types, particularly limestones and dolomites, should not occur, although ephemeral puddles may yield local evaporites. Seasonal wettings associated with the expansion and contraction of the Martian polar caps may bring about some chemical changes. Without water, chemical fractionation is more difficult, but still possible if atmospheric transport and deposition are selective of certain minerals such as micas and clays.

These chemical process studies must aim for some understanding of volumes moved, rates of movements and the balances of geochemical cycles. Special interest would apply to the cycles of nitrogen, sulfur, oxygen, carbon, and available water.

Sequence and Dates of Geologic Events. Application of principles of superposition to the landforms and geologic units will yield a general sequence of events for the planet, much as terrestrial photo-geology or the lunar mapping has done. Distinctive stratigraphic markers of wide areal extent would be most important. For instance, a widespread volcanic outpouring, as might result from a planetary thermal maximum, would provide a marker; or a widespread but short-lived tectonic event would provide correlation for the several regions of the planet. Exposures of stratigraphic columns should be sought and examined in as much detail as possible. It must be recognized that without water for gross chemical separations, the sedimentation may be hopelessly monotonous. However, any major changes that appear may represent very significant nonuniformitarian changes, as discussed in the next section.

Once established, the sequence must be keyed in to some absolute ages. Crude estimates may be possible from rate studies but the only real solution is radioactive age dating. At present, sample return is the only satisfactory procedure, but even then, the result from a single sample may not provide a sufficiently reliable estimate. It will be well to watch for methods of doing remote age determinations. However, being realistic, this phase of the problem will most likely await manned landings and sample return, which begin to be possible in the 1980's.

Nonuniformitarian Events. Almost all the preceding discussion has involved a relatively straightforward attack on the problem of planetary surfaces using that cornerstone of geology, uniformitarianism—"The present is the key to the past." However, these planets may have experienced, and still retain evidence of, nonuniformitarian events. Such abrupt departures from present conditions may include some of the most significant prizes to be gleaned from the surficial geology of the planets. Some of the possibilities are listed below.

Fossil Oceans

Discovery that oceans had existed on one of the other terrestrial planets in the past would be of major value in understanding the rate of degassing of a planet and its effect on the rate of evolution of our own oceans. Detection in the stratigraphic sequence of a number of characteristic marine sediments, particularly the limestones, would be the best clue. The former oceans would have permitted the greater diversification of sediments, the possibilities of more normal geosynclinal filling and, perhaps, related linear fold mountain systems.

Planetary Tectonic Patterns Changing with Time

The fracture or fold system of the planet might have changed with time. Depending on the changes, the pattern could give indications of Runcorn's theory of progressive increase in the number of convective cells as a function of time; of progressive tiltings of the axis if the fracture systems were related to zonal rotation; or to a past planetary thermal maximum reflected by extinct fracture systems and extensive volcanic outpourings.

Ancient Glacial Periods

The Earth has suffered several glacial eras at widely separated times, the underlying causes of which are unknown. If they were caused by decreases in solar intensity corresponding glacial advances might be found on other planets.

Catastrophism

This has been a difficult ghost to lay on Earth. If a number of satellites have indeed been captured, as much lunar theory suggests, tidal effects may have drawn some into catastrophic impact with the primary body, leaving scars still visible in the surficial geology.

Fossils

The chances of finding fossils on other planets by remote devices are too slim to be worth the effort of mounting a specific search. However, the presence of organic-rich formations in the sedimentary record should not be precluded. If found, these formations should be examined more carefully as supplements to the general biological program.

Past Atmospheres

We are still uncertain about the nature and effects of the early atmosphere of the Earth. If it was reducing, what was the over-all effect on the surficial chemical processes at that time? Conditions on other planets may now resemble those of the primitive Earth. The other planets may also have had long-term atmospheric changes reflected by a change in average oxidation state of the sediments, or other progressive compositional effects.

Concluding Remarks and Recommendations

1. The long-range objectives of geological study of the planets are quite parallel to biological study. Both need to define the environmental processes, one discipline to discover where and how life might evolve and adapt to these processes, the other to see how the processes have created the planetary surface geology.

2. In general, a continuing broad base of scientific research is more important than single high cost elaborate planetary probes, no matter how sophisticated. Without this scientific base, we will not be able properly to prepare instruments for planetary probes, nor interpret the results from them.

3. The value of going from the general to the specific, of combining a variety of types, scales, and areas of map data with surface measurements and observation, cannot be overemphasized. With Mars as the next major target two types of missions are needed: a topographic and geologic mapping mission from orbiter fly-by, and landing craft to do much of what Surveyor is planned to do on the Moon. Because of power

limitations much of the orbital mapping may have to be of a passive nature. These Mars missions should not miss the 1969-1973 opportunities, but concurrently, or soon thereafter, at least one mission for radar mapping of the surface of Venus should be flown before we are too deeply embarked on the Mars program.

4. Very much more can be done from Earth, by balloons or satellite-borne telescopes to extract planetary data. We should encourage measurements and instrumentation for this, expanding our knowledge of emission characteristics, permanent features, and secular changes.

5. Probably the most serious problem in planetary exploration is that of data return. High-quality images are among the most valuable data for surficial geology but, unfortunately, images have very high bit requirements. Every effort should be made to improve telemetry or the ability to return data directly to Earth, or both in the near future.

6. It is highly probably that prior to the 1980's all planetary surface work will have to be done by remote methods with little or no possibility of sample return. Concurrent development of orbiting Earth-sensing and Moon-sensing equipment will provide instrument capability, if not actual components, for the more distant missions. The planetary program should encourage this hardware development as well as the interpretative capabilities through application to terrestrial and lunar geologic problems.

7. We should plan for balanced and continuing exploration of the Moon and planets, avoiding the behavior of the bee in the garden, flitting from one colorful and exciting prospect to the next. Instead, a solid base of exploration of our main objective should be laid progressively, while exploration of other bodies continues concurrently.

UPPER ATMOSPHERES OF THE PLANETS

Joseph W. Chamberlain and Richard M. Goody

Introduction: Motivations for Research on Upper Atmospheres

The term "upper atmosphere" has no clear-cut definition in terrestrial studies and embraces an even less definite group of ideas in the other planets. However, from a practical point of view, we may note the following characteristics of upper atmosphere studies:

(i) They concern regions remote from the planet's surface (with the possible exception of Mercury) and emphasis is placed on simpler probes, such as small orbiters, rather than drop sondes, landers, or manned missions.

(ii) Interest is usually strongest in noninteracting phenomena (i.e., phenomena not closely related to the planetary dynamics). Planning is, therefore, often more precise, and instrumentation more easily designed, than is characteristic of meteorology and geology and biology.

(iii) Many workers in universities and research institutions have a strong interest in this field. Theoretical backing is therefore unusually good.

(iv) Since the Earth's upper atmosphere had to be explored with remote sensors and rocket and satellite probes, the general approach to planetary problems is already well understood, and much instrumental experience is already available.

Thus, there are valid practical reasons for considering upper atmosphere studies in a separate category, but it should be borne in mind that they constitute only one aspect of a complete study of the atmosphere.

This report is intended to serve as a background for further thought and discussion about the proper emphasis that the upper atmospheres of the planets should receive in the national program of space science and exploration. Were the matter one of individual preference for research activity at rather modest costs, no problems would exist. But the emphasis here is on research that forms part of multi-billion-dollar programs, so that the matter of motivation ceases to be mainly a private and individual one. And that is the basic reason why we rank (below) the "national motivations" for research in the order given below.

We recommend that experiments dealing mainly with planetary upper atmospheres be reviewed and selected for support with due consideration of the following priority ratings:

1. (Urgent). Serving Engineering Requirements for Support of Exploration. We are not aware of any particularly urgent needs in this category at the present time. There are obvious examples of the kind of information that could conceivably be important: upper-atmosphere densities govern the drag on planetary orbiters; a planetary ionosphere might affect communications in an important way.

2. (Important). Related to Exobiology and the Evolution of Life. The study of extraterrestrial life, to be of any but philosophical significance, has to be viewed in the perspective of the planetary environment and its evolution. Upper-atmosphere studies may furnish information on atmospheric constituents that are important in this regard. But perhaps more important is the escape temperature of the planetary exosphere - this temperature governs the rate of loss of atmospheric constituents and is a basic parameter to the evolutionary study of a planet. It can be obtained directly from certain spectroscopic observations.

3. (Desirable). Bearing on Origin and Evolution of the Solar System. After the question of extraterrestrial life, the history of the solar system stands as probably the most important scientific question directly related to the space sciences. We suspect that Jupiter, as the largest and nearest of the major planets, may contain much hidden information bearing on this larger question. Just as it is necessary to study life as an evolving phenomenon, it will be important to understand the evolution of the Jovian atmosphere, and upper-atmosphere observations that bear on this study should now be considered especially desirable.

4. (Interesting Academically). Illuminating the Comparative Anatomy of Atmospheres. This topic, of course, overlaps the previous three, but it also includes the entire vast area of the subject. As always, a purely academic approach to research could turn up the most exciting finds.

But the amount of funding required for planetary exploration leads us to recommend that experiments justifiable only under this category be given less priority than others. In a larger sense, the atmospheric sciences should stand ready to aid the program of exploration wherever possible and be agile enough to benefit from its unique opportunities and discoveries.

The Present State of Knowledge: An Annotated Bibliography

In the broad sense almost any work on a planetary atmosphere bears on its upper atmosphere. The atmospheric composition is an especially essential item, ordinarily obtained from spectral absorption studies of the lower atmosphere. However, for purposes of this review we have omitted specific consideration of such work and have regarded the upper atmosphere as the region where photodissociation or ionization, or both of them are important. (This region might extend down to the surface.)

General Reviews. Of the many reviews on planetary atmospheres, a few touch on upper atmospheres; similarly, a few studies of the Earth's upper atmosphere have dealt with other planets, sometimes in a comparative way. Several examples (in brackets we note their bearing on the present topic) are:

Spitzer, L., Jr. [atmospheric evaporation and evolution], The terrestrial atmosphere above 300 km, in The Atmospheres of the Earth and Planets, 2nd ed., G.P. Kuiper, ed., Chap. VII, pp. 211-247, University of Chicago Press, Chicago, 1952.

Singer, S.F. [suggested experiments from above an atmosphere], Geophysical research with artificial Earth satellites, in Advances in Geophysics, H.E. Landsberg, ed., Vol. 3, pp. 301-367, Academic Press, New York, 1956.

Urey, H.C. [atmospheric evaporation and evolution], The atmospheres of the planets, Handbuch der Physik, 52, 363-418, 1959.

Mayer, C.H. [Jovian magnetically trapped particles], Radio emission of the Moon and planets, in Planets and Satellites, G.P. Kuiper and B.M. Middlehurst, eds., Chap. 12, pp. 442-472, University of Chicago Press, Chicago, 1961.

Öpik, E.J. [lunar atmosphere], Surface properties of the Moon, in Progress in the Astronautical Sciences, Vol. I, S.F. Singer, ed., Chap. V, pp. 215-260, North-Holland Publishing Co., Amsterdam, 1962a.

Öpik, E.J. [photochemistry of Mars; temperature profile of Venus], Atmosphere and surface properties of Mars and Venus, in Progress in the Astronautical Sciences, Vol. I, S.F. Singer, ed., Chap. VI, pp. 267-342, North-Holland Publishing Co., Amsterdam, 1962b.

Rasool, S.I. [vertical atmospheric structure], Structure of planetary atmospheres, AIAA J., 1, 6, 1963.

Sagan, C., and W.W. Kellogg. [Venus microwave radiation as possibly having an ionospheric origin; Martian photochemistry], The terrestrial planets, Ann. Rev. Astron. Astrophys., 1, 235-266, 1963.

Sonett, C.P. [the Venus Mariner findings], A summary review of the scientific findings of the Mariner Venus mission, Space Sci. Rev., 2, 751-777, 1963.

Cameron, A.G.W. [photodissociation], Physics of the planets, in Space Physics, Part II, Solar and Planetary Physics, D.P. LeGalley and A. Rosen, eds., Chap. 5, pp. 127-165, John Wiley & Sons, New York, 1964.

Atmospheric Evaporation and Evolution; Planetary Coronas. The theory of escape of gases from a planetary atmosphere has a long history. In recent years the subject has been developed and/or applied to atmospheric evolution by

Spitzer, L., Jr., The terrestrial atmosphere above 300 km, in The Atmospheres of the Earth and Planets, 2nd ed., Chap. VII, pp. 211-247, University of Chicago Press, Chicago, 1952.

Bates, D.R., and M.R.C. McDowell, Atmospheric helium, J. Atmos. Terrest. Phys., 11, 200-208, 1957.

Bates, D.R., and M.R.C. McDowell, Escape of helium. J. Atmos. Terrest. Phys., 16, 393-394, 1959.

Nicholet, M., The aeronomic problem of helium. Ann. geophys., 13, 1-21, 1957.

Urey, H.C., The atmospheres of the planets, Handbuch der Physik, 52, 363-418, 1959.

Jastrow, R., Outer atmospheres of the Earth and planets, in The Exploration of Space, R. Jastrow, ed., pp. 142-151, The Macmillan Company, New York, 1960.

Öpik, E.J., Selective escape of gases, Geophys. J., 7, 490-509, 1963.

Chamberlain, J.W., Planetary coronae and atmospheric evaporation, Planetary Space Sci., 11, 901-960, 1963.

The latter paper also treats the closely related problem of the "corona" or "outermost atmosphere" or "exosphere" of a planet. For the Earth the neutral hydrogen corona has been observed from rockets - see, for example, a review by

Chamberlain, J.W., The geocorona, in Research in Geophysics, Vol. 1, Sun, Upper Atmosphere, and Space, H. Odishaw, ed., Chap. 8, pp. 189-196, The M.I.T. Press, Cambridge, 1964.

and similar observations could yield the coronal temperature of other planets. An experiment developed by C.A. Barth and L. Wallace was expected to obtain data on the Mars O and H coronas from aboard the Mariner IV spacecraft, but it was reportedly disconnected during the latter stages of flight preparation.

Composition and Photochemistry. General recent reviews of the Venus and Mars photochemical problems are given by

Öpik, E.J., Atmosphere and surface properties of Mars and Venus, in Progress in the Astronautical Sciences, Vol. 1, S.F. Singer, ed., Chap. VI, pp. 267-342, North-Holland Publishing Co., Amsterdam, 1962.

Sagan, C., and W.W. Kellogg, The terrestrial planets, Ann. Rev. Astron. Astrophys., 1, 235-266, 1963.

Observational techniques for deriving chemical compositions spectroscopically in the photochemically active regions have been discussed by

Singer, S.F., Geophysical research with artificial Earth satellites, in Advances in Geophysics, H.E. Landsberg, ed., Vol. 3, pp. 301-367, Academic Press, New York, 1956.

Kaplan, L.D. Interpretation of planetary probe measurements, in The Atmospheres of Mars and Venus, A Report by Panel on Planetary Atmospheres, Space Science Board, prepared by W.W. Kellogg and C. Sagan, pp. 105-106, 1961a.

Kaplan, L.D., On the determination of upper-atmosphere composition from satellite measurements, in Chemical Reactions in the Lower and Upper Atmosphere, Chap. 17, pp. 269-274, Interscience Publishers (Wiley), New York, 1961b.

Kaplan, L.D., The spectroscope as a tool for atmospheric sounding by satellites, J. Quant. Spectry. Radiative Transfer, 1, 89-95, 1961c.

Another technique for deriving compositions, proposed by

Brandt, J.C., A note on Rayleigh and Raman scattered Lyman-α radiation from Jupiter and Saturn, Planetary Space Sci., 11, 725-726, 1963,

is based on observations of the Raman spectrum of the planet. (Still other optical methods are included in Section 5, below.)

Most of the work on the photochemistry of planetary atmospheres has been theoretical. For Mars there appears to be some disagreement between the different calculations of

Paetzold, H.K., On the problems of a Martian ozonosphere, Mem. Soc. Roy. Sci. Liege, 7, 452-462, 1962.

and

Marmo, F.F., Shardanand, and P. Warneck. Ozone distribution in the atmosphere of Mars, J. Geophys. Res., 70, 2270-2272, 1965.

The nitrogen oxides on Mars have been discussed theoretically by

Warneck, P. and F.F. Marmo, NO_2 in the Martian atmosphere, J. Atmos. Sci., 20, 236-240, 1963.

Sagan, C., P.L. Hanst, and A.T. Young, Nitrogen oxides on Mars, Planetary Space Sci., 13, 73-88, 1965.

Observations yielding useful upper limits have been made by

Sinton, W.M., An upper limit to the concentration of NO_2 and N_2O_4 in the Martian atmosphere, Publ. Astron. Soc. Pacific, 73, 125-128, 1961.

Spinrad, H., The NO_2 content of the Martian atmosphere, Publ. Astron. Soc. Pacific, 75, 190-191, 1963.

Molecular oxygen (O_2) may exist on Mars and Venus only as a result of photodissociation of CO_2 and subsequent reassociation of O. Further, the O_2 thus formed will shield the CO_2 from photodissociation. These effects were first examined for Mars by

Chamberlain, J.W., Upper atmospheres of the planets, Astrophys. J., 136, 582-593, 1962,

and have been discussed for Venus by

Shimizu, M., Vertical distribution of neutral gases on Venus, Planetary Space Sci., 11, 269-273, 1963.

Upper limits of O_2 have been established from spectral observations by

Prokofiev, V.K., and N.N. Petrova, On the question of the presence of oxygen in the atmosphere of Venus, Mem. Soc. Roy. Sci. Liege, 7, 311-321, 1963.

Spinrad, H., and E.H. Richardson, An upper limit to the molecular oxygen content of the Venus atmosphere, Astrophys. J., 141, 282-286, 1965.

Some photochemical problems peculiar to the major planets have been discussed by

Cadle, R.D., The photochemistry of the upper atmosphere of Jupiter, J. Atmos. Sci., 19, 281-285, 1962.

Structure of Upper Atmospheres and Ionospheres. Direct information on planetary upper atmospheres is virtually nonexistent. However, an occultation of Regulus by Venus, as observed by

Vaucouleurs, G. de, and D.H. Menzel, Results of the occultation of Regulus by Venus, July 7, 1959, Nature, 188, 28-33, 1960,

was analyzed to yield the pressure at a height well above the cloud tops.

Illustrative models for the temperature and density distribution in the middle atmosphere of Mars were first computed by

Goody, R.M., The atmosphere of Mars, Weather, 12, 3-15, 1957.

In these models heat was transported by radiation absorbed and emitted by assumed amounts of either CO_2 or H_2O. He pointed out, however, that the presence of O_3 could have important effects on such models. Extensions of this work have been made by

Arking, A., Non-grey convective planetary atmospheres, Mem. Soc. Roy. Sci. Liege, 7, 180-189, 1963.

Ohring, G., A theoretical estimate of the average vertical distribution of temperature in the Martian atmosphere, Icarus, 1, 328-333, 1963.

Prabhakara, C., and J.S. Hogan, Jr., Ozone and carbon dioxide heating in the Martian atmosphere, J. Atmos. Sci., 22, 97-109, 1965.

An illustrative model of the upper atmosphere of Mars, built, so to speak, on top of Goody's model, was computed by

Chamberlain, J.W., Upper atmospheres of the planets, Astrophys. J., 136, 582-593, 1962,

and this work has been considerably extended by

McElroy, M.B., J. L'Ecuyer, and J.W. Chamberlain, Structure of the Martian upper atmosphere, Astrophys. J., 141, in press, 1965.

with emphasis on the uncertainties in the problem. It appears that a satisfactory measurement of the escape (exospheric) temperature would remove the main unknowns in the models.

In these models it is necessary to solve for the temperature distribution and density distribution simultaneously; hence the heat-flow problems are essential elements to any legitimate models. There are some models available in which no attempt is made to compute the temperature structure accurately:

Yanow, G., A study of the Martian upper atmosphere and ionosphere, J. Astronaut. Sci., 8, 103 (only), 1961.
Schilling, G.F., Extreme model atmosphere of Mars, Mem. Soc. Roy. Sci. Liege., 7, 448-451, 1963.
Schilling, G.F., A note on the upper atmosphere of Mars, J. Geophys. Res., 68, 4875-4876, 1963.

(The work of Schilling is concerned mainly with establishing useful, realistic limits.)

Rather different ionospheres of Mars have been proposed by

Chamberlain, J.W., Upper atmospheres of the planets, Astrophys. J., 136, 582-593, 1962.
Danilov, A.D., Model of the ionosphere of Venus and Mars, Geomagnetizm i Aeronomiya, 1, 281-285, 1961.
Danilov, A.D., Models for the ionosphere of Venus and Mars, in Space Research III, W. Priester, ed., pp. 1026-1035, North-Holland Publishing Company, Amsterdam, 1963.

Danilov's papers also consider Venus' ionosphere. It now seems most unlikely that the microwave radiation from Venus originates in the ionosphere, see, for example,

Sagan, C., and W.W. Kellogg, The terrestrial planets, Ann. Rev. Astron. Astrophys., 1, 235-266, 1963.

It has been proposed that the Martian blue haze is an upper-atmospheric effect. For example, it was attributed to absorption and fluorescence by upper-atmospheric ions by

Urey, H.C., and A.W. Brewer, Fluorescence in planetary atmospheres, Proc. Roy. Soc. (London), A 241, 37-43, 1957.

More recently

Sagan, C., Is the Martian blue haze produced by solar protons? Icarus, 1, 70-74, 1962,

re-examined the hypothesis and considered it unlikely.

Recently a model upper atmosphere for Jupiter has been calculated by

Gross, S.H., and S.I. Rasool, The upper atmospheres of Jupiter, Icarus, 3, 311-322, 1964; Erratum; ibid. 4, 110, 1965.

It is not clear yet to what extent Mercury has an atmosphere and therefore whether, like the Moon, all its atmosphere is an upper atmosphere. Recent pertinent work is by

Spinrad, H., and P.W. Hodge, An explanation of Kozyrev's hydrogen emission lines in the spectrum of Mercury, Icarus, 4, 105-108, 1965.

Spinrad, H., G.B. Field, and P.W. Hodge, Spectroscopic observations of Mercury, Astrophys. J., 141, 1155-1160, 1965.

The lunar atmosphere has been the subject of studies by

Costain, C.H., B. Elsmore and G.R. Whitfield, Radio observations of a lunar occultation of the Crab Nebula, Monthly Notices Roy. Astron. Soc., 116, 380-385, 1956.

Elsmore, B., Radio observations of the lunar atmosphere, in Paris Symposium on Radio Astronomy, R.N. Bracewell, ed., Part I, Paper 6, pp. 47-49, Stanford University Press, Stanford, 1959.

Öpik, E.J., Surface properties of the Moon, in Progress in the Astronautical Sciences, Vol. I, S. F. Singer, ed., Chap. V, pp. 215-260, North-Holland Publishing Company, Amsterdam, 1962.

Weil, H., and M.L. Barasch, A theoretical lunar ionosphere, Icarus, 1, 346-356, 1963.

Airglow and Other Optical Missions. A considerable literature is growing on possible day and night airglows to be expected from a planet and on how such observations might be interpreted. General articles are by

Chamberlain, J.W., The interpretation of ultraviolet spectra of planetary atmospheres and the near-infrared CO_2 bands of Venus, in The Atmospheres of Mars and Venus, A Report by Panel on Planetary Atmospheres, Space Science Board, prepared by W.W. Kellogg and C. Sagan, pp. 147-151, 1961.

Chamberlain, J.W., Airglow and the physics of upper atmosphere, Science, 142, 921-924, 1963a.

Chamberlain, J.W., Dayglow spectra of the planets, J. Quant. Spectry. Radiative Transfer, 3, 499-505, 1963b.

Barth, C.A., Ultraviolet spectroscopy of planetary atmospheres, in Dynamics of Manned Lifting Planetary Entry, S.M. Scala, A.C. Harrison, and M. Rogers, eds., pp. 82-94, John Wiley & Sons, New York, 1963.

A possible strong ultraviolet dayglow on Mars, arising from association forming CO_2, has been suggested by

Inn, E.C.Y., Martian airglow, J. Atmos. Sci., 21, 220-221, 1964.

The theory of daytime airglows arising from fluorescent scattering in the ultraviolet has been developed (and applied to N_2 emissions from Mars) in a series of papers:

Chamberlain, J.W., and Y. Sobouti, Fluorescent scattering in planetary atmospheres. I. Basic theoretical considerations, Astrophys. J., 135, 925-937, 1962.

Sobouti, Y., Fluorescent scattering in planetary atmospheres. II. Coupling among different transitions, Astrophys. J., 135, 938-954, 1962.

Sobouti, Y., Fluorescent scattering in planetary atmospheres. III. Formation of Lyman-Birge-Hopfield bands of N_2 in the Martian atmosphere, Astrophys. J., 138, 720-747, 1963a.

Sobouti, Y., Fluorescent scattering in planetary atmospheres. IV. Formation of Lyman-Birge-Hopfield bands of N_2 in the terrestrial atmosphere, Astrophys. J., 138, 748-760, 1963b.

In a sense, high-energy, magnetically trapped particles may be considered part of the planet's outer ionosphere. The Jupiter decimeter radiation is now interpreted as arising from such particles. From a rather large literature we mention

Mayer, C.H., Radio emission of the Moon and planets, in Planets and Satellites, G.P. Kuiper and B.M. Middlehurst, eds., Chap. 12, pp. 442-472, University of Chicago Press, Chicago, 1961.

Chang, D.B., and L. Davis, Jr., Synchrotron radiation as the source of Jupiter's polarized decimeter radiation, Astrophys. J., 136, 567-581, 1962.

Ellis, G.R.A., The radio emissions from Jupiter and the density of Jovian exosphere, Australian J. Phys., 16, 64-81, 1963.

The Mariner to Venus did not detect near that planet any significant enhancement of charged particles over the interplanetary densities:

Frank, L.A., J.A. Van Allen, and H.K. Hills, Mariner 2: Preliminary reports on measurements of Venus: Charged particles, Science, 139, 905-907, 1963.

Smith, E.J., L. Davis, Jr., L.J. Coleman, Jr., and C.P. Sonett, Magnetic Measurements near Venus, J. Geophys. Res., 70, 1571-1586, 1965.

Neugebauer, M., and C.W. Snyder, Solar-wind measurements near Venus, J. Geophys. Res., 70, 1587-1591, 1965.

Earlier reports of airglow or auroral radiation from the dark side of Venus have not been confirmed and are of doubtful validity. There is also the possibility that any such emission actually arises from lightning discharges near or even below the clouds. The principal papers on this topic are

Kozyrev, N.A., The night sky glow of Venus (translated title), Izv. Krymsk. Astrofiz. Observ., 12, 169-176, 1954.

Newkirk, G., Jr., The airglow of Venus, Planetary Space Sci., 1, 32-36, 1959.

Warner, B., The emission spectrum of the night side of Venus, Monthly Notices Roy. Astron. Soc., 121, 279-283, 1960.

Weinberg, J.L., and G. Newkirk, Airglow of Venus: a re-examination, Planetary Space Sci., 5, 163 (only), 1961.

Meinel, A.B., and D.T. Hoxie, On the spectrum of lightning in the atmosphere of Venus, Communications of the Lunar and Planetary Laboratory 1, No. 7, 35-38, 1962.

Some preliminary ultraviolet photometry of planets from rockets has been reported for Mars and Jupiter by

Boggess, A., and L. Dunkleman, Ultraviolet reflectivities of Mars and Jupiter, Astrophys. J., 129, 236-237, 1959.

Low-resolution ultraviolet spectra have been obtained on film with an objective-grating spectrograph of Mars by

Evans, D.C., The observed Ultraviolet reflectivity of Mars, Science, 149, 969-972, 1965.

and of Venus and Jupiter by

Evans, D.C., A. Boggess, and R. Scolnik, The reflectivity of Venus and Jupiter in the middle ultraviolet, Astron. J., 70, 321 (only), 1965.

Photoelectric ultraviolet spectra of Jupiter were recorded by

Stecher, T.P., The reflectivity of Jupiter in the ultraviolet, Astrophys. J., 142, (No. 3), in press, 1965.

The Observations

From the atmospheric point of view the most interesting planets that are likely to be accessible in the next ten years are Mars, Venus, and Jupiter.

Observations can be made from the Earth's surface, balloons, rockets, Earth orbiters, fly-bys, planetary orbiters, and drop sondes. The observational program should include all of the methods in a unified program guided by a strong theoretical effort. The need to create facilities should, however, not be judged with respect to the upper atmospheres program alone, for astronomy and lower atmosphere studies have some similar requirements.

We shall now review some topics in upper atmosphere research in order to illustrate their priority and the type of observational system required.

(a) Topside sounders can provide information about composition and temperature as well as electron densities. Some of the data are needed for communications engineering and are of high priority. In principle, some work could be done from a near-Earth satellite but weight and power requirements are probably excessive. The work is ideally performed from a planetary orbiter or fly-by.

(b) Airglow measurements can yield valuable composition data. Measurements can take place from the Earth or from balloons, and a space probe is unnecessary.

(c) Densities from drag measurements. Upper atmospheric densities are required for engineering design and should be systematically investigated. To obtain densities from drag measurements, planetary orbiters are essential and would be particularly interesting in the case of Venus.

(d) Magnetic fields will presumably be measured for geophysical purposes. A fly-by is necessary.

(e) Special scans of resonance lines give information on atmospheric constitution and the escape temperature. The results have some relation, therefore, to the dynamics of the solar system. Can be performed only from an orbiter or fly-by.

(e) Raman spectra allow measurement of hydrogen and other substances. Stabilized rockets or Earth satellites are required for observations of this kind.

(g) Ultraviolet photometry. Wide-band and spectral investigations in this region can lead to data on atmospheric composition and density. Some data could be obtained from balloons, some from rockets, but the preferred system is an Earth-orbiting observatory.

(h) Ultraviolet absorption of sunlight. The vertical distribution of ozone, oxygen, and other absorbers can be determined by flights through the twilight zone. A planetary orbiter or fly-by is necessary.

(i) Far infrared emissions. The 62-μ oxygen line and microwave lines give information about composition. Observations could be made from an Earth-orbiting planetary observatory.

(j) Occultations. Density measurements can be made from stellar occultations during the approach to a fly-by or orbiter mission.

Recommended Program

To carry out the foregoing program, we recommend

(a) Maintenance of a flexible exploratory program based upon the minimum space hardware, capable of sending small probes to Venus, Mars, Jupiter, and perhaps other planets, at every suitable opportunity over the next 10 years.

(b) In order to involve the academic community in an effective way, an organizational and engineering study should be made to determine the minimum possible lead time between submission of a good proposal and launch. A target of $2^1/_2$ years is suggested, and this is already so long as to be a serious impediment to the involvement of important sections of the scientific community.

(c) The space probes must not be considered in isolation, but a part of an integrated approach using ground-based instruments, balloons, rockets and Earth orbiting laboratories. Such an integrated program includes:

(i) the availability of a 100-in. telescope as a support and patrol instrument;

(ii) the development of a fine pointing control for rockets; a satisfactory performance would be an absolute accuracy of 10 sec with 2 sec jitter.

(iii) the instrumentation of an Orbiting Astronomical Observatory for spectrographic observations of the planets.

ON THE CIRCULATION OF THE ATMOSPHERES OF JUPITER AND SATURN

Raymond Hide

Introduction

Most discussions of the circulations of planetary atmospheres have concentrated on the Earth and Mars, but rather little attention has been given to the great planets, Jupiter and Saturn, and to Venus. The present paper deals only with the great planets; Venus presents somewhat different (and equally interesting) problems and has to be considered separately.

Present knowledge of the internal structure of the great planets is so meager that any attempt to discuss the dynamics of their atmospheres, especially their lower atmospheres, might seem pointless, even frivolous. Nevertheless, the dynamics of the atmospheres of the great planets deserves some attention, in spite of more pressing hydrodynamical problems concerning those regions of the solar system to which we either possess direct access already or expect to gain such access in the near future. By concentrating on the interpretation in terms of hydrodynamical theory of certain prominent visual features, and avoiding in the first instance the problem of the general circulation, it should be possible to identify certain key processes, so that when in due course the problem of the general circulation is taken up, the degree of speculation involved can be kept within acceptable limits. Moreover, as recent work has shown in the case of Jupiter, there is the possibility that observations of visual markings will, especially when combined with radio-astronomical observations, lead directly to information on the internal structure of the planet of a kind that may be unobtainable by any other means.

Observations

Jupiter. The mean radius of the visible surface of Jupiter is about 69,000 km. The reflection of sunlight by this surface is due largely to opaque clouds suspended in an atmosphere of hydrogen, helium, and methane. These clouds, which may be composed of ammonia crystals, effectively shroud the underlying planet from direct view, so that the depth of the atmosphere below the visible cloud layer is unknown.

Some of the earliest telescopic observations of the ephemeral markings of Jupiter's visible surface were made by Robert Hooke in the seventeenth century, and for the past hundred years information on the visual appearance of Jupiter has been collected and recorded sytematically, mainly by dedicated amateurs using telescopes of around 10 inches in diameter. An account of this work is given in Peek (1958), where many important details can be found.

The most prominent markings are the bright cloud belts, of which there are usually about seven or eight. These belts run parallel to the equator and are separated by darker zones. The belts and zones are not entirely regular. Dark patches often appear on the bright regions and bright patches on the dark regions, and the boundaries between belts and zones often take on a serrated shape. The most striking marking of all, the Great Red Spot (G.R.S.), is elliptical in shape, having its long axis along zenocentric latitude 22°S, and occupies 30 degrees of longitude and 10 degrees of latitude. While the belts and zones are permanent features only in the sense that the planet always exhibits a banded appearance, the bands are subject to significant variations in latitude. The Red Spot, on the other hand, has always appeared in the same latitude ever since the first telescopic observations revealed its presence in 1664.

The motion of irregular markings across the visible disk yields information on the rotation of the planet. Rotation periods thus measured depend both on time and position on the disk; they suggest a complicated variation with latitude that is unsymmetrical about the equator. The rotation period of the equatorial zone, which lies between approximately ±7° latitude (see Figure 1), is roughly five minutes less than that found at higher latitudes, and for measuring the longitude of features of Jupiter's surface astronomers have found it convenient to introduce two separate reference systems: System I with a rotation period of 9h 50m 30s.003, and System II with a period of 9h 55m 40s.632. (A third system, System III, rotation period 9h 55m 29s.37, was introduced quite recently for convenience in studying the emission of radiation on radio-wavelengths from Jupiter.)

Within the equatorial zone the rotation is most rapid roughly on the equator itself. Temporal fluctuations in period, with amplitudes of up to 30 sec, are indicated by the observations.

At higher latitudes, spatial variations of the mean (with respect to time) period range from 9h 55m 5s to 9h 55m 54s. As regards temporal variations, these are erratic and range from 10 sec to 20 sec, depending on position.

During the past hundred years or so, the period of rotation of the G.R.S. has varied between 9h 55m 31s and 9h 55m 44s, the average value being 9h 55m 38s. The average (with respect to position) period of ephemeral markings of the South Tropical Zone, within which the G.R.S. is located, has varied between 9h 55m 27s and 9h 55m 36s, the corresponding average linear velocity of the South Tropical Zone with respect to the G.R.S. being about +2m/sec, the positive sign indicating that this motion is in an eastward (westerly) direction referred to the planet (see Figure 2). For the sake of comparison, points on the equator of Jupiter rotate about the axis of the planet at 12 km/sec, and the linear velocity of the equatorial zone relative to points on the equator of a hypothetical surface rotating with the angular velocity of higher-latitude regions is about +100 m/sec.

It is impossible in a summary such as this to do justice to the great wealth of detail concerning the duration and appearance of individual markings presented in Peek's book. Typical lifetimes range from days for the smallest markings, to decades for markings of dimensons comparable in size with the radius of the planet. An example of one of these large features is the so-called "South Tropical Disturbance", which made its appearance in 1901 as a short dark streak in the South Tropical Zone, some distance downstream of the G.R.S. For the next four decades it grew in length until it stretched nearly two-thirds of the way around the planet. Its rotation period was somewhat less than that of the G.R.S., conjunctions of these two features having occurred on nine occasions between 1901, when it vanished, giving way, apparently, to three white spots in the belt just south of the Red Spot which have been observed continuously until the present time. The details of the bizarre interaction between the Red Spot and the South Tropical Disturbance contain a wealth of clues for the fluid dynamicist. The most remarkable feature of this interaction is the tendency for the South Tropical Disturbance to skirt around the edge of the G.R.S., at a speed which is up to ten times the speed at which it approaches and recedes from the G.R.S.

Saturn. The radius of the main disk of Saturn is 57,000 km, somewhat less than that of Jupiter. The two planets are believed to have somewhat similar compositions, though the mean density of Saturn, 0.68 gm/cm^3, is only half that of Jupiter. Saturn is 9.5 AU from the Sun, nearly twice the distance of Jupiter.

Saturn, like Jupiter, is enveloped in dense clouds arranged in belts parallel to the rotation axis. These belts appear to be more regular than those on Jupiter. Spots and other eruptions on Saturn are infrequent, and nothing quite comparable with the Red Spot on Jupiter has been seen on Saturn. A detailed account of visual observations of Saturn has been given by Alexander (1962).

Transits of long-lived spots yield rotation periods of 10h 13m near the equator and 10h 40m in mid-latitudes, the corresponding wind velocity associated with the equatorial current being 400 m/sec in a positive direction (i.e., eastward or westerly), four times that of the equatorial current on Jupiter (Robinson, 1961; Dollfus, 1963, 1965).

Theoretical Remarks

The complete specification of any hydrodynamical system requires knowledge of the physical properties and chemical composition of the fluid one is dealing with, the geometry of the system, and the nature and distribution of energy sources. Ability to solve the mathematical equations governing the system is also required. Only in a make-believe world, where such knowledge and ability would be available, would observations serve merely as a check on theoretical calculations. In practice, as with most other problems in planetary science, it is more usual to work backwards from the observations to achieve the best hypothetical model that is consistent with our imperfect observational and theoretical knowledge.

There is direct spectroscopic evidence for the existence of methane and ammonia in the atmospheres of Jupiter and Saturn. The presumption that the main constituents of these atmospheres are hydrogen and helium is based on the low mean densities of the planets and, in the case of Jupiter, on a measurement of the light variation of a star during an occultation, leading to an estimate of the mean molecular weight of the material above the cloud level (Baum and Code, 1953).

Color variations across Jupiter's disk have not yet been explained. It has been suggested that these variations may be due to traces of sodium, potassium, or some other metal, and certain free radicals, at cloud level in the atmosphere (see Payne-Gaposchkin, 1956; Rice, 1956).

Theories of the internal constitution of the great planets do not seem capable of giving a direct estimate of the depths of their atmospheres (de Marcus, 1958; Peebles, 1964; Wildt, 1961).

Atmospheric motions on Jupiter and Saturn may derive their kinetic energy not only from solar heating but also from internal sources. Öpik (1962), for example, believes that the rate at which internal sources supply energy to Jupiter's atmosphere is between 20% and 100% of that associated with solar radiation, the corresponding figure for Saturn being slightly higher. For the sake of comparison, the gravitational energy of the planet divided by the total solar energy intercepted by the planet in four billion years is about 50 in the case of Jupiter and 20 in the case of Saturn; the corresponding figure for the Earth is about 3×10^{-2}.

Of the parameters required to characterize the dynamics of the atmosphere of a rapidly rotating planet, those involving the angular velocity of basic rotation of the planet, Ω, are the so-called "Rossby number",

$$R \equiv U/2L\,\Omega \tag{1}$$

a rotational Mach number,

$$M \equiv L\Omega/c \tag{2}$$

and a parameter

$$B \equiv \omega^2/4\Omega^2 \tag{3}$$

where U is a typical horizontal wind speed, L is a length characteristic of the horizontal scale of the motion, c is the speed of sound in the atmosphere, and ω is the so-called Brunt-Väisälä frequency, given by

$$\omega^2 \equiv -g\,\Gamma/\rho \tag{4}$$

where g is the acceleration of gravity, Γ is the vertical gradient of potential density, and ρ is the actual density. When Γ is positive (i.e., subadiabatic lapse rate), ω is real and B is positive. A fluid particle displaced vertically by a small amount from its equilibrium position oscillates about that position with angular frequency ω. When Γ is negative (i.e., superadiabatic lapse rate), ω is imaginary and B is negative. The hypothetical equilibrium state of such an atmosphere would then be unstable to disturbances involving vertical displacements of fluid particles.

To R, M, and B should be added a fourth parameter,

$$D \equiv 2\,d/a \tag{5}$$

where a is the radius of the planet and d is a length characteristic of the vertical structure of the atmosphere.

If the electrical conductivity of the atmosphere is anywhere so high that the magnetic Reynolds number exceeds about 10 (see below), this parameter would also have to be considered.

For the sake of definiteness, consider planetary-scale features only, i.e., those for which $L \sim a$. For the Earth's atmosphere $R \sim 10^{-1}$, $M \sim 1$, and $D \sim 10^{-3}$. The vertical lapse rate is stable, so that B is positive. The pressure forces that give rise to atmospheric motions result entirely from the horizontal north-south temperature gradient. In the theory of these motions the parameter BD^2 plays an important role, and it is significant not only that this parameter is positive for the Earth but also that it is less than unity, since only when BD^2 is sufficiently small will nonaxisymmetric flow giving rise to the main energy-producing eddies—the cyclones—occur (Charney, 1947; Eady, 1949).

In the atmospheres of Jupiter and Saturn, much smaller values of R are encountered, typical values ranging from 10^{-5} in equatorial regions down to 10^{-2} at higher latitudes (Lowell Observatory, 1952; Hide, 1961, 1963).

At very low values of R the effects of quite shallow topographic features of any bounding surface underlying the atmosphere will extend upward throughout the depth of the atmosphere, a simple example of such a phenomenon being the Taylor column. This result has been used in an attempt to give a rational explanation of Jupiter's Great Red Spot (Hide, loc. cit.). The implications of this suggestion in regard to

(i) the physical and chemical nature of the region underlying Jupiter's atmosphere,

(ii) the angular momentum transfer between different parts of the planet required to account for the Red Spot's variations in latitude (see Figure 2) and the reconciliation of these variations with the "radio-period" of the planet, and

(iii) the nature of the topographical feature, which may in fact be an electric current loop, have yet to be fully worked out (see below). These issues are fairly controversial at the present time.

The existence of rapid equatorial currents in the fluid layers of rapidly rotating planets seems fairly general; Jupiter and Saturn exhibit such currents, and on Earth we have the Cromwell current in the ocean (Knauss, 1963) and the Berson westerlies in the atmosphere. There is as yet no entirely satisfactory theory of these currents, though dynamical oceanographers have given much attention to the Cromwell current in the past few years (see Deep Sea Research, 1960). The angular latitudinal width of the current (measured in radians) should be of the order of $R^{1/2}$ [Eq. (1)], and if these currents represent sinks of kinetic energy and angular momentum originating at higher latitudes, it can be argued (Hide, unpublished) that they might build up until $R \sim D$ [see Eqs. (1) and (5)]. Observations (see, for example, Figure 1) are at least consistent with the first of these results, and the second result suggests that d is 250 km for Jupiter and 1000 km for Saturn. 250 km is consistent with an independent (though equally tentative) estimate of d suggested by Hide's model of the Great Red Spot (Hide, 1961, 1963).

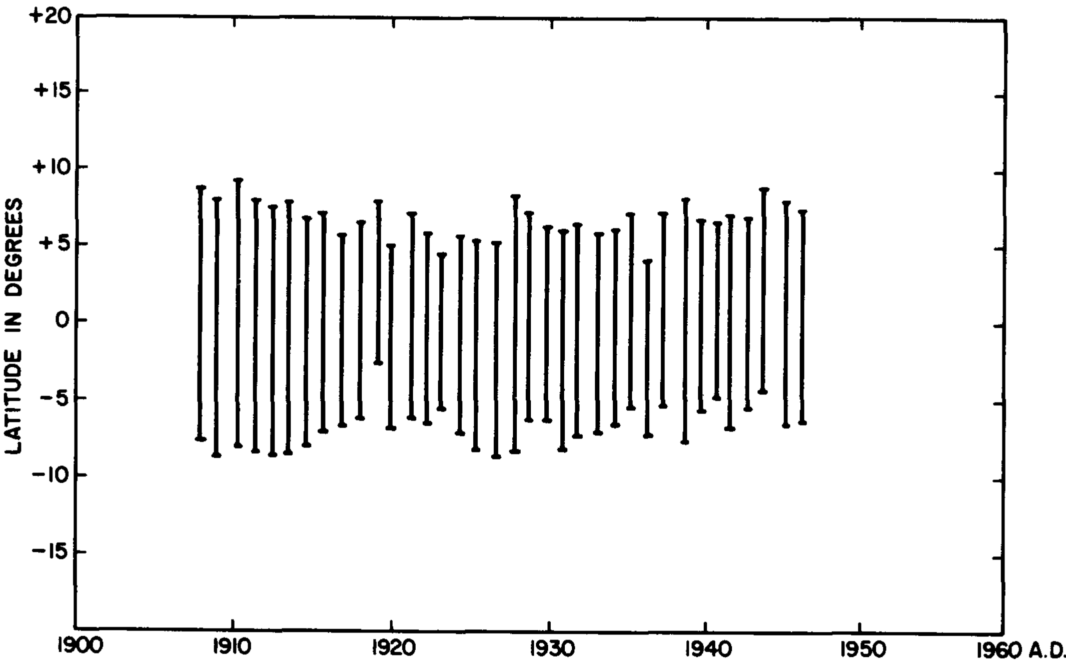

Figure 1. Illustrating apparent variations in width of the Equatorial Zone of Jupiter from 1908 to 1947. Each line indicates the range of latitude occupied by the Zone. (Based on numerical values in Peek, 1958, p. 63.)

Typical values of M are of the order of 10 for Jupiter and Saturn, and it is possible that in having values of M significantly greater than unity the great planets differ in a fundamental way from the rotating terrestrial planets (Mars and Earth), the long lifetime of Jupiter's South Tropical Disturbance and the dramatic acceleration of the South Tropical Disturbance when it encounters the Great Red Spot having been interpreted as observational evidence in support of this suggestion (Hide, 1963; Toomre, unpublished).

The sign of B, let alone its value, is unknown for Jupiter and Saturn. If internal energy sources are comparable with solar heating, B may well be negative, in which case vertical overturning associated with a super-adiabatic lapse rate might constitute the principal mode of hydrodynamical flow.

Finally, a few remarks should be made about the location of the homogeneous dynamo that, presumably, causes Jupiter's general magnetic field. If Jupiter's atmosphere is sufficiently deep, the electrical conductivity of its lower reaches may be sufficiently high for a dynamo mechanism to operate there (cf., Hide, p. 86, this report). This raises the possibility that energy dissipation in Jupiter's atmosphere may be due mainly to ohmic heating, and that the atmosphere may be coupled with the underlying parts of the planet by magnetic fields and not by mechanical friction. Radio-astronomical observations indicate a definite rotation period for the magnetic field. The motion of the magnetic field corresponds, presumably, to the motion of material within the planet at the lowest depth at which the magnetic Reynolds number exceeds about 10 (cf., p. 84, this report). Though the radio-period was apparently constant when first determined, changes have now been detected. The reconciliation of these changes with the motion of other internal parts of the planet (as evinced by excursions in longitude of the Great Red Spot (see Figure 2)) will involve the discussion of angular momentum transfer within the planet. It is not inconceivable that variations in the radio-period and in the Red Spot period are manifestations of a gross torsional hydromagnetic oscillation.

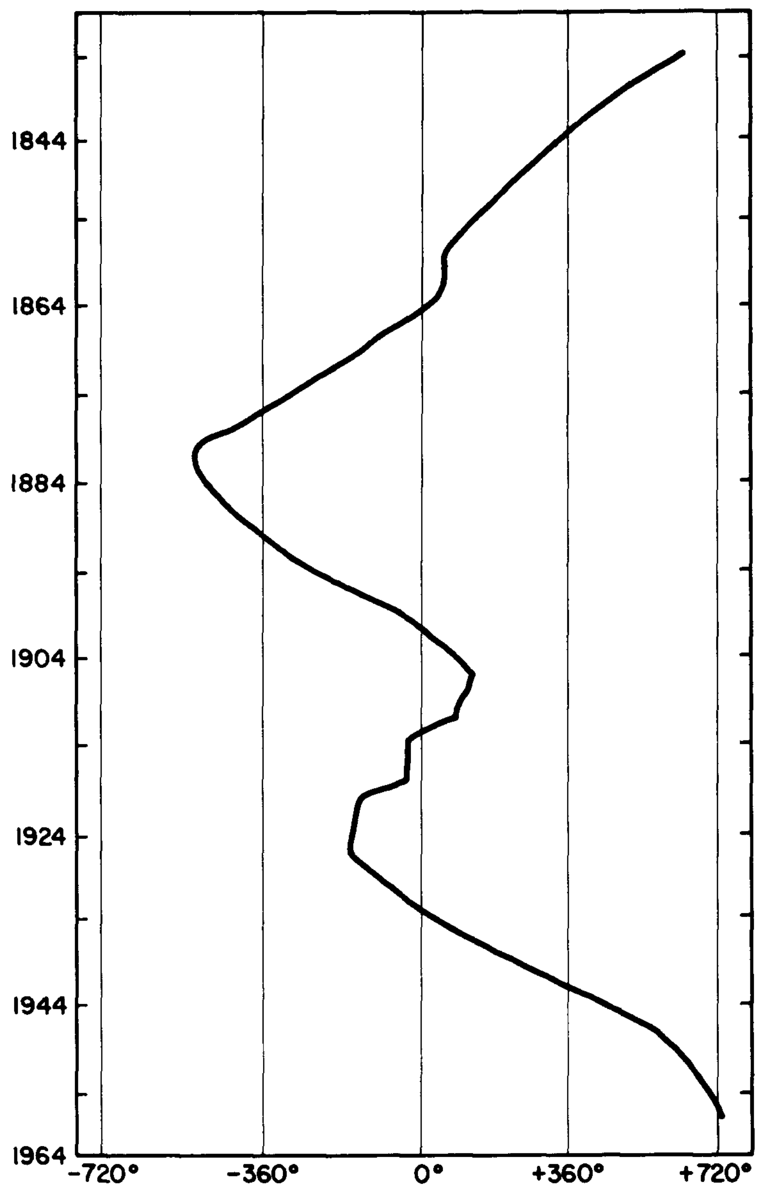

Figure 2. The wanderings of the Great Red Spot in longitude, 1831 to 1960. The abscissa is $\gamma_2 - 264^{\circ}.3 + 28^{\circ}.62t$, where γ_2 is longitude in System II. t is time measured in units of 398.88 days, the mean intervals between oppositions of Jupiter. The ordinate is time in years. (Based on a diagram by Peek, 1958; reproduced here by permission.)

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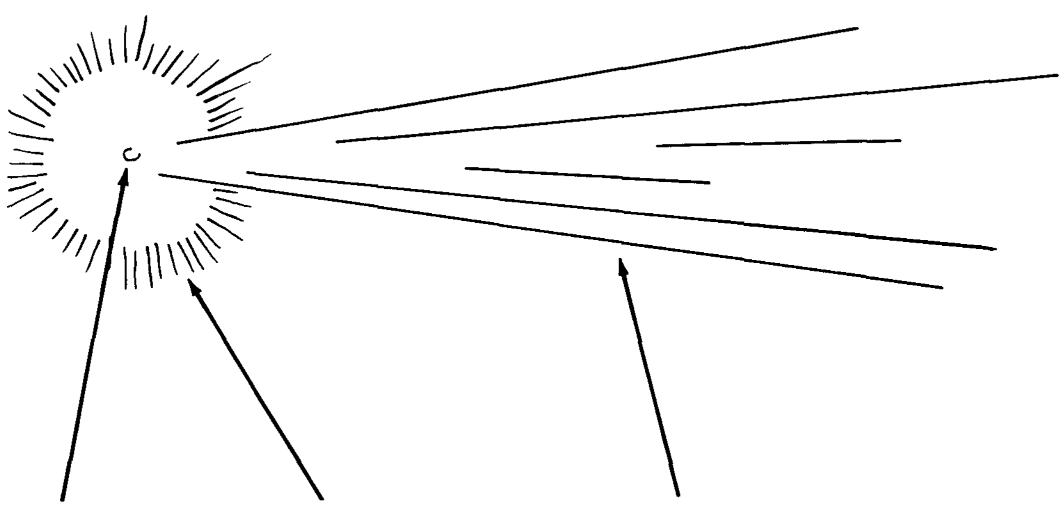
COMETS AND INTERPLANETARY DUST

J.A. Wood

1. Comets - A Review

Comets are, apparently, small (a few kilometers) bodies composed of a mixture of ices and earthy particles, in eccentric orbits about the Sun. Each time a comet nears the Sun some of its ice sublimes away. Inside about 3 AU the escaping gas is visible as a luminous head or coma. Inside about 1.5 AU a tail of escaping ionized gas appears.

Comets vary from one to another, but a large, bright comet near perihelion might have these properties:



Nucleus	Head or Coma	Tail
(A few km) Solid, ice?	$(10^5 - 10^6$ km) Gas and dust streaming out from nucleus	(Up to 10^8 km) Gas and dust streaming out of head, away from Sun.
Bright continuous spectrum from reflected sunlight	Gas molecules excited by sunlight, emit band spectra. Mostly neutral molecules observed: CN, C_2, C_3, NH, OH, NH_2, CH. Lines of OI, Na, (Fe, Ni). Some ionized gas, as in tail. Gas density*: $10^4 - 10^6$ molecules/cm^3 near nucleus	Ionized gas molecules excited by sunlight, emit band spectra. Observed: CO^+, N_2^+, CO_2^+, CH^+, OH^+. Gas density* near nucleus, $\sim 10^3$ molecules/cm^3
		Dust particles reflect continuous spectrum from sunlight

* Very approximate values

There are near-parabolic comets and periodic comets. Orbits of near-parabolic comets may be parabolic or may be elliptic with extremely high eccentricity, reaching aphelion 10^4 to 10^5 AU out, with periods of many thousands of years. None is hyperbolic. We see each near-parabolic comet only once, unexpectedly. An average of four a year are observed. They approach from directions essentially isotropically distributed about the Sun, and half of them move in retrograde orbits.

The near-parabolic comets are, apparently, bona fide members of the solar system, existing in vast numbers in a shell or cloud about the Sun, about 10^5 AU in radius. It has been suggested by Öpik and Oort that passing stars occasionally perturb members of this comet shell into sunward orbits. It seems improbable that comets are captured from interstellar space or from other stellar systems: if this were the case, they should travel in markedly hyperbolic orbits.

Periodic comets are, apparently, near-parabolic comets that were perturbed into short-period elliptic orbits by the planets after they entered the solar system. Perturbation operates selectively, producing rather regular short-period orbits from the randomly oriented parabolic orbits. Most periodic comets have direct motion, $i = 0 - 20^\circ$, period = 5 - 9 years, perihelion distance = 1 - 2 AU, aphelion distance = 5 - 6 AU, $e = 0.5 - 0.8$. Fifty-four have been seen to make two or more appearances, and about four per year appear (predictably) inside the orbit of Mars.

After a finite number (perhaps about a hundred) of approaches to the Sun, a comet's ices are, of course, exhausted. Presumably it ceases to exist. Conceivably, however, an earthy nonvolatile residue is left. The asteroid Hidalgo has a comet-like orbit, and might be such a residue.

2. Comets - Questions

Except for their orbital elements, our knowledge of comets is fragmentary in the extreme.

Composition

1) Composition, temperature, and physical properties of the nucleus are unknown. The very existance of a solid nucleus (Whipple's "icy conglomerate" model) is unproven, though it is highly probable.

2) Composition of coma and tail gases is partly known. Resonance spectra of many possible molecules (H_2, N_2, CO, NO, NH_3, H_2O, CH_2, CH_3, CH_4, NH^+) do not fall in the observable region.

3) Composition of dust is all but unknown. Size distribution and density estimates have been made from polarization, intensity of continuous spectrum, and meteors.

Processes

1) Formation of head gases and tail plasma from parent molecules in the nucleus: release mechanism, chemical reactions, dissociation, ionization are virtually unknown. Photoionization alone is inadequate, but probably plays some part. Ionization is at present thought to be due to the solar wind, through a poorly understood shock phenomenon. Photodissociation is likely.

2) Spectral emission by gas and plasma molecules is undoubtedly fluorescence excited by solar electromagnetic radiation.

3) Acceleration of tail away from Sun:

(a) Dust tails are probably due to solar light pressure.

(b) Gas tails—light pressure is inadequate; the solar wind is probably responsible. However, the mechanism for momentum transfer is ill-understood.

4) Bursts of cometary activity and ray structure in tails are ill-understood.

3. Importance of Comet Studies

We can distinguish two important goals in the study of comets. The first would be simply to fill the gaps in our knowledge of comets outlined above; to come to an understanding of them as a natural phenomenon. Much can be done to this end by ground-based studies and Earth satellite experiments (4 - 6 below). Most valuable would be one or more comet fly-by probes, relatively modest Mariner-class missions (7 below).

The second goal would be a thoroughgoing study of the material in a comet nucleus, not so much for the sake of understanding comets, but because we may gain valuable insight into the primordial state of the solar system. It appears impossible that comets could be forming in the solar system as it is presently constituted, and improbable on dynamic grounds that they originate outside the solar system. The idea that they are surviving condensations from the primordial solar nebula has gained considerable currency. If this is so, they are of very great interest to us, being primordial material that is:

1) Uncommonly well preserved, apparently having been in deep freeze since the beginning (whereas most meteorites, which are widely held to be primitive planetary material, show evidence of secondary thermal effects).

2) Relatively unfractionated, containing both earthy substances and (as ices) volatile compounds of C, N, and H (whereas meteorites are all but devoid of these elements, and severely depleted in many elements of moderate volatility as well, such as Hg, Cd, In, I, Tl, Bi).

Obviously, achievement of this goal entails rendezvous with a comet nucleus, a far more ambitious mission than a fly-by. Further, the detailed isotopic, chemical, and structural studies we would want to make on primordial solar system material could not be conducted remotely: a sample return would be necessary (8 below).

4. Ground-Based Study

A great deal of information can still be obtained from ground-based studies:

1) Improved classical astronomy
2) Infrared and ultraviolet spectroscopy from balloons
3) Polarization studies of comets

4) Radio observations of comets

5) Occultation of stars behind comets (in the visible); radio observation of occultation of radio stars

6) Laboratory studies of molecular gases—additional compounds, extended wavelength ranges (infrared and ultraviolet), f-values, cross sections for dissociation, ionization

7) Laboratory studies of ice systems in vacuum, subject to solar-type radiation, proton fluxes, etc.

5. Sounding Rockets

A widely advocated experiment is the release at high altitude of gases that are expected to be stable parent molecules in comets, so their photodissociation and photoionization by solar radiation may be studied. Likely parent gases include NH_3, H_2O, CO_2, CO. They would be released singly or in appropriate mixtures, at an altitude in excess of 400 km (to avoid reaction with atmospheric atomic oxygen). Preliminary experiments of this type have already been conducted.

Ability to make the appropriate spectral measurements from the ground seems marginal, even when massive amounts of gas are released. A more promising approach would be to carry cameras and optical spectrographs up with the gas to be released, and make measurements from close range. For example, one instrument package might be positioned inside the expanding gas cloud and another 50 km away.

6. Earth Satellites

Or, going one step further, a model of a comet nucleus could be made of an ice or mixture of ices and orbited about the Earth. A one-ton iceball would survive sublimation for several days. Again, instruments in a nearby satellite or indeed attached to the nucleus would learn more than ground-based instruments. The object would be to learn the space densities of the various neutral and ionized molecules as a function of distance from the nucleus. An orbiting mass spectrometer would be valuable for this purpose, in addition to optical spectrographs.

Observations of genuine comets from orbiting astronomical observatories, especially in the far ultraviolet, would be valuable but difficult.

7. Comet Probes: Fly-by Mission

The above notwithstanding, really important increases in our knowledge of comets will probably require space probes. As already noted, about four each per year of parabolic comets and periodic comets appear inside Mars' orbit. Of these, one or two per year, on the average, are in suitable Earth-opposition to be reached by a Mariner-type planetary probe. To a first approximation, payload possibilities would be similar to the Mars Mariner mission (Atlas-Agena, 575 lb spacecraft, 60 lbs of instruments; Atlas-Centaur, 2000 lb spacecraft, perhaps 1000 lbs of instruments).

Our present knowledge of orbital parameters of comets is inadequate to achieve interception by a space probe. If a cometary mission is contemplated in the future, it is important that the target comet be selected and observations begun to refine its orbit as soon as possible.

Encounter velocity would be 20 to 30 km/sec. Ideally, the spacecraft should pass through the coma, then the root of the tail, within 10^4 km of the nucleus, then out through the coma on the other side. Traversal would last a few hours. We would want to measure, as a function of position in the comet:

Total gas density
Ion density
Composition and relative abundance of gas molecules (mass spectrometry of major constituents seems feasible)
Space density and size distribution of solid particles
Magnetic field

From ion density and magnetic field measurements, we would hope to learn about the mode of coupling of the solar wind with tail plasma. There is also a possibility that the nucleus itself has a magnetic field. Gas and ion densities and compositions would, of course, inform us about the composition of the nucleus and the progressive dissociation and ionization of outflowing gases.

It would be desirable to learn something about the nucleus, at least to confirm the existence of a solid nucleus. Television photography might be worthwhile. At 10,000 km a 20-cm telescope would resolve 100-m features on the (sunlit) surface of the nucleus. We might be fortunate enough to observe cometary burst activity.

8. Landing on a Comet Nucleus

To land a spacecraft on a comet, it would be necessarv to match their orbits.The thrust requirements for such a maneuver are great. Typically, a periodic comet travels at about 27 km/sec at 1.75 AU, while a spacecraft reaching aphelion at the same point (perihelion - 1 AU) would be moving at only 19 km/sec. Calculations of rocket performance and cost (given in "Launch Vehicle Estimating Factors", June 1965, Office of Space Science and Applications, National Aeronautics and Space Administration) indicate the possibilities shown in Table 1.

We have about one opportunity per year to make a landing on a periodic comet. Near-parabolic comets arrive in comparable numbers, but the randomness of their direction of approach means that rarely can a substantial fraction of the Earth's orbital velocity (which we are given free to begin with) be applied to matching the cometary velocity vector. In fact it works against us for retrograde comets. Thus, the thrust that would have to be applied to the spacecraft would be impracticably large except for a small fraction of the near-parabolic comets, perhaps 10% of them. This would leave us only one opportunity per decade or two, and that always without warning. This restriction makes landing on a near-parabolic comet unlikely—regrettably, because their nuclei should be less shopworn than periodic nuclei, which have lost much of their substance, probably selectively, during earlier passes.

Table 1

Launch Vehicle*	Spacecraft Weight, lbs: To favorable periodic comets	To favorable near-parabolic comets	Cost per launch (10^6 $)
1970-74:			
SLV3X - Kick	250	--	9
SLV3X - Centaur - Kick	1100	300	17
Saturn IB - Centaur	2400	(700)	38
Saturn IB - Centaur - Kick	4500	2300	43
1975-79:			
SLV3X - Centaur F - Kick	1600	600	19
Saturn IB - Centaur F	5000	3500	40

* Note on rocket stage names: SLV3X is an improved version of the Atlas; it is expected that an auxiliary kick stage will be added to the SLV3X and all other vehicles that include the Centaur stages; Centaur F is an improved Centaur.

As noted in 3 above, our principal motivation in landing on a nucleus would be to return a sample to Earth. By using a large booster and choosing a comet with a relatively "easy" orbit, this appear to be marginally possible. Spacecraft weight permitting, it might also be possible to include some of the experiments that were listed for a fly-by mission.

9. Dust

Interplanetary dust is believed to be cometary debris for the most part. It would seem we could obtain cometary matter cheaply by orbiting a Pegasus-like satellite of large area during a period of meteor shower activity (i.e., high dust density), collecting dust in its paddles, folding it up and bringing it back to Earth. However, the relative velocity between Earth and dust is again about 30 km/sec. This means particles would strike the spacecraft with kinetic energy about 100 kcal/gm but the heat of vaporization of silicate material is only about 2 kcal/gm. To collect reasonably intact dust from space, relative velocity would have to be reduced to a small fraction of 30 km/sec. This amounts to putting the spacecraft in and out of a cometary orbit, which is almost as difficult as rendezvous with a comet and return of a sample.

Efforts have been and are being made to collect cometary dust after its velocity is checked by the upper atmosphere. Only small grains (less than about 5 microns in size) survive atmospheric deceleration without being vaporized or melted. These collection programs are of great value, but are not to be compared with the acquisition of a sample from a comet. Micrometeorite collection yields microgram amounts of a particular size fraction of cometary dust, to some extent thermally degraded and to some extent contaminated. The sample return contemplated would provide us with bulk amounts of dust and ice, the former fraction unsorted, undamaged and uncontaminated.

Much could be learned about the zodiacal dust cloud by making brightness and polarization measurements from planetary probes, at various distances from the Sun, and coupling these data with particle impact count rates.

THE EARTH VIEWED FROM SPACE

Leo Steg

Introduction

In recent years, notable advances in observational techniques have yielded much information on the features of planetary surfaces and atmospheres.

Infrared photometry permits temperature measurements of the cloud layer topology of Venus, Jupiter, and Saturn, as well as the solid surfaces of Mercury and Mars. The apparent temperature of the solid surface of Venus is measured by radiometry at radio frequencies; nonthermal radio emissions have been observed from Jupiter and Mars; the surface features of Mercury and Mars have been studied.

Much effort has been spent in attempts to deduce Martian atmospheric and surface features, and much information is available; more quantitative work is needed on other subjects, such as atmospheric scattering.

The study of the Earth as a planet seen from space remains interesting. There are many practical applications of global viewing, such as meteorology and global reconnaissance; in addition, quantitative predictions of the Earth's appearance from outside the atmosphere are of interest. This information could then be compared with actual observations from various positions, view angles, and spectral regions. Many astronomical techniques for studying other planets have been used to view the Earth from low orbiting satellites. In many cases, there is relatively little quantitative data that would serve either to confirm the theoretical assumptions or prove the experimental techniques.

This paper covers general problems in the areas of geodesy, ionospheric and radio physics, and planetary atmospheres. Geology is treated separately.

Geodesy

Some of the salient topics are:

(i) determination of the shape and size of the Earth
(ii) determination of the gravitational field, both in general and in fine structure

The detailed geometry of the geoid as a function of latitude and longitude and its stability are important because they may illuminate the elastic and plastic properties and behavior of Earth as a solid body. The development of an accurate microwave altimeter, (1-cm resolution) recommended by von Arx, would materially aid in these investigations.

Ionosphere and Radiophysics

Some study of the Earth by means of ultra-high-frequency and microwave radiometry has been made and these measurements will also be relied on in the study of other planets from orbiters and fly-bys. In the case of the Earth, the possibility exists for comparison of the observations with predictions based on measurements of temperature and emissivity of surface materials, and absorption by atmospheric constituents, such as oxygen and water vapor. Interference from ground-based radars may introduce complications, but no satellite data of the power spectrum of this interference, covering large regions of the Earth over extensive periods of time, has appeared in the literature. Such surveys should be undertaken, both to evaluate the background interference, and to attempt correlations with predicted emission and atmospheric absorption mechanisms.

Radar mapping at various wavelengths might be employed. Such mapping could aid in the large-scale determination of the sea state, and could test methods for determining variation with depth of conductivity and dielectric constant for extended soils and rocks. Such techniques, besides possible usefulness in surveys of terrestrial resources, are needed by orbiters for remote geophysical sensing of such bodies as the Moon.

In the high-frequency regime, where ionospheric absorption sets in, studies should be made of possible radio emission from the Earth's atmosphere and ionosphere that may be due to sferics and, perhaps, plasma oscillations. Ionospheric leakage studies should be made for signals originating from ground-based transmitters, both to check the theory for Z-mode propagation, and to understand possible man-made interference effects in the study of natural radio phenomena at these frquencies.

A correlation of very-low-frequency studies of whistlers with magnetic field measurements and studies of energetic charged particles and plasmas could prove significant.

Investigation of the processes in the terrestrial ionosphere might prove gratifying and the question of the chemical reactions involved in ion recombinations remains of interest. Striking uses of satellites might be found; a multifrequency beacon in synchronous orbit for the detailed monitoring of electron content may prove to be an excellent supplement to top-side sounders.

Atmosphere

The general characteristics of the Earth's atmosphere remain of interest: temperature, density, and scale height, particularly in equatorial and polar regions.

Optical studies of the aurora performed simultaneously with measurements of charged particle precipitation with satellites, would shed light on the basic processes involved. Simultaneous photography of auroral patterns over large areas of the auroral zone, using polar orbits, might help to resolve problems such as the injection of solar plasma into the geomagnetic field.

Studies of airglow could be performed by using well-collimated spectrometers to view the upper atmosphere tangentially. These measurements should be supplemented with measurements of solar x-ray and ultraviolet fluxes.

Calculations of the effects of Rayleigh scattering on the intensity emerging from the top of a planetary atmosphere have, in some cases, made use of actual measurements of the reflectance properties of extended Earth surface areas, such as sand and clay. Since the polarizing properties of surface and atmospheric scattering are, in general, quite different, measurements of the polarization of the emergent light by means of Earth satellites will be required to confirm the assumptions made in some of these calculations. For example, measurements should be made through a clear atmosphere in order to determine the extent to which aerosol scattering can be neglected.

Another area in which extensive measurements and calculations should be undertaken is the study of scattering by atmospheric aerosols. Both extensive sampling programs and scattering calculations are desirable. Comparisons of light intensity and polarization measurements from above the atmosphere, with calculations based on known ground reflectance properties and calculations for a Rayleigh atmosphere, would allow an approximation to total aerosol scattering effects, provided the measurements are made with sufficient precision at a number of wavelengths. An ultimate application of such new knowledge could be the detection and evaluation of aerosols over extensive Earth areas from Earth satellites.

The application of satellite radiometry measurements to heat balance would be greatly aided by some of these studies.

Optical and Infrared Observations from Above the Atmosphere

Many photographs of the Earth have been taken from space, but the most extensive optical observations of large regions of the Earth from above the atmosphere are those that have been obtained from the Tiros and Nimbus satellite programs.

While large amounts of data have been gathered in the form of pictures and radiometer maps, both in the visible and infrared, quantitative interpretation of these data has been to a large extent lacking. In many cases, basic data are unavailable regarding the intensity of light reflected from typical regions of the Earth's surface as a function of wavelength, and phase angle. For example, the brightness distribution over desert regions is certainly affected by the general property of rough materials to have maximum optical reflectance toward the illuminating source. For most Earth surface materials, reflectances as a function of illumination and viewing angles are not available. In addition, the effects of the atmosphere in terms of diffuse scattering and reduction of contrast between different surface areas are poorly understood.

It would be desirable to provide a quantitative basis for the extensive optical measurements being made of the Earth from space. This would require a comprehensive set of measurements of reflectance and polarization of materials typical of large regions of the Earth's surface and of the effects of atmospheric scattering. This should include measurements at a number of discrete wavelengths on materials such as the sea in various states, various soils, vegetation, and snow and ice. Airplane measurements of reflection characteristics of various cloud covers should also be extended.

In addition to these measurements, provision should be made for the in-flight calibration of radiometers used to observe the Earth from satellites. The lack of such provisions in the earlier meteorological satellites has greatly reduced the value of the information obtained from them.

Extensive surveys of the Earth's surface, using infrared radiometry, including in-flight calibration procedures, would allow more precise studies of the Earth's heat balance. Supporting laboratory measurements should be made of the infrared emittance and directional characteristics of materials typical of extended areas of the Earth's surface. Such calculations would allow direct measurement of surface temperature and a check on the Lambert assumption for wavelengths in the atmospheric windows.

Studies of line absorption and emission by atmospheric gases, such as water vapor and carbon dioxide, remain of interest, both to support future studies in aeronomy with Earth satellites and to calibrate methods that will later be applied to studies of other planetary atmospheres from orbiters.

Conclusions

The role of the Earth in the planetary exploration program can be viewed from at least three aspects:

1) Earth (and its extended atmosphere) remains a scientifically attractive subject. Only superficial utilization has been made of the capabilities of satellite-based (or probe-based) observation coupled with ground-based balloon and rocket observations to test theory and yield new data.

2) Planetary exploration generally requires advances in observational technology and data interpretation. Earth and the cislunar environment provide an excellent testing and proving ground for the scientific and technical rigors of planetary exploration. Little should fly to Mars and Venus that has not been tested (and interpreted) in Earth orbit, where relevant.

3) The most immediate and predictable applications of space exploration—of significance to the people at large—comes from Earth orbiting satellites.

In view of the relatively adequate and broad base of the present NASA program, no major changes are indicated. The three views expressed above should serve as a frame of reference for all planetary observation.

GEOLOGY OF THE EARTH'S SURFACE AS VIEWED FROM SPACE

D. U. Wise

The numerous manned and unmanned large orbiting laboratories planned for the next few years will result in a vast number of multispectral observations of the Earth. Unlike the lunar and deeper space probes, where only the most critical and imaginative experiments can be undertaken because of high cost of the programs, the relatively inexpensive near-Earth observations will be done in large numbers with every conceivable type of sensor. Many of the spacecraft and orbits will be chosen with specific Earth-viewing experiments in mind; other less critical experiments will fly on engineering test vehicles as the choice between a sandbag and an instrument.

Scientific observation of the Earth from space is now receiving such massive consideration by such a variety of organizations that it is virtually impossible to suggest a type of observation that someone has not proposed previously. Design, fabrication, and testing of many kinds of remote sensing equipment are now underway with possible applications to geology, meteorology, oceanography, biology, and geography. Mere listing of the possibilities constitutes a considerable catalogue.

Selection of Projects

Of the vast number of experiments possible, a few seem worthy of particular mention:

1) The mass and thermal budget of the ice caps seems most suitable to orbital mapping procedures.

2) Fracture and tectonic pattern analysis of the broadest kind can be done through generalizations of vast areas produced by radar imagery. Possibilities for mineral orientation mapping exist among some of the polarizing techniques.

3) Synoptic sea state studies of entire oceans are possible on a day-to-day basis of coverage.

The experiments selected for flight should take advantage of the unique opportunity provided by an orbiting spacecraft to obtain data that can be gathered in no other way. Chief among these advantages are:

Area of coverage. The detection and mapping of very subtle changes over areas of subcontinental size is possible because large areas are visible simultaneously from orbital altitudes. Many large-scale, but very faint linear elements or density gradients can be detected best in these overall views.

Speed of coverage. Spacecraft speed permits an essentially synoptic view of a continent or ocean. Meteorological conditions, sea state, soil moisture after a storm, or thermal pattern are all possibilities.

Long-term surveillance. Ordinarily, airborne equipment is flown over a region only once and the cost of continuing surveillance by that method becomes prohibitive. The coverage provided by successive orbits or even synchronous orbit of a spacecraft make continuous observation or monitoring of large areas practicable.

Accessibility and precision of location for remote regions. The difficulties of using airborne equipment in remote regions without good horizontal or vertical control are obvious. The fact that an orbiting spacecraft can make the same observations and that its position can be known to about ten feet makes it a much better platform for many forms of reconnaissance mapping.

The main limitation in doing terrestrial geology from space is the strong attenuation of short wavelengths by the Earth's atmosphere. Almost all orbiting, downward-looking experiments using gamma rays, x rays, and most of the ultraviolet are precluded. Sensors capable of measuring these properties will probably be put into Earth orbit, but they will be merely engineering tests for equipment destined for other bodies that do not have the same atmospheric constraints.

Geologic Suggestions

The more useful terrestrial geologic data will come from the wavelengths of the photographically visible and longer. At present there is a need for ultra-high-altitude conventional aerial photography, including infrared for haze penetration, for use in regional tectonic analysis. Detection of subtle lineaments, finding extensions of known linear structures and seeing interrelationships of known linear features should all be done at many possible scales of viewing. At present the ultra-high-altitude scale is the only one denied to us. An example of the kind of information that it is possible to obtain from a new scale of viewing is Raisz' discovery of the Olympic-Wallawa lineament by painstaking integration of sketches of topographic elements from aerial photographs of the Washington-Idaho region. Complete coverage of North America at 1: 2,000,000 or 1: 3,000,000 scale photography could easily be achieved. (The photographs could be published in atlas form, as single sheets, or as a single 6 or 8 foot wide wall map).

Radar mapping will be a very useful supplement to coverage at photographically visible wavelengths. These wavelengths, in addition to penetrating haze, cloud, and vegetation cover, are capable of penetrating a meter or more into the soil cover. The result is a view of the Earth uncluttered by the ordinary detail of aerial photographs, and appearing more like a pristine planetary surface. The technique can be used in rapid coverage of huge areas without loss of detail, the main limitation being the power supply necessary to generate the outgoing pulse. For this reason, high-altitude aircraft may be more efficient for some forms of radar mapping than spacecraft.

Several test sites are now under investigation to determine the characteristics of the various sensors and the ways in which their data can be interpreted. As these sensors are developed and put into space in the next few years, it will be important to do sample mapping of the major classes of tectonic zones. These would include:

Major strike-slip fault zone (probably the San Andreas)
An island arc system (Probably the Aleutians or West Indies)
Rift Valley of Africa
Tectonic zones of an alpine mountain system (probably the Appalachians)

This would include fold and fracture patterns, mineral orientations and, possibly, an examination of the problem of curvature and offset.

These remote-sensing image studies will supplement existing geologic maps with respect to accuracy of boundaries, areal distribution of soils and bedrock, shallowly buried structures, and structural grain of various types. Geologic maps of such well-known areas as parts of the British Isles have already been revised on the basis of radar photographs. The possibilities open in less well-known regions to the new highly sophisticated sensors will represent a major breakthrough for geologic mapping —provided the data become widely available in an orderly and usable fashion. At present, the state of the art and the potential usefulness of the data are such as to make a side-scan radar map of the United States a feasible and logical objective of a combined NASA, U.S. Geological Survey, and state geological survey program. (The directors of both the Pennsylvania and Maryland Geological Surveys have already expressed interest in state mapping of this type). This first step of systematic mapping by side-scan radar would be followed by other types of sensor mapping as the instruments reach suitable stages of development.

In considering the experiments in which the Earth is viewed from space, it is important to recognize that these are, for the most part, relatively mundane extensions of observations being made or soon to be made from the ground or from aircraft; that they will be far less glamorous than the exploration of the frontiers of the solar system; and that much of the information will be of a practical and immediately useful nature to the public at large. However, utility is no cause for depreciating these experiments for they are also good science. Ultimately, they will make possible a much clearer understanding of our own planet. Consequently, these mundane experiments deserve endorsement in their own right as well as for the support they provide to their more spectacular deep space relatives.

INTERACTION OF THE SOLAR WIND WITH THE PLANETS

Paul J. Coleman, Jr.

Introduction

The characteristics of the flow of the solar wind, a tenuous magnetized plasma emitted continuously by the Sun, are known to be altered greatly when the plasma encounters an obstacle of dimensions in excess of 100,000 kilometers. The magnetosphere of the Earth presents an obstacle of such size. In studies of magnetohydrodynamics, properties of the resulting interaction between the solar wind and the magnetosphere are of interest for a number of reasons. In connection with planetary exploration, the reason for much of our interest is a practical one, namely, that the interaction creates disturbances in a large volume surrounding the obstacle, thereby increasing the detectability of a planetary magnetic field.

In the following, the observations of the properties of the interplanetary medium and the development of theoretical models for the solar wind will be summarized. Next, from the available measurements, parameters typical of a quiet solar wind will be calculated. The interaction of the solar wind with the Earth's dipolar magnetic field will then be described, and the method employed in scaling the observed effects for interactions with dipolar fields of different moments will be given. Finally, some speculations concerning interactions of the solar wind with planetary bodies not shielded by magnetic fields will be summarized.

Background

Just before the turn of the century, Birkeland (1896) theorized that auroras and geomagnetic storms were produced by charged particles streaming from the Sun and interacting with the geomagnetic field. Shortly after 1900, Störmer undertook a more detailed analysis of the trajectories that such charged particles would follow after entering the dipolar field of the Earth, in order to compare these trajectories to the auroral shapes (Störmer, 1955). About thirty years later Chapman suggested that the auroras and geomagnetic activity were produced by ionized gas or plasma, rather than by more energetic charged particles, emitted from the Sun during solar flares and reaching the Earth a few days later. Subsequently, Chapman and Ferraro (1931, 1932, 1933, 1940) developed specific models for the mechanisms responsible for these solar-terrestrial effects. The possibility that magnetic fields from the Sun might be carried into interplanetary space by the solar plasma was discussed by Alfvén (1950), who considered this possibility in an effort to establish Forbush decreases in the intensity of cosmic rays as phenomena effected by plasma streams produced by the active Sun.

The models for the interplanetary medium that resulted from this earlier work, based upon observations of solar-terrestrial effects, differ from the models presently accepted mainly in that localized, intermittent streams of solar plasma were included in the former while, in the latter, a constantly flowing solar wind is the essential feature.

According to Scarf (1964), the development of the more recent models of the solar wind and its source was stimulated by the observation of Edlén (1942) that the temperature of the lower corona is of order 10^6 degrees and by the observation of Wurm (1943) that comet tails of Type I are accelerated outward from the Sun. This latter observation led Biermann (1951, 1952, 1953, 1957) to assert that the outward flow of solar corpuscular radiation is a continuous process and that the resulting flux of particles increases during periods of solar activity. The observation of relatively high temperatures in the solar corona led Chapman (1957) to develop a static model that suggested the great extent of the corona.

Parker (1958), in seeking consistency with both observations, described a hydrodynamic model that included a solar wind with a radial velocity of order 10^2 km/sec at the orbit of the Earth. Chamberlain (1960, 1961) developed hydrodynamic models in which the velocities were an order of magnitude smaller. Noble and Scarf (1963) also provided solutions to the fluid equations that yield a fast solar wind.

Many other indications of the properties of the interplanetary medium have resulted from studies of Earth-based observations, such as the observations of comet tails, mentioned previously, the polarized component of the zodiacal light, and the position of the gegenschein. Other information, still indirect, has been obtained with particle detectors and magnetometers on board near-Earth satellites and from measurements of satellite drag.

The initial Soviet and American efforts in the exploration of deep space by means of instrumented spacecraft permitted measurements that directly confirmed the existence of ionized gas and magnetic fields in interplanetary space and provided details concerning various properties of this medium. In January and September 1959, the first direct measurements of the solar plasma flux were recorded by plasma detectors of the Faraday-cup type on board Luniks 1 and 2 (Gringauz et al., 1960). In March 1960, the initial measurements of the interplanetary magnetic field were obtained with a search-coil magnetometer on board Pioneer V (Coleman et al., 1960).

Mariner II, launched in August 1962, carried a plasma probe of the electrostatic analyzer type as well as a magnetometer that was designed to provide more detailed measurements of the interplanetary magnetic field and to facilitate a search for a magnetic field from Venus. Among the results obtained with these instruments was direct evidence that the solar wind flows continuously from the Sun to at least the orbit of Earth (Neugebauer and Snyder, 1962) with velocities in the range 300-900 km/sec, and that the solar wind plasma nearly always contains magnetic fields of strengths of order 1 γ (Coleman et al., 1962).

These results, and other supporting observations obtained in the last few years, provide strong support for Parker's solar-wind model for the coronal expansion. One other feature of Parker's model is an interplanetary magnetic field in which the lines of flux of the solar field are carried along in the highly conducting solar wind. Beyond some heliocentric range the gas cannot rotate with the Sun so that a spherically symmetrical outflow is approached. Since the field lines in this model remain connected to a point on the Sun, each describes a spiral (Figure 1) in the region of radially symmetric flow. Thus, in a spherical coordinate system, the field at a point (r, θ, φ) will make an angle with the radius vector from the Sun given by $\alpha_p \approx \tan^{-1} (\omega_S r \sin \theta / V_p)$, where ω_S is the Sun's angular velocity, V_p is the velocity of the solar wind (assumed to be radial), and r is the distance from the Sun, or more accurately, the distance from the inner boundary of the region of spherically symmetric flow. The measurements of the interplanetary field made to date have provided results that are for the most part consistent with this spiral-field model. These measurements will be discussed in the following section.

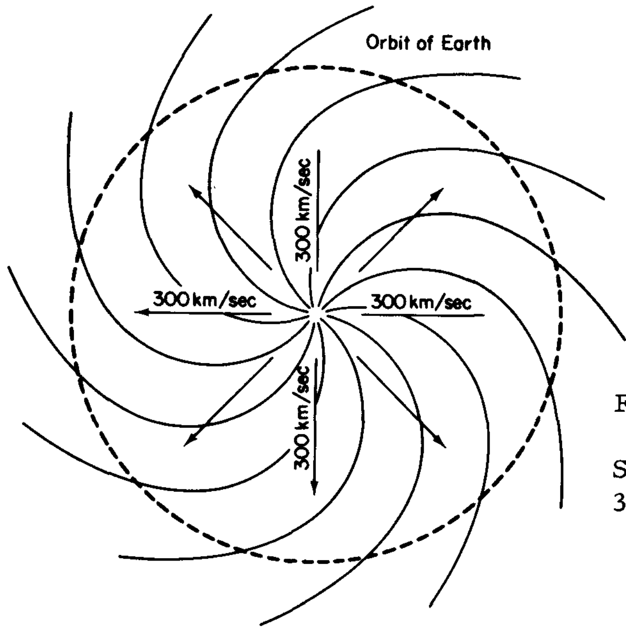


Figure 1. Spiral-field lines of force.
Solar equatorial plane, V_p = 300 km/sec.

Properties of the Solar Wind

Direct measurements of the solar wind have been obtained between the orbits of Venus and Mars at low solar latitudes. The most extensive measurements reported to date (Snyder et al., 1963) were obtained with the Mariner II plasma probe between the Earth and Venus. For the four-month duration of the flight, the radial velocity of the plasma varied between 300 and 800 km/sec. The density of the plasma was calculated under the assumptions that the bulk velocity, V_p, of the solar wind is, in fact, radial and, therefore, normal to the aperture of the plasma probe, and that the temperature associated with the transverse motion of the gas is zero. The values varied from 0.5 to 10 cm^{-3}. Under the additional assumption that the distribution of the radial velocity about V_p is Maxwellian, the temperature associated with the radial motion of the ions was found to vary between $6 \cdot 10^4$ and $5 \cdot 10^5$ °K. The ranges given above for the various parameters of the solar wind plasma apply to over 90 percent of the measurements. However, values beyond these ranges were recorded during a few periods of higher solar activity.

The directional spectral intensity, given in terms of energy per unit charge, often shows two peaks, suggesting thereby the presence of a significant α-particle component. However, a reliable determination of the ratio of number density of α-particles to that of protons was difficult to obtain from the Mariner II results because of the energy resolution employed in the plasma detector. More recent results (Wolfe, 1965), obtained with Earth satellites in orbits that extend into the nearby interplanetary medium, indicate that this ratio is of order 10^{-2}.

No significant dependence of the solar wind velocity upon heliocentric range could be established from the measurements taken between 0.7 and 1.0 AU. Further, the ion flux averaged over periods of a few days showed an increase with time that was consistent with the expected inverse square dependence upon the heliocentric range. These results lend additional support to the model of the radially flowing solar wind, at least at low solar equatorial latitudes. Solar wind measurements obtained with other satellites and spacecraft have been consistent with this description drawn from the Mariner II results. With Mariner IV direct measurements have been extended to the orbit of Mars. There is some evidence from observations of comet tails that the solar wind extends to 5 AU (Biermann, 1964).

The magnetic field in interplanetary space, to the first order of approximation, does not affect the flow of the unobstructed solar wind. Accordingly, only a few of the observed properties of the field will be mentioned. As mentioned above, the measurements obtained with magnetometers on board the interplanetary spacecraft Mariner II (Coleman et al., 1962) and Mariner IV (Coleman et al., 1965) show that the fields behave, on the average, in a manner consistent with Parker's spiral-field model. This behavior has also been observed in regions just beyond the influence of the Earth and its magnetosphere. (Heppner et al., 1963; Ness et al., 1964). It should be mentioned here that most of the measurements of the interplanetary field indicate the existence of a small but persistent θ component (southward) that is not predicted by the spiral-field model.

The magnitude of the interplanetary field is typically 2 to 8 γ ($1\ \gamma = 10^{-5}$ gauss) at the orbit of Earth. There is evidence, from Mariners II and IV, for an inverse dependence of the field strength upon heliocentric range r. However, because of the variations in the field, it has not yet been possible to determine whether the dependence corresponds quantitatively to that expected, i.e., r^{-2} for the radial component, and r^{-1} for the other two.

From these observations of the interplanetary medium, the following values are selected as typical of the quiet solar wind at 1 AU:

Proton Number Density, $n = 5\ cm^{-3}$

Solar Wind Velocity, $V_p = 500$ km/sec

Proton Temperature, radial, $T = 10^5\ ^\circ K$

Magnetic Field Strength, $B = 5\gamma$

For this case, values of other parameters are:

Directed Energy, proton $^1/_2 m_i V_p^2 = 2.1 \cdot 10^{-9}$ erg = 1300 eV

Particle Thermal Energy per degree of freedom,

$kT/2 = 6.9 \cdot 10^{-12}$ erg = 4.3 eV

Thermal Energy Density per degree of freedom,

$nkT/2 = 3.5 \cdot 10^{-11}$ erg cm^{-3}

Energy Density of Magnetic Field, $B^2/8\pi = 1.0 \cdot 10^{-10}$ erg cm^{-3}

Solar Wind Energy Density,

$(nm_i V_p^2 + 2nkT + B^2/4\pi)/2 \approx nmV_p^2/2 = 1.0 \cdot 10^{-8}$ erg cm-3

Alfvén Velocity, $V_A = B/(4\pi m_i n)^{1/2} = 5 \cdot 10^6$ cm/sec

If we assume that the ion gas and the proton gas each have two degrees of freedom, and that both gases have $T = 10^5\ ^\circ K$, various properties of the particle interactions can be obtained. (See, for examples, Spitzer, 1956; Alfvén and Falthammar, 1963.) In the following, e is the electronic charge in esu, and $\ln \Lambda$ is the Coulomb logarithm.

Debye Length, $\lambda_D = (kT/4\pi ne^2)^{1/2} = 9.8 \cdot 10^2$ cm

Plasma Frequency, $\omega_p = (4\pi ne^2/m_e)^{1/2} = 1.3 \cdot 10^5$ sec^{-1}

Thermal Velocities:

electron (2 degrees of freedom), $v_e = 1.8 \cdot 10^8$ cm/sec

proton (2 degrees of freedom), $v_i = 4.2 \cdot 10^6$ cm/sec

Gyro Frequency, electron, $\Omega_e = eB/m_e c = 8.8 \cdot 10^2$ sec^{-1}

Gyro Frequency, electron, $\Omega_i = eB/m_p c = 4.8 \cdot 10^{-1}$ sec^{-1}

Neglecting the bulk velocity, V_p:

Larmor Radius, electron, $R_e = v_e/\Omega_e = 2.0 \cdot 10^5$ cm

Larmor Radius, proton, $R_i = v_i/\Omega_i = 8.5 \cdot 10^6$ cm

Relaxation Times:

Self-Collision Time, electrons, $\tau_e = m_e{}^2 v_e{}^3/0.714 e^4 n\, 8\pi \ln \Lambda$
$= 3.8 \cdot 10^5$ sec

Self-Collision Time, protons, $\tau_p = 1.6 \cdot 10^7$ sec

Equipartition Time, $\tau = m_e m_i \dfrac{\left[(2kT_e/m_e) + (2kT_i/m_i)\right]^{3/2}}{e_e{}^2 e_i{}^2 n (6\pi)^{1/2}\, 8 \ln \Lambda}$

For $T_e = T_i$ and $e_e = e_i$

$\tau = 0.52\,(m_i/m_e)^{1/2}\,\tau_i = 3.5 \cdot 10^8$ sec

Mean Free Path, protons or electrons, $\lambda_F = \lambda_e v_e = 6.8 \cdot 10^{11}$ cm

Speed of Sound, $V_S = (2kT/m_i)^{1/2} = 4 \cdot 10^6$ cm/sec

For particles with velocity V_P:

Larmor Radius, electron, $5.7 \cdot 10^4$ cm

Larmor Radius, proton, $1.0 \cdot 10^8$ cm

Interactions with Planetary Bodies

With the properties of a typical solar wind established at r = 1 AU, we may now consider the types of interactions that might be expected when such a medium encounters a planet. In the event that the planet has a relatively strong magnetic field, the interaction should be similar to that which occurs with the Earth's magnetosphere. In this connection a strong planetary magnetic field is one in which the field energy density at altitudes well above any atmosphere is greater than the energy density of the solar wind. The description of such an interaction may then best be obtained by straightforward analogy with the interaction that occurs at the Earth.

Observations obtained with Earth satellites and deep space probes in the magnetosphere and beyond have provided evidence for three relatively distinct regions associated with the Earth's field and its interaction with the solar wind. These three regions, shown in Figure 2, are the magnetosphere (A), the magnetic tail (B), and the interaction region (C). The

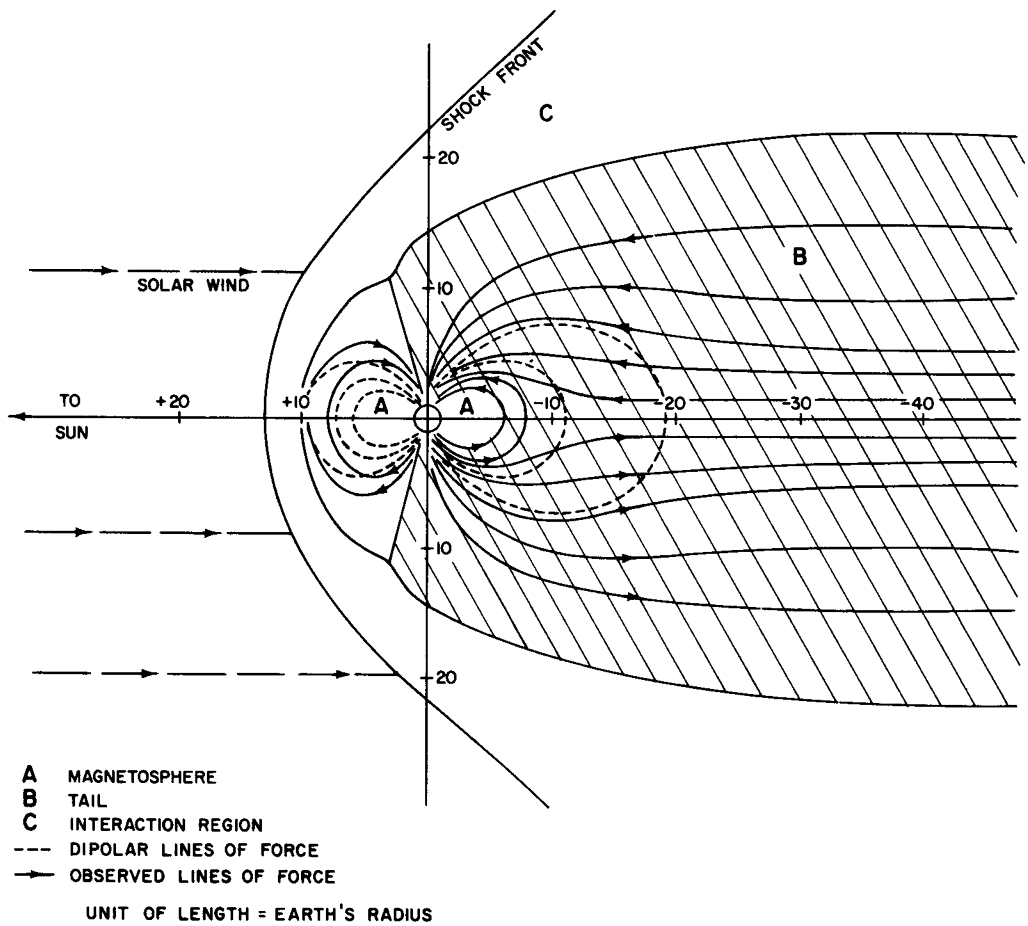


Figure 2. Magnetic fields near Earth. Cross section in r θ plane.

magnetosphere itself, Region A, is essentially composed of the field lines that intersect the Earth at latitudes below 74° (the auroral zone latitude). This part of the field exists in a somewhat distorted, but basically dipolar, configuration. The lines of force above latitude 74° are drawn out behind the Earth to form a magnetic tail, Region B. Between the interplanetary medium and the fields of terrestrial origin is an interaction region characterized by magnetic fields that exhibit a good deal of short-period variation, and by plasma that exhibits different states from that of the solar wind plasma. The essential features of this field configuration have recently been confirmed in considerable detail by measurements obtained with the first Interplanetary Monitoring Platform (IMP) satellite (Ness et al., 1964; Wolfe et al., 1965).

Chapman and Ferraro (1931) described models for the interaction of the solar wind and the Earth's field in which the shape of the region or cavity occupied by the magnetosphere was obtained by equating the dynamical pressure of the wind normal to the surface of the region to the magnetic pressure of the geomagnetic field tangential to the surface. Theoretical work on the shape and extent of this region has flourished recently with the result that several models are available. Most of them are characterized by the fact that the radius of the magnetopause, R, is connected with the other relevant quantities by a formula that equates magnetic pressure to the dynamic pressure of the solar wind. The formula reduces to

$$(M/R^3)^2 = nm_i V_p^2 / f^6 \qquad (1)$$

where f is a dimensionless function of order-of-magnitude unity that depends on the angles between $\bar{R}$, the radius to the point of observation, $\bar{M}$, the vector dipole moment, and $\bar{V}_p$, the vector solar wind velocity. The mass density of the wind is nm. The case in which the solar wind is a collisionless plasma, with the ions and electrons reflected specularly from the magnetosphere, and in which $\bar{V}_p$ is normal to $\bar{M}$, has been solved in reasonably exact form by Midgley and Davis (1963) and by Mead and Beard (1964). Calculations dealing with the case in which the magnetic moment is not normal to the solar wind direction are based on a less valid model and have not been carried so far. However, the work of Spreiter and Briggs (1962) indicates that changes in this orientation have no drastic effect.

These models were studied in some detail because the long mean free paths, indicated for the solar wind plasma, suggested that the particles behaved independently in any region of dimensions less than λ_F. However, Gold (1959), Zhigulev (1959), Axford (1962), and Kellogg (1962) argued that an interplanetary field would cause the solar wind to behave as a continuous fluid over dimensions in excess of the Larmor radius of the protons. Thus, since the speed of the solar wind relative to the Earth is supersonic, i.e., greater than V_A, a standing shock wave of a collision-free nature would be generated upstream from the magnetospheric boundary.

In the above discussion of the shape of the magnetosphere, the models described were based upon the assumption that the solar wind contains no magnetic field and that interactions between the particles may be neglected. If the consideration of the fluid behavior of the solar wind is carried to the theoretical limit, an alternative model based upon the continuum approximation for hypersonic flow can be employed to estimate the shape of the magnetosphere. This approximation was developed for the case in which the collision mean free path is much smaller than all the other characteristic lengths, which is certainly not true here, but which may represent reasonably well the situation in which the particles are organized into a fluid by the magnetic field and the flow velocity is high compared to the magnetoacoustic wave or Alfvén velocity. In this case, it is only necessary, according to Lees (1964), to multiply f by $(12/5)^{1/6}$. Even though some of the basic assumptions of this model are not particularly appropriate, it probably treats more realistically the momentum balance that determines the scale of the magnetopause than do the specular reflection models, so that the correction factor should probably be used in any application of the specular reflection models.

Thus far, the discussion has centered on the extent of the magnetosphere, the region in which the magnetic lines of force lead back to the planet. But it is clear that a planetary magnetic field modifies the interplanetary field to a considerably greater distance. The essential point is that the solar wind must flow around the magnetosphere and that its flow pattern will be modified over a region whose size is proportional to the size of the magnetosphere. Thus, as previously mentioned, we must be concerned with the supersonic flow of a nearly collisionless plasma containing a small magnetic field around a blunt obstacle, the magnetosphere.

Under the specific assumption that the solar wind plasma obeys an equation of state with the ratio of specific heat equal to 2, Spreiter and Jones, (1963), determined the shapes and stand-off distances of the shock waves for different Mach numbers, using various approximate shapes for the magnetosphere. The observations of the shape of the outer boundary of Region C fit the resulting models surprisingly well, thus supporting the collision-free-shock model. However, a number of other characteristics expected of the interaction region are somewhat different from those observed.

In any case, the observed interaction results in an extension of the region influenced by the geomagnetic field. The Earth's field at the geomagnetic equator is about 36,000 γ. At 20 R_e (1 R_e = radius of Earth) the equatorial dipolar field is 4.5 γ, which is comparable to the strength of the interplanetary field, and would, therefore, be difficult to detect. As shown in Figure 1, the shock front is between 15 and 20 R_e at angles from the Earth-Sun line between 0 and 90°. However, beyond 90° the geocentric range of the shock front continues to increase. Mariner IV data taken at 105° (Coleman et al., 1965) demonstrate that the interaction region may extend to 35 R_e. The IMP data show that the tail of the magnetosphere, with field strengths of 10 γ, extends well beyond 42 R_e.

Thus, the procedure employed in scaling this model of the Earth's interaction region is as follows. For a specified solar wind and dipole moment, the boundary of the magnetospheric cavity, Region A, is determined assuming specular reflection of the solar wind particles at the surface of the cavity. Next, the scale of the resulting surface is adjusted in accordance with the observation that a gas, with a specific heat ratio of 2, develops a stagnation pressure of 0.84 nmV_p^2 for all free-stream Mach numbers greater than about 5. Finally, the shape and stand-off distance of the shock front is determined under the assumption that the ordinary gasdynamic relations for a gas with a specific heat ratio of 2 are applicable. This scaling method should be applicable to any case in which the dimensions of the interacting body are considerably greater than the proton Larmor radius.

Following the flight of Mariner II past Venus, this procedure was applied by Smith et al., (1965) in establishing an upper limit for the magnetic dipole moment of the planet. Mariner II passed within 41,000 km of the center of Venus, but no evidence of the magnetospheric or interaction region fields was found. Application of the procedure yielded an upper limit for the dipole moment of Venus of 3.7:10^{24} emu, or about 0.05 times that of Earth.

With the Earth and its atmosphere shielded, for the most part, from direct interaction with the solar wind, a number of indirect effects of the more distant interaction of the Earth's magnetic field with the wind are observed. Examples include the auroras, magnetic storms, and other geomagnetic variations, hydromagnetic wave generation, ionospheric and upper atmosphere heating, injection and acceleration of radiation-zone particles, etc. For a planetary body with a weaker field relative to the energy density of the solar wind more pronounced effects of these types would be expected. Further, for weaker fields, the latitude above which field lines are dragged back into a tail configuration would decrease. Thus, auroral phenomena would occur in a band of lower latitudes.

Consider next some more speculative situations. If the field is so weak that the solar wind reaches atmospheric altitudes, the scaling approach is probably not applicable unless the upper atmosphere is sufficiently conducting to permit the generation of substantial currents. The energy flux of the solar wind at 1 AU is $5 \cdot 10^{-8}$ w/cm^2 compared to $1.4 \cdot 10^{-1}$ w/cm^2 for the solar constant at this heliocentric range. Thus, only effects at the higher altitudes would be expected in any appreciable atmosphere. For example, if the Earth had no magnetic field the solar wind energy would be deposited above altitudes of order 100 km since the mean free path in the outer magnetosphere is only about 10 km. To date, little consideration has been given to the details of likely solar wind-atmosphere interactions. Interactions similar to those observed in comet tails might be expected under certain conditions. Ionospheric effects also seem likely. For example, observations of a relatively intense airglow on Venus (Shaw and Bibrovnikoff, 1959) suggest that the solar wind reaches atmospheric altitudes. Scarf (1963) assumed that the field at Venus was sufficiently weak so that part of the wind reached the ionosphere. He postulated that the two-stream plasma instability would arise during the interaction to produce a nonthermal source of the relatively strong microwave radiation from the planet.

Little is known of the ion composition of the solar wind. However, if heavier ions exist in the solar wind they could act to change appreciably such sensitive parameters as the isotopic abundance ratios. For example, Bernstein (1964) has discussed such a possibility for the xenon in the Earth's atmosphere. In this case, it would be necessary to assume that only very heavy ions could penetrate the interaction region between the wind and the geomagnetic field. However, the effects upon atmospheres less well shielded would be more general.

Next, consider the limiting case of a planetary body with no appreciable field and very little atmosphere. The Moon may be an example. Nakada and Mihalov (1962) described a model for the accretion of the solar wind ions by the Moon, in the absence of a lunar field, to form an atmosphere. If, on the other hand, there exist sources of gas on the Moon, the solar wind reaching the surface will produce significant losses through scattering of atmospheric atoms by the solar-wind ions.

Returning to the discussion of the fluid-dynamical properties of the solar wind, consider next the interaction of the wind with the Moon. Measurements obtained with the USSR's Lunik 2 (Dolginov et al., 1961) place an upper limit of 100 γ on the magnetic field at the lunar surface. The lunar radius is 1738 km. In the absence of a magnetosphere, there is some question as to whether the Moon presents a sufficiently large obstacle in the path of the solar wind to generate a shock wave and interaction region similar to that of the Earth. If it is large enough, the configuration of the interaction region can probably be scaled down from that of the Earth, with the Moon itself acting as the obstacle rather than the magnetosphere.

If no interaction region is developed, the solar wind may reach the lunar surface, as mentioned previously. However, other possibilities have been considered. Gold (1964) has suggested that if the Moon is a sufficiently good conductor, a weak lunar field will be set up by the resulting interaction. Other mechanisms for the generation of magnetic fields in objects interacting with the interplanetary medium have been discussed by Alfvén (1957) and by Harwit and Hoyle (1962) in connection with studies of the accelerations of comet tails.

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PART TWO

II
Optical Astronomy

1. SUMMARY AND RECOMMENDATIONS

The Working Group on Optical Astronomy was organized "to examine the future needs of optical astronomy for large-aperture orbiting telescopes of a generation beyond the orbiting astronomical instruments which are now being readied for launching." The Group interpreted this charge to include the space program for optical astronomy generally, since consideration of large instruments requires study of the scientific data as well as engineering experience gained with small instruments. As applied to the Working Group's area of concern, optical astronomy in space was defined to include all astronomical research carried out with reflecting telescopes in space at wavelengths from 800 Å to 1 mm, excluding solar studies. In terms of the instruments used, this definition is logical, since a conventional optical telescope with near-normal-incidence reflecting optics can be used for a wide variety of observational studies in this wavelength range. At the lower wavelength limit, somewhat shorter than 912 Å, mirror reflectivities tend to be low, and stellar radiation is probably completely cut off by the interstellar hydrogen absorption. Above the upper limit of 1 mm, the atmosphere becomes transparent and larger radio telescopes on the Earth's surface are more effective. (Solar research, with different problems of thermal control and guidance requirements, needs different types of telescopes from those used for observing stars, stellar systems, nebulae, and planets, and was therefore the subject of study by a different Working Group.)

The space astronomy discussions at Woods Hole in 1965 were in some ways a continuation of earlier discussions by the Astronomy Working Group at the Iowa Summer Study Group in 1962 (" A Review of Space Research", Publication No. 1079 of the National Academy of Sciences—National Research Council). During the three-year interval since that earlier study, great strides in space technology have been made. Large rocket boosters have placed tons of equipment in orbit, and the Gemini flights in the spring of

1965 have shown that man can operate effectively in space, even outside the spacecraft. The progress of optical space astronomy in the study of objects other than the Sun has been impeded by the difficult pointing requirements, but the accumulating data on ultraviolet stellar spectra obtained with sounding rockets (including a recent spectrogram with 1 A resolution), and the progress made in fabricating and testing Orbiting Astronomical Observatories, suggest that rapid progress in this field can now be expected. The Woods Hole discussions naturally reflect the confidence resulting from these developments.

The present report is designed primarily to present the recommendations made by the Working Group, together with enough background material to explain the chief reasons underlying each specific recommendation. Many of the auxiliary points discussed by the Group are not mentioned here. To provide general background information, Section 2 presents a brief discussion of some of the most important and striking research objectives of astronomy in general and of optical astronomy in space in particular. Section 3 discusses the short-range program in optical space astronomy, including flights planned during the next ten years, and related programs in astronomical instrumentation, optical design, and ground-based research generally. Section 4 is devoted to the longer-range goal of a large space telescope. Section 5 comprises three appendixes—the working papers of the Group.

RECOMMENDATIONS

The Working Group on Optical Astronomy has considered the possibilities for studying stars, star systems, nebulae, and planets by means of telescopes in space sensitive to electromagnetic radiation at wavelengths between 800 Å and 1 mm. For the short-range program (1965-1975), the following recommendations (all summarized here) have been made:

(1) The number of coarse-pointing sounding rockets available each year for optical space astronomy should be increased to twice the present level.

(2) Two or more telescopes having apertures of 40 inches or larger should be included in the Apollo Extension Systems (AES) program. The Orbiting Astronomical Observatory (OAO) program should be continued until AES launchings are definitely scheduled.

(3) Development of various detectors required in space telescopes should be supported by NASA.

(4) Development of improved gratings would be of central importance in the space astronomy program.

(5) Development of optical interferometers should be pressed, with probable initial operation on the ground.

(6) Research and development concerned with problems of space-

telescope optics, especially with the primary mirror, should be supported by NASA.

(7) Support of ground-based astronomy should be increased, as such support is urgently needed for the continuing healthy growth of astronomy in general and of space astronomy in particular.

With regard to the long-range program (after 1975), the Working Group has concluded that the focus of the national effort in optical space astronomy generally should be toward, and in the context of, a very large orbital telescope to be used with a wide variety of astronomical instrumentation. To help pursue this objective the following recommendation (given in full here) was adopted:

(8) We conclude that a space telescope of very large diameter, with a resolution corresponding to an aperture of at least 120 inches, detecting radiation between 800 Å and 1 mm, and requiring the capability of man in space, is becoming technically feasible and will be uniquely important to the solution of the central astronomical problems of our era. We recommend that the Space Science Board of the National Academy of Sciences appoint an *ad hoc* panel to work toward this Large Orbital Telescope and to encourage studies of those critical areas where particular research and development is required in the near future to further this program.

2. RESEARCH OBJECTIVES

The broad goal of astronomy is the understanding of the vast physical universe in which the Earth is located. To achieve this goal requires answers to many sweeping and challenging questions, such as:

(1) Is the Universe finite or infinite, and if it is finite, what is its size?

(2) Is the Universe in a steady state, and if not, how did it begin and how will it end?

(3) Do the laws of physics as deduced on the Earth apply without change for all times and over all distances? Alternatively, are there fundamental physical laws or phenomena still undiscovered in terrestrial laboratories, that are observable only on an astronomical scale?

(4) Were the chemical elements that form all matter build up out of hydrogen, and if so, how?

(5) How are stellar systems, stars, and planets formed?

Partly in order to answer such fascinating questions and partly because of an interest in understanding all major constituents of the Universe, astronomers have investigated a host of interrelated problems, such as, the internal structure of stars and sources of stellar energy generation; the structure of stellar atmospheres and the complex physical processes that take place in the outer atmospheres of the Sun and the stars; the dynamical equilibrium and evolution of a stellar system, such as a rotating galaxy composed of many billions of stars; and the formation, evolution, and gravitational contraction of a rarefied cloud of interstellar matter, which forms the birthplace of new stars.

Optical space astronomy can make major contributions to these studies in two ways. First, with a telescope in space, observations of radiation from the stars and from nebulae can be made in the ultraviolet, at wavelengths down to the absorption limit of interstellar hydrogen at 912 Å, and in the infrared. Second, a telescope above most of the Earth's atmosphere can, in principle, distinguish fine detail with a spatial resolution limited only by the diffraction of light. Because of the enormously greater simplicity and economy of ground-based observations, it is certain that in the foreseeable future most astronomical research will be carried out from the ground. However, some information cannot be obtained from ground-based observations, and certain key questions in astronomy require space telescopes for their answers.

Astronomical research problems in which space observations could play a decisive role have been discussed in detail in earlier publications ("Science in Space," edited by L.V. Berkner and H. Odishaw, McGraw-Hill, 1961; also "Prospectus, 1965," by the Office of Space Science and Applications, NASA), and no exhaustive analysis of this topic was carried out by the Working Group. Four such problems of astronomical research are discussed here briefly as examples.

(1) The cosmic distance scale. The distance scale for the Universe is essentially determined (on the basis of the principle of the uniformity of nature) from measurements of the brightness of individual objects in other galaxies recognizable as comparable in intrinsic brightness with their counterparts in our own galaxy, such as novae, star clusters, H II regions, associations, variable stars, and supergiants. With ground-based instruments, such measurements cannot be made much beyond the Virgo cluster of galaxies, and, as a result, the farther-distance scale is uncertain. A large diffraction-limited telescope in space, capable of producing sharper stellar images against the fainter background light, could extend these measurements to much more distant galaxies. In addition, determinations of the angular diameter of hydrogen emission regions (H II spheres) around early-type stars, which could be made with a high-resolution space telescope, could give an independent measure of extragalactic distances beyond the nearest great clusters of galaxies.

(2) Structure of nuclei of galaxies. Evidence is accumulating that the galactic nuclei are the sources of the vast energies observed from quasi-stellar galaxies, and possibly from other types of radio galaxies as well. These nuclei are too small to be resolved with existing telescopes. A

diffraction-limited 120-inch telescope in space could resolve detail 0.03 sec of arc across, one-tenth the angular size that can be distinguished from the Earth's surface: observations with enhanced resolution could be completely decisive in our interpretations of the nuclei of galaxies.

(3) Distribution of molecular hydrogen. Hydrogen in molecular form may constitute an appreciable fraction of the mass of our Galaxy, and if present at all, certainly plays a dominant role in the thermal balance of the interstellar gas and thus in the process of star formation. There is apparently no way of detecting interstellar H_2 directly from the ground. Observations with a space telescope in the ultraviolet and infrared can determine with precision the spatial distribution of H_2, and much about its temperature as well.

(4) Detailed analysis of low-temperature objects. Dark interstellar clouds, globules, protoclusters, and protostars are expected to radiate energy in the neighborhood of 100 microns. When these objects are projected against a bright background, they can be detected by the absorption that they produce, but measures of the infrared emission would give more useful data for analyzing the physical nature of these objects, which are thought to play such an important role in the evolution of stars and galaxies.

3. THE SHORT-RANGE PROGRAM

Programs of space astronomy that will lead to launchings within ten years may be regarded as of short range. Supporting research that will affect launchings not later than 1975, or that will be helpful in analysis of the results obtained by 1975, may also be regarded as part of the short-range program. This short-range program of space astronomy is discussed here.

The following three subjects are important for both short-range and long-range programs:

(i) Flight hardware, including sounding rockets and satellites.

(ii) Supporting activities, including development of astronomical instrumentation and engineering techniques for space telescopes as well as researches in physics and astronomy needed for the interpretation of space astronomy data.

(iii) Training of scientific manpower; the success of the rapidly growing space astronomy program during the coming decade will depend in large part on the ability and training of scientists who enter this field.

The Working Group discussed certain aspects of these subjects; recommendations reached are given below, together with brief summaries of some of the background discussion.

SOUNDING ROCKETS*

Aerobee rockets without pointing have been successfully used for broad-band ultraviolet spectrophotometric measurements of the brighter early-type stars. More refined measurements require pointing, and difficulties experienced with the coarse-pointing system developed for NASA by Space General Corporation have delayed progress in optical space astronomy. Fortunately, recent flights of this system have been relatively successful, and coarse pointing, with an accuracy of about $\pm 2°$ and a jitter of $\pm 0.25°$, is now available for research programs. More sophisticated guidance systems, capable of guiding on a 3rd magnitude star with a precision of 30 sec of arc, are now being developed by the Goddard Space Flight Center and other groups, both in the U.S. and abroad.

It appeared to the Working Group that the sounding-rocket program should play a central part in optical space astronomy for a number of years, if not indefinitely. For obtaining preliminary data, for testing components in flight, and for gaining experience generally in space astronomy, the relatively low cost and frequency with which small rockets can be launched offer very important advantages. The number of rockets available with the present coarse-pointing system has already become rather limited, however. With the recent successes in this field, and the growing interest in infrared research, additional launchings will be needed by existing groups as well as by other astronomers who should be encouraged to take an active interest in this work. The following recommendation was therefore adopted by the Working Group:

Recommendation 1

We believe that continuing research with sounding rockets is of the greatest importance in obtaining exploratory scientific data and in testing space instrumentation in advance of satellite flights. A coarse rocket-pointing system of adequate reliability now seems to be available, and we recommend that the number of launchings of pointed rockets available to optical astronomy, for research in ultraviolet and infrared, be doubled.

* A fuller discussion of the use of sounding rockets in optical astronomy has been prepared for the Working Group on Rocket-Satellite Research by N.U. Mayall, A. Boggess, and T.A. Chubb, and appears in Chapter 7, this report.

We also recommend that a fine-pointing system for Aerobee rockets be made generally available as soon as feasible.

ORBITING ASTRONOMICAL OBSERVATORY (OAO) PROGRAM

In the present NASA program, four Orbiting Astronomical Observatories are funded, with a fifth approved technically. In all of these, the primary payload is an optical telescope used at ultraviolet wavelengths; x-ray detectors are also included in the first and fourth OAO spacecraft.

In view of the great effort and expense that went into the development of the OAO spacecraft and related facilities, and the high capability for space astronomy anticipated with the OAO, continuation of this program is clearly desirable and inclusion of other fields of space astronomy would be appropriate. If the Apollo Extension Systems (AES) program is modified to provide a comparable capability, this system, which can be manned and which therefore is potentially more reliable, might well replace the unmanned one. However, the OAO program should be continued until the AES program is sufficiently far advanced that definite launchings can be scheduled and the astronomical capability of the AES can be reasonably well assured. (A definite recommendation to this effect is included in Recommendation 2, below, which is concerned primarily with the AES.)

It will be noted that infrared research is listed as one of the fields that should be included in the future OAO program. This recommendation reverses the stand taken by the Iowa Study in 1962. During the last three years, important progress has taken place in infrared astronomy from the ground and from balloons, and active plans for sounding-rocket flights in this field are under way. It may be anticipated that, in the near future, experience obtained in these researches will indicate definitely how much would be gained scientifically by devoting an OAO to infrared studies.

APOLLO EXTENSION SYSTEMS (AES) PROGRAM

This program can make use of man in operating and maintaining sophisticated scientific equipment. While AES flights should yield useful and scientific data, the Working Group felt that a more important aspect of the AES program was the intermediate step that it provides toward a much larger instrument, which in the opinion of the Group should constitute the objective of the long-range program in optical space astronomy (see Section 4).

In particular, the Working Group felt that, as part of the AES program, it is essential to test at a relatively early stage the ability of man to adjust, maintain, repair, and occasionally operate a large space telescope. To this end, it was proposed that a least two telescopes of intermediate aperture (40 to 80 inches) be included on AES flights (see Proposed Schedule, in Section 4.2 below). Because of their greater aperture and presumably their greater flexibility and repairability, these AES telescopes might, if successful, carry out more effectively some of the tasks that later OAO's might otherwise undertake; if so, the later OAO's might be phased out. Possibly, after an AES telescope had operated for a substantial period with some particular instrumentation, men might substitute other, different instrumentation: for example, they might replace ultraviolet sensors with infrared sensors.

Since the AES flight tests would be critical for obtaining information necessary to the development of the larger telescope that would presumably follow, it is essential that the flight tests not be dropped even if the subsequent AES program were cancelled for some reason. In fact, apart from the overriding priority necessarily given to the astronauts' safety, the primary mission of the AES flights carrying astronomical telescopes should be the operation of these telescopes.

The following recommendation was adopted by the Working Group:

Recommendation 2

We believe that the presently planned Apollo Extension Systems program provides an important intermediate step in the development of the Large Orbiting Telescope, and also provides an opportunity for significant scientific observations. We recommend that two or more telescopes of appreciable aperture (40 inches or larger) be launched as part of this program.

We further recommend that, when major astronomical experiments are included in AES launchings, the experiments be designated as the primary objectives of these flights.

Until developments in the Apollo program make it possible to schedule definitely a number of AES launchings for scientific purposes, we recommend that the OAO program be continued, with experiments in the following fields: high-resolution imaging; infrared photometry and spectrophotometry with cryogenics; planetary spectroscopy in the ultraviolet; polarization and photometry; x rays.

INSTRUMENTATION

Photography. Photography today has an established usefulness in astronomy that no other image recording and storing technique can rival. In addition, there seems to be little immediate prospect that image tubes of any sort will rival a photograph in the number of information elements that can be recorded simultaneously*. We therefore foresee that, for some purposes, photography will be used in space for some time, provided that the photograph can be returned to Earth, or can be processed in space, scanned electronically, and the data telemetered, without degradation, to the Earth. (In Appendix 2, G. Münch discusses the technical properties of photographic emulsions, and describes recent developments in this area.)

Development of new emulsions with higher quantum yield would do much to enhance the value of photography for all astronomy. There does not seem to be any real physical reason for the quantum yield of the photographic process to be less than the 20% obtainable with photoelectric surfaces. For space application, development of an emulsion sensitive only in the ultraviolet (such as a solar-blind phototube) would be desirable, as would also be the development of an emulsion sensitive beyond 1.3 microns.

Image tubes. The primary advantage of an image tube for taking pictures with visual or ultraviolet radiation is its sensitivity, which ideally might be an order of magnitude higher than is obtainable with present emulsions. For space application, a secondary advantage of an image tube, when used with an electron-beam readout, as in a television camera, is that the signal can easily be telemetered to Earth. However, present television cameras have not yet been used successfully for astronomical programs; astronomical research requires precise photometry and long integrating times, which are unimportant for commercial television. Of the tubes now available, it appears that some might be usefully adapted for astronomical work. Further test and development in this area seems highly worthwhile, since a reliable image tube with significantly greater sensitivity than photographic film and with electronic readout would be extremely useful in optical space astronomy.

Present image tubes can record about 10^5 independent picture elements, as compared with at least 100 times this many on large photographs. It is evident from the Appendix that there is no clear agreement as to the number of resolution elements required in the pictures recorded for astronomical research. Quite possibly, even if image tubes are generally used in space astronomy, photography will still be in demand for special programs requiring a wider field.

Infrared detectors. Astronomical research at wavelengths between about 10^{-4} and 10^{-1} cm is limited by the sensitivity of present detectors. A brief survey of available detectors in this range is given in Appendix 2

* But see p. 30, "Note on Recent Developments in Image Tubes" added in proof.

by F.J. Low. Especially at wavelengths longer than 3.5 microns, it appears that, by use of liquid-helium temperatures (see paragraph on Cryogenics, below) and by a careful selection of materials, substantial improvement in signal-to-noise ratios should be achievable over currently available devices. Intensive research in this field appears highly desirable.

Solid-state imaging devices. Most imaging work has been done either with chemical (film) or photoelectric devices, but, in principle, solid-state techniques might be utilized. These would have the advantage of being usable at infrared wavelengths. Thus far, Westinghouse (under contract with the NASA Marshall Space Flight Center) has made a silicon mosaic with 50 x 50 elements in a 2 cm x 2 cm square, but the picture is very grainy, as one would expect. Extremely high resolution could, in principle, be obtained, equal to the spacing between the trapping sites in the semiconductor; this spacing may be the interatomic distance in the crystal, but it is more likely to be the distance between the impurity sites. Possibly, the image could be read off the crystal by scanning the back side with a spot of light (for semiconductors in which incident light lowers the resistance) or with a shadow-spot (for semiconductors in which light raises the resistance). The size of the scanning spot is likely to set the limit to the resolution.

Cryogenics. Low temperatures are needed to achieve high sensitivity in detectors for wavelengths longer than 1 micron. Although 4 to 10°K may suffice for the interval from 1.0 to 1.8 microns, much lower temperatures are needed at longer wavelengths. (See two paragraphs immediately above, and Low's Appendix.) Solid H_2 is an efficient coolant for temperatures between 6 and 20°K. Ordinary liquid He^4 can be used from 1 to 6°K. Liquid He^3 provides useful cooling between 0.25 and 1°K. In order to store adequate quantities of these coolants in a small space, they must be kept at rather high pressures; also, fairly complex devices must be developed to regulate the temperature and pressure and to avoid waste. These problems appear to be more tractable in a space environment than those associated with mechanical refrigerators. Studies by the NASA Ames Research Center indicate that 65 pounds of solid H_2 would maintain a detector at 10°K for about one year. Research and development along these lines should be continued.

As a result of its discussion concerning instrumentation, the Working Group adopted the following recommendation:

Recommendation 3

We recommend that NASA be urged to support the development of the detectors required in space telescopes, together with the necessary auxiliary equipment. Particular emphasis should be placed on the following problems:

(i) Development of new photographic emulsions or the equivalent, with (a) higher quantum yields, (b) selective responses in the ultraviolet, and (c) effective responses beyond 1.3 microns.

(ii) Adaptation of television techniques for astronomical use.

(iii) Development of new infrared detectors.

(iv) Development of image-registering devices using solid-state detection.

(v) Development of the cryogenic apparatus needed to maintain temperatures as low as 0.25 °K in a satellite.

Gratings. Astronomical spectrophotometry requires gratings of high efficiency and high optical quality. Much of the advantage of a large telescope in gathering photons can be lost if the grating does not concentrate in a single order most of the light of a given wavelength. Precise photoelectric spectrophotometry requires that the grating response be relatively uniform. High resolution and freedom from ghosts are also very important. Because of their suitability for spectroscopic observations with an image tube, which has a small area of sensitivity, echelles are of particular interest for space astronomy. An imaginative program for developing gratings and echelles of higher quality would be of very great assistance to optical astronomy in space. The following recommendation was adopted by the Working Group:

Recommendation 4

We strongly emphasize the importance of improving the efficiency, uniformity, and freedom from imperfection of gratings and echelles, especially for ultraviolet work. Additional effort in this area would be of very central importance in the space astronomy program.

Interferometry. In principle, a stellar interferometer can make fundamental measurements of stellar diameters. Moreover, just as in radio astronomy, many observations with a two-element interferometer can, in principle, produce a picture the ideal resolution of which would correspond to that of a telescope whose aperture equals the maximum spacing between the elements (aperture synthesis). In practice, the poor seeing produced by the atmosphere has made the use of stellar interferometers on the ground very difficult; Michelson's 20-foot instrument has produced results only under exceptionally good conditions, and the 50-foot beam interferometer has never worked satisfactorily. Above the atmosphere, these particular difficulties would not be present, and interferometric techniques could be an important tool for optical space astronomy.

Recent studies give some hope that the seeing problem may be ameliorated with modern high-speed electronic methods of observation. Experiments using such methods are being discussed at the University of

Chicago, where two telescopes would be used in an interferometer system. Clearly, optical interferometers should not be sent into space until these possibilities for use on the ground have been explored further. Nevertheless, in view of the potential importance of an operable stellar interferometer, the following recommendation was approved:

Recommendation 5

In view of the importance to astrophysics of a knowledge of the diameters of celestial objects of extremely small angular size, we recommend that the development of optical interferometers of various types be pressed, bearing in mind, however, that such instruments may initially prove to be more useful for ground-based observations than for space.

Miscellaneous instrumentation. Among the various instrumental possibilities discussed by the Group, without any action taken, the use of space vehicles for absolute astrometry may be mentioned. Among the possibilities mentioned were: photographing a satellite of known position against a star field to obtain absolute positions; rotating a telescope or other object at a uniform rate, and using this as a standard for absolute positions; development of an optical system that would rotate a light ray through some fixed angle (say 90°) with a precision of a small fraction of a second. Further study is desirable to determine the ways in which precise astrometric observations can best be made from space.

OPTICAL RESEARCH AND DEVELOPMENT

The optical-performance requirements of a large space telescope are extremely exacting and difficult to meet. The mirror figure should be as close as possible to the theoretically desired surface, preferably within about a fiftieth of the wavelength to be used for high-resolution studies, and should not change appreciably with the passage of time or with changes in the thermal environment. To achieve this objective requires methods of measuring the actual figure as well as special techniques to hold the figure constant. Research on these problems has been carried out at a number of locations, partly in connection with the Stratoscope II balloon telescope program and partly in connection with plans for large satellite mirrors.

In addition, the reflecting coating applied to the mirror should have a high reflectivity for all wavelengths of interest. An aluminum coating applied in a vacuum appears to have exactly this property, with high reflectance down to wavelengths less than 1000 Å. To eliminate oxidation of the surface aluminum, which occurs very rapidly in air, it may be necessary to coat satellite mirrors in orbit. Dielectric coatings may still be required, however, and further exploration of the properties of these coatings is needed.

Unquestionably, other problems in the design and construction of space telescopes also require intensive effort. Most of these, however, lie outside the competence of astronomers, and many were not discussed in detail by the Working Group, though some other engineering problem areas are referred to in Section 4 below. The following recommendation, dealing with research and development in optical systems, was approved by the Working Group.

Recommendation 6

We recommend that NASA be urged to support basic research and development leading to the optimum design and construction of space telescope optics. The vital areas are:

(i) Improved mirror materials and construction to provide adequate thermal and structural stability, including particularly methods of measuring and reducing internal strain.

(ii) Mirror surfaces to provide high ultraviolet reflectivity, precision of figure, and freedom from scattering.

(iii) Methods for rapidly evaluating mirror figure and alignment under normal and zero gravity.

(iv) Methods for generating and maintaining diffraction-limited mirror quality in space, by passive or active means.

GROUND-BASED ASTRONOMY

As pointed out at the beginning of this Section, space astronomy depends vitally on ground-based astronomy: first, as a source of scientific data and information and of trained manpower, and, second, as a means to follow up discoveries in space. To cite an example—one of many—of this dependence on ground-based observations for scientific information: the limited amount of spectroscopic information that has thus far been obtained for stars in the southern sky will make it very difficult to carry out much research on the ultraviolet spectra of these stars; thus, to use a satellite telescope for a spectroscopic survey of southern stars would be enormously wasteful unless such ground-based information were also available. Hence, space astronomy cannot sustain a healthy growth unless ground-based astronomy is also expanding at a somewhat comparable rate. To emphasize this point the Working Group adopted the following recommendation:

Recommendation 7

To make efficient use of orbital telescopes requires the most complete knowledge obtainable with ground-based instruments, and requires also the education of many new young astronomers. We wish to emphasize most strongly that increased support of ground-based astronomy, in the spirit of the Whitford Report, is urgently needed for the continuing healthy growth of astronomy in general, and of space astronomy in particular.

4. THE LARGE ORBITAL TELESCOPE

It seems clear that during the next ten to twenty years the U.S. space program will have an increasing potential for launching complex scientific equipment into space, and maintaining it there. The Working Group considered in some detail what the program in optical astronomy should be during this long-range interval. The possibility of launching a single very large, diffraction-limited telescope into space has been discussed previously, for example, at the Iowa Summer Study in 1962 and in various NASA planning documents. Consideration of such a space telescope as a goal for the long-range program raises the following two major questions:

(1) Would a large telescope of conventional optical design be a vitally important research tool several decades hence?

(2) Would a single large telescope be preferable to many smaller ones?

These questions were discussed in detail and at length by the Working Group.

The first question is of particular importance because of the long lead time, between one and two decades, for launching a large space telescope. However, it seems reasonable to assume that so long as astronomy is of interest to man, the measurement of electromagnetic radiation from the stars will continue to be an important objective of astronomy, and the use of a reflecting telescope seems to be much the simplest method of achieving this objective over a wide range of wavelengths. It is conceptually possible that methods might be developed for circumventing the varying atmospheric refraction of starlight (bad seeing), and also the distortion of large mirrors under gravity (flexure), and thus achieving high-resolution pictures from the Earth's surface. There is no simple physical reason why such a technological development should be impossible, though clearly it would be difficult. However, until such technology has been shown to be possible, either by analysis or by experiment, this possibility must be regarded as remote. In any case, there seems to be no possibility whatever that radiation below 2900 Å, and in certain wavelength bands in the infrared, can ever be observed from the Earth's surface. On the basis of these considerations, one can conclude that a large diffraction-limited telescope in space would certainly be of very vital importance in astronomical research for many years to come, though for high-resolution pictures, such a telescope might conceivably in time have some competition from ground-based instruments, but only in the event that a major technological advance is made in ground-based instrumentation.

With regard to the question of whether a single large telescope would be preferable to many smaller ones, there are strong reasons, peculiar to space astronomy, for favoring the single large instrument. On the assumption that diffraction-limited images can be obtained, a single large instrument clearly has a capability that cannot be matched by several smaller instruments with the same surface area; while the use of interferometry and aperture synthesis could in theory provide equivalent resolution, use of several small telescopes in this way could not yield the same signal-to-noise ratio as a single large telescope, and could not, therefore, reach the same limiting magnitude. The great advantage of manned supervision and maintenance of this complex equipment to ensure its reliable operation is a further argument in favor of a single large instrument. Moreover, in view of the enormous cost and effort required to maintain man in space, it would be difficult to justify manned supervision of many small instruments, even if these were all located near a single space station; a single large instrument would presumably require fewer man hours of maintenance per year than many small instruments, each with its own pointing system, power supplies, and communication systems. A single large telescope, maintained at regular intervals, could have a useful scientific life of decades, as compared with about a year or less for complex unmanned equipment at present.

The great flexibility of an optical space telescope, which could be used for most types of astronomical research at wavelengths from about 10^{-4} to 1 mm, is an argument for concentrating the long-range program on a single instrument, with the largest possible aperture. Among other types of instruments that might be needed for optical space astronomy, the Working Group considered the three listed below.

Survey telescope. An all-reflecting Schmidt telescope might be used to find objects that radiate only in the infrared or ultraviolet and therefore cannot be found on plates taken in the visible or photographic regions of the spectrum. It is not clear whether such objects exist, and at present there is no wide-field imaging sensor available for wavelengths longer than 1.2 microns. The ultraviolet surveys planned for the OAO program (Smithsonian experiment) and for the Apollo program (University of Arizona) will indicate whether a larger ultraviolet survey telescope would be useful. For a diffraction-limited survey, a Schmidt telescope is not suitable because the focal ratio must be large if the size of the image on the sensor is to exceed the wavelength of light.

Infrared telescope. A telescope designed to be diffraction-limited at a wavelength somewhere between 10 and 100 microns might conceivably be made very much larger than an instrument designed for ideal optical performance at 0.5 micron. Until this field of research has been explored more fully from the ground and from space, the value of such a specialized instrument cannot be assessed.

Interferometer. The beam interferometer, designed to achieve very high resolution on particular objects, would be a useful instrument in optical astronomy. Current efforts to use this technique from the ground have been discussed above, and further information is required before the need for interferometric equipment in space can be evaluated.

After study of these various points the Working Group concluded that at present the long-range program in optical astronomy should be concentrated on a single general-purpose telescope, though special-purpose instruments might be included at a later date, when and if a clear demonstration of their value can be made.

Following considerable discussion the Working Group adopted the following recommendation:

Recommendation 8

We conclude that a space telescope of very large diameter, with a resolution corresponding to an aperture of at least 120 inches, detecting radiation between 800 Å and 1 mm, and requiring the capability of man in space, is becoming technically feasible and will be uniquely important to the solution of the central astronomical problems of our era. We recommend that the Space Science Board of the National Academy of Sciences appoint an *ad hoc* panel to work toward this Large Orbital Telescope and to encourage studies of those critical areas where particular research and development is required in the near future to further this program. (See p. 2-21 for considerations leading to the last part of this Recommendation.)

Confidence in the technical feasibility of a diffraction-limited 120-inch space telescope was based on the various technical studies carried out for NASA directly or indirectly by various groups (Boeing, American Optical, Perkin-Elmer); the engineering problems of such a large instrument were discussed only briefly by the Group. The design goal of a 120-inch aperture was adopted in the belief that a long-range instrument should be a very significant advance over the insturements used in the Stratoscope and OAO programs, whose apertures are in the 30 to 40 inch category. The aperture could well be greater than 120 inches, if that proves technologically feasible (see page 17).

It was the conviction of the Group that this large instrument could provide a dramatic central focus for the optical space astronomy program, and that it would be an appropriate major space program for the nation. It was to help emphasize the central character of this instrument in the national space effort that the name "Large Orbital Telescope" (LOT) was proposed. While the term "orbital" was used for this large-span telescope, the possibility of a lunar location was not strongly excluded.

Clearly, adoption of the LOT program would have a significant impact on the short-range program in optical space astronomy. While the short-range program discussed in Section 3 is designed primarily to obtain significant scientific results, the data obtained and experience gained would be absolutely essential for the LOT effort. In particular, the AES effort could be an important forerunner of the manned high-resolution LOT. In general, considerable expansion of much of the short-range program might be required if the LOT were to be effectively used within the time scale outlined below.

The subsequent sections discuss the possible design parameters for the Large Orbital Telescope, a time schedule that may be visualized for its construction, and some administrative problems that might be associated with this enterprise.

DESIGN PARAMETERS

The general characteristics of a large space telescope, discussed in earlier sections of this report, apply to the LOT as well. Thus, this large telescope would be a general-purpose instrument, focusing electromagnetic radiation in the wavelength range from about 800 Å to 1 mm. The Group discussed briefly the engineering problems of this telescope and the design parameters that might be chosen in view both of these problems and of the scientific objectives. While no recommendations were adopted on most of these items, the conclusions are summarized here for reference.

Aperture

For reasons already outlined, the goal of designing a diffraction-limited 120-inch telescope was adopted by the Group. The actual diameter of the instrument would depend, of course, on the technical situation at the time the instrument was designed. One possibility discussed by the Group was that the actual diameter might substantially exceed 120 inches, but with the image size corresponding to a diffraction-limited 120-inch mirror. Such an increase in light-gathering power would be desirable for many researches and might be technically feasible if a corresponding decrease in angular image diameter were not required. (If the Saturn V were used to place the LOT in orbit, and the primary were a single mirror, the diameter could not exceed 250 inches; without doubt, other engineering considerations would limit the diameter to a substantially smaller figure.)

Role of Man

It was generally agreed that the LOT should be usable for many decades, with occasional changes and improvements in the instrumentation provided at the focal plane. This requirement can presumably not be met unless a man is intimately involved in maintaining and repairing the equipment, and presumably a man will also be required for the initial adjustment and operation. The design of the LOT should provide for ease in trouble shooting, for access to all parts of the telescope, and for replacement of defective modules. The extent to which a man should actually operate the telescope is a matter of debate, and it is not excluded that the entire system should be completely automated. Guidance on stars will presumably be automatic, and, during this time, man should probably not be coupled to the instrument. However, guidance by man might prove useful for observations of a rotating planet, for which automatic guidance would be difficult. Similarly, in a crowded star field, acquisition of the desired object by a man might be useful, though this could be done through use of a television camera rather than by looking through the telescope. There was agreement that the instrument should be completely controllable at will, either by equipment on the ground or by a man nearby. There was some discussion of the likelihood of failures resulting from human error.

Location

After reviewing the recommendations of the Report on Lunar Exploration Systems after Apollo (LESA, North American, 1965), the Group discussed the relative advantages of the following three different locations for a large space telescope: low orbit (below the Van Allen belts), at 400 km altitude or less; high orbit (above the Van Allen belts), at 30,000 km altitude or more; and on the Moon. Most of the considerations examined would appear to favor the high orbit. As compared with location on the lunar surface, the advantages of a high orbit include no gravitational flexure, no secondary micrometeorites, and lower cost. A possible major disadvantage of the high orbit is greater risk of exposure of equipment and men to high-energy radiation from solar flares, though evidence presented to the Group suggests that adequate shielding is no problem. Objects close to the Sun, however, might be more difficult to observe from a high orbit than from the lunar surface. As compared with a low orbit, the advantages of the high orbit are: negligible occultation of objects by the Earth (in a low orbit, occultations complicate the programming and are likely to reduce the net observing time by about one half); nearly constant thermal environment, which much simplifies the maintenance of the mirror figure; reduction of external torques due to gravity gradients, magnetic fields, and air drag by at least two orders of magnitude, with resultant simplification of the guidance problem; darker sky than in low orbit, where airglow may contribute light; and virtual absence of oxygen atoms striking the telescope and oxidizing the aluminum. From a high orbit, communication with the ground might be simplified by continuous radio contact, but, as compared with a low orbit, communication would be complicated by the increased distance. The greater exposure to solar flare radiation may be an important disadvantage of the high orbit, especially in view of the longer time (at least 10 hours) required to return a human operator to Earth from the high orbit. A very clear disadvantage of a high orbit is that it requires a Saturn V for launching instead of a Saturn IB; since this additional cost would be required for each visit by men, this could be a conclusive argument for the low orbit. The Working Group unanimously came to the conclusion that, on technical grounds, the high orbit appears at the moment to be the optimum location for the LOT.

Optical Design

A conventional parabola-hyperbola or a Ritchey-Chrétien system seems indicated. The primary should have a relatively low focal ratio to minimize the over-all length of the instrument. Use of the prime or Newtonian focus would not seem to offer any particular advantages, and all of the instrumentation would presumably be at the Cassegrain focus, possibly with tiltable mirrors to direct the light toward the desired instrument or sensor. Careful baffling would be required to keep earthlight as well as sunlight out of the optical path, and the secondary supports should presumably be apodized (with Couder strips). Automatic focusing would presumably be required and, probably, automatic collimation as well.

SCHEDULE

To visualize how long it might be before the LOT could be launched, the Working Group formulated a very tentative schedule (see Table 1). This schedule was regarded as the fastest that could be imagined for such a large instrument, consistent with careful engineering and optical-design studies and with detailed tests of components and concepts both on the ground and in space. By comparison with the history of other large astronomy programs, some extension of this 14-year schedule might be anticipated. While some observers experienced with large space programs regarded the schedule as optimistic, others were of the opinion that a substantial acceleration would be possible.

Table 1. Proposed Schedule—
Large Orbital Telescope and Related Programs

Calendar Year	Run'g Years	Stage of Development	Launch of OAO	Manned Operation Gemini/Apollo	Corresponding dates for 40 - 80 inch AES telescopes
1966	0	Application for funds, at least for the early phases	A1		1966
1967	1	Preliminary design studies	B A2	Henize: ultraviolet, small unstabilized camera/Gemini	1966.5
1968	2	Detailed design; begin breadboarding, etc.	C		1967
1969	3		D*	U. Arizona: 6-inch ultraviolet Schmidt/Apollo	
1970	4	Start construction of prototype	E** F**		1968
1971	5		G** H**		
1972	6	Deliver first of two blanks for primary mirror†	I**		
1973	7	Figuring, testing mirrors			
1974	8	Figuring, testing mirrors			
1975	9	Figuring, testing mirrors			

Table 1, continued

Calendar Year	Run'g Years	Stage of Development	Launch of OAO	Manned Operation Gemini/Apollo	Corresponding dates for 40 - 80 inch AES telescopes
1976	10	Prototype completed			1970
1977	11				
1978	12	Flight-model completed			1971.5
1979	13	Launch on Saturn V			1972.5

* Approved by NASA, but not funded.

** In present long-range planning only, not approved.

† Optical work on primary and secondary mirrors of main optics can proceed independently of the main telescope construction, at least in part. Figuring and testing of flight-model mirrors can continue until completion of flight model.

ADMINISTRATIVE PROBLEMS

Three different phases of the program were considered: (a) preliminary phase, (b) design and construction, and (c) post-launch operation. As entirely different administrative problems would be encountered in each of these phases, they are discussed separately here.

Preliminary phase

Such a major astronomical effort as the LOT should not be undertaken until a majority of the astronomical community supports the program with enthusiasm. It appears to the Working Group that progress in space research generally, and in space astronomy particularly, combined with increasing awareness of the close interdependence of space astronomy and ground-based astronomy, may help in generating enthusiasm for the LOT among U.S. astronomers.

To help in explaining LOT plans to their colleagues, and in pressing for the program generally, the Working Group concluded in effect that the Group as a whole, or a representative fraction of it, should continue in existence, as an *ad hoc* panel, and requested the National Academy of

Sciences to endorse a proposal to this end as contained in Recommendation 8, page 3). The purpose of the panel would be:

(i) To attempt to broaden the base of support, for (a) the space astronomy program in general, and for (b) an eventual launching of a large astronomical instrument in particular. By discussion with their colleagues, they would hope to clarify the issues involved and to stimulate the interest of astronomers who are at present unfamiliar with the aims of the space program.

(ii) To begin an orderly examination of some of the technical problems that will arise in the design of a large orbiting telescope, anticipating that more permanent arrangements will be made later.

(iii) To implement these two aims by holding fairly frequent informal meetings, preparing discussions of specific subjects, inviting the participation of other astronomers, and generally to keep alive the idea of working toward a large orbital telescope.

Design and construction phase

In the initial organization of the program and during all successive stages until launch, there must be close and effective contact between NASA and its engineering contractors, on the one hand, and the astronomical community on the other. How this contact can best be maintained and integrated into the vast administrative structure required for such a large program is a question that deserves careful study. Perhaps a group of astronomers might be organized to carry out detailed design studies, with advice from engineers and optical experts; such a group might then serve in an advisory capacity during the engineering design phase that would follow. Perhaps a committee under the National Academy of Sciences, with representatives from various interested groups, might serve a useful function in this context, and might help to provide a bridge between the NASA organization for the LOT and the scientific community. Further exploration of these and other possibilities is desirable.

Operations phase

Clearly, the LOT would be a truly national facility, and should be administered as one. The plan should be workable from the standpoint of NASA's internal administration, since the situation would be complicated by the fact that flights would be involved. The Working Group visualizes that the detailed program for operating the LOT (allotment of observing time, expeditious recovery of data, proposals to place auxiliary instruments of newer design on board, etc.) would need to be managed in a way analogous to present ground-based national facilities. Responsibility for detailed scheduling must be defined, as it would depend not only on the scientific program but also on such factors as the relative position of the telescope, the Earth, the Sun, the object to be observed, communications, etc. Experience with the OAO-D program, in which two-thirds of the observing time will be allotted to guest investigators (i.e., investigators other than the principal investigator, who is responsible for the experiment), may help to reveal some of the administrative problems in these areas.

APPENDIX 1: LIST OF PARTICIPANTS

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APPENDIX 2: WORKING PAPERS

DETECTORS FOR DIFFRACTION-LIMITED IMAGING

G. Münch

My task is to analyze, as the basis for discussion, the subject of the dimensions of the diffraction-limited field desirable for a large orbital telescope. The question clearly does not have a general answer, and in order to reach some sort of guiding decision we have to consider the following factors:

(i) Nature of the astronomical problems to be studied,
(ii) Limitations imposed by the effective f-ratio employed and resolving power of detector,
(iii) Guidance requirements.

The number of important astronomical problems requiring the highest resolution is very large. Indeed, every one of the outstanding astronomical problems of our time requires high resolution for further understanding. Among the outstanding problems of current interest we may mention the angular dimensions of the quasi-stellar radio sources, the structure of the nuclei of galaxies, the resolution of galaxies into stars to refine the distance scale, and the study of the faint end of the population of clusters of galaxies and stars. Of paramount importance for the cosmological problem would be the study of the background population of galaxies, say five magnitudes below the limit set at present by the various components of the surface brightness of night sky. A field as small as 20 sec of arc, truly diffraction-limited, say with resolution of about 0.''03, would provide information toward the solution of these important problems. It would appear that such a small diffraction-limited field should not be traded for a larger field with degraded resolution, although it is obvious that a larger field would be highly desirable for reasons of economy in the process of data acquisition. However, the linear dimensions of the diffraction-limited image should correspond to the resolving power of the detectors used, and in this context we have to consider the limitations introduced in (ii), above.

To consider a specific case, we take as a guide the 120-inch, f/4 primary discussed in the Boeing report.[1] The dimensions of the field and of the Airy disk (at $\lambda = 0.55\,\mu$) corresponding to various effective focal ratios are given below:

	Field		Airy Disk (μ)	Matching Detector
f/4	6 sec of arc =	0.4 mm	2.5	Very fine grain
f/8	20 sec of arc =	2.5 mm	5	V-O (>225 line/mm)
f/15	40 sec of arc =	9 mm	10	III-O (96-135 lines/mm)
f/30	120 sec of arc =	55 mm	20	IIa-O (50-70 lines/mm)
f/100	5 min of arc =	450 mm	66	Phosphors*

In the last column we have entered the photographic emulsion with resolving element not larger than the Airy disk. Only for the f/100 beam could we use a phosphor.* But for a 1-inch tube the dimensions of the field would be effectively limited to 16 sec of arc. The emulsions ordinarily used in ground-based astronomical observations and specially treated for reciprocity failure, such as Eastman IIa-O, would require a beam of about f/30. Emulsions of higher resolving power are not generally treated for reciprocity failure, but assuming that we can do something about it (baking, for example, effectively reduces reciprocity failure of fine-grain emulsions), they can be used just as effectively, for it is known that the quantum efficiency of the emulsions remains very nearly constant as the resolving power increases. For the very highest resolving powers (very fine grain) the loss in speed may amount to a factor of 2, but it is difficult to establish test procedures that apply to such cases.

It may be appropriate to mention here that today there exist very remarkable emulsions for special purposes, which might be of great value in an orbiting telescope if we can treat them for reciprocity failure. The Eastman film type SO-132 or 4404, "designed for extremely high-altitude, stable-platform photography," is rated with a resolving power of 475 lines/mm at a test-object contrast of 1000:1 and a root-mean-square granularity of 0.023, both in excess of the V-O ratings. The ultra-fine-grain emulsion, Kodak Type SO-243, has similar characteristics and could be used at small f-ratios.

The restrictions on the dimensions of the diffraction-limited field imposed by the guidance requirements have been briefly analyzed in the Fecker report.[2] It is stated here that with a 120-inch it should be possible to guide on a 16th-magnitude star. Mean star densities from star counts all over the sky lead to the estimate that in a 60 sec of arc field there would be, on the average, three stars to guide on. This estimate appears somewhat optimistic, and most probably, for extragalactic work, estimates of the number of possible guide stars should be based on star densities at the galactic poles. A field of 3 min of arc at an f/15 ratio would certainly satisfy all of the guidance requirements and also be sufficient for most of the high-resolution astronomical programs.

* But see p. 30, "Note on Recent Developments in Image Tubes" added in proof.

References

1. Systems Study of a Manned Orbital Telescope: Midterm Report. Boeing Co., Seattle, Wash., May 1965.

2. Final Report, Feasibility Study of a 120-Inch Orbiting Astronomical Telescope, J.W. Fecker Division, American Optical Co., Pittsburgh, Pa., 1963.

Both these reports were prepared for the NASA Langley Research Center.

INFRARED DETECTION

F. J. Low

The following is a brief review of the present status of detection techniques at wavelengths from 1 to 1500 microns.

As a starting point, let us consider the photomultiplier tube with the S-1 surface. The peak quantum efficiency of about 2% occurs at 0.8 micron and, when the tube is cooled to dry-ice temperature, it has a noise equivalent power (N.E.P.) of about 2×10^{-16} watt. At longer wavelengths, the efficiency drops off rapidly.

As we proceed to longer wavelength devices, the relation between detector area and N.E.P. must be taken into account. This subject is somewhat simplified if we stipulate that the smallest focal-plane diaphragm which is useful on large astronomical telescopes is 1 mm in diameter. There are exceptions to this rule, however; in particular, the use of a cooled Fabry lens has been shown to permit detector areas as small as 0.25 mm x 0.25 mm. In future space telescopes, even smaller detector areas should be usable.

At wavelengths from 1.2 to 3.5 microns, the most sensitive detector used thus far for astronomical measurements appears to be a PbS cell cooled with liquid N_2. Its N.E.P., as reported by H.L. Johnson, is 1.0×10^{-14} watt with an area of 0.25 mm x 0.25 mm. Although the peak response and sensitivity of the PbS detector can be varied a little with temperature and construction, it has already reached its practical limit of development. Recent work with doped germanium cooled to 4°K indicates that the gap between the S-1 and PbS peaks can be partially filled in. Experiments with this inverse photoconductive effect covering the range 0.7 to 1.7 microns show that, because of its extremely low noise, this new detector may come close to an N.E.P. of 1×10^{-16} watt.

In the range between the PbS cutoff, at 3.5 microns, and the practical onset of superheterodyne reception, at 3.5 mm, there are two classes of detectors to be considered: (i) the cooled thermal detectors, represented in practice by the low-temperature germanium bolometer and (ii) the cooled photoconductors, represented chiefly by impurity-doped germanium cells such as mercury-germanium.

The latter class includes a wide variety of detectors developed originally for military applications in which the background radiation is high and the response must be very fast. Great attention has been paid to minimizing the cooling requirements for these detectors. Mercury-doped germanium cells cooled with liquid H_2 have been applied to ground-based astronomical observations, providing an N.E.P. of about 1×10^{-12} watt at 12.5 microns. Various other dopants have been used to extend the long-wavelength cutoff. It appears likely that the N.E.P. of very small detectors of this class may be better than 1×10^{-13} watt. Indium antimonide cooled to liquid-helium temperatures provides a unique photoconductive response out to wavelengths of a few millimeters. A large-area (about 1 cm^2) Putley detector has been reported at 1×10^{-11} watt.

Unlike the photoconductive cells, the cooled thermal detector can, in principle, be designed to work efficiently at any wavelength. The germanium bolometer has been used in a radiometer optimized for the 1.0- to 1.5-mm window. When this instrument is cooled to 2.0°K, an N.E.P. of 4×10^{-14} watt has been achieved. A number of 1 mm x 1 mm germanium bolometers have been used at 5, 10, and 20 microns with an N.E.P. about 2×10^{-13} watt. At a minimum size of 0.25 mm x 0.25 mm and with special care in reducing the background radiation, the N.E.P. at all wavelengths should be about 1×10^{-14} watt for bolometers cooled to 2°K. By cooling to 0.9°K, the practical limit for ordinary liquid helium, the N.E.P. should be reduced to 2×10^{-15} watt. If liquid helium-3 is used to reach 0.25°K, even higher sensitivities may be attained. At temperatures below 1°K, a number of possible thermal detector mechanisms should be considered. It is quite clear that materials other than the few that have been investigated may provide substantially higher sensitivites. Indeed, the germanium bolometer is only one approach among many.

The devices discussed above have all been broad-band incoherent detectors. The maser or laser should be included in the discussion since it may provide amplification or detection very close to the photon limit at a specific wavelength. However, the narrow bandwidths attainable restrict the usefulness of such devices in astronomy. If a laser were tunable over a sufficiently large range, certain types of spectroscopy would become possible, particularly the study of narrow emission lines. It should be noted in this respect that even if a maser were available at 1 mm, its sensitivity as a radiometer for continuum measurements would be less than that of the best thermal-detection radiometer utilizing the low-temperature germanium bolometers which we already have.

Thus far, only individual detectors have been considered. Although one or more individual detectors may be used to scan an area of sky to build up an image or to scan a spectrum, the light-gathering power of the telescope is then only partly utilized. The problem is not only to separate the telescopic image into its independent elements but to record all of the information each element contains. This goal is approached by the photographic plate and by photoemissive image tubes. Unfortunately, existing infrared image devices fall short of the goal in both sensitivity and storage capability. This remains the case even though diffraction reduces the number of independent elements in the image at longer wavelengths. The bright sky background in the infrared would reduce the effectiveness of such devices for ground-based observations; however, their significance for space astronomy cannot be overemphasized.

From the foregoing discussion the following conclusions may be drawn: despite sizable advances in detector technology, the limiting factor in a well-designed infrared spaceborne telescope is detector sensitivity. Unless new breakthroughs occur in photoconductive techniques, the best hope for extending sensitivity beyond 2 microns is to develop thermal detectors cooled below 1°K. Although great effort is required to improve image devices in the infrared, their potential applications in space astronomy are so fundamental that their development must be continued.

NOTE ON RECENT DEVELOPMENTS IN IMAGE TUBES

L.W. Fredrick
(Added November 1965)

Dramatic developments in the field of high-resolution phosphors for image tubes have been reported by several research groups at the Symposium for Photoelectric Imaging Devices held in London, 20-24 September 1965. Improved techniques now make it possible to deposit phosphor grains with diameters in the range of 5 microns down to less than 1 micron, to form screen coatings that yield image resolutions of 100 line pairs/mm or better. This resolution is degraded somewhat in the multiplication type intensifiers. The efficiency of the new coatings is claimed to be better than the older coatings by one group, but not quite as good by another group. Nevertheless, the prospect that image tubes that compare favorably with photographic emulsions for high-resolution astronomical work will become available within a few years, is greatly enhanced as compared with the prospects in June 1965 when the Summer Study met.

At the same symposium, developments in image orthicon tube technology were announced that should improve the application of this type of tube to astronomical problems. In particular, the Westinghouse group discussed the new secondary electron conduction target and the long storage times available. Images were stored and retained for periods of 24 hours with the tube turned off.

III
Solar Astronomy

1. INTRODUCTION

AIMS OF THE WORKING GROUP

As part of a summer study of space science by the National Academy of Sciences, the Solar Astronomy Working Group met at Woods Hole, Massachusetts, from June 21 to July 3, 1965. In many ways this study can be regarded as a follow-up to a similar conference held at the State University of Iowa in 1962, the proceedings of which have been published as "A Review of Space Research" (Publication No. 1079 of the National Academy of Sciences—National Research Council).

The direct objective of the Solar Astronomy Working Group was stated as follows: "To examine future needs of, and opportunities for research in solar astronomy, using all space techniques ranging from orbiting observatories more advanced than the Advanced Orbiting Solar Observatory (AOSO) and possible manned facilities, to small satellites and sounding rockets."

Scientists participating in the work of the Working Group on Solar Astronomy are listed in the Appendix. The recommendations of the Working Group are listed at the end of this Section; the discussion leading to these recommendations is found in Section 2.

SOLAR ASTRONOMY AND SOLAR SPACE ASTRONOMY

At the beginning of a report on solar space astronomy, it is appropriate to emphasize the unity of solar research, and to point out that observations from space and observations from the ground are simply two aspects of a single venture. Clearly, if one wants to obtain all possible information about the Sun, one uses every means at his disposal; one must never forget that space and ground-based astronomy are partners—not competitors—and the knowledge to be gained from using the two together is very likely greater than the sum of the two used separately. If, for example, the time development of flares could be followed in the light of He I λ584 from a spacecraft and in the light of He I λ10830 from the ground, it would be far more valuable to observe a particular flare simultaneously in both lines than simply to observe any flare in either line separately.

There are three major reasons why the Sun is important: first, because of its intrinsic interest as the only nearby star; second, because of its effect on the planets and, in particular, the Earth and the human race; third, because it is a valuable astrophysical laboratory.

As the only star on which detailed observations can be made, the Sun holds a unique position in stellar astronomy. Because the disk of the Sun can be resolved, single features—sunspots, prominences, granules, and flares—can be observed, and spectral scans across the disk from center to limb can be made, which in turn give directly the temperature and density in the photosphere as they vary with depth. The solar atmosphere can be used as a standard against which theories of stellar atmospheres and spectral-line formation can be tested. Stellar spots and cycles of activity can at present be studied in detail only on the Sun. Theories of convection can be tested on the Sun, where granulation and chromospheric microstructure can be seen. Chromospheres and coronas are seen nowhere else in as great detail as on the Sun. Finally, the Sun is a stable, main sequence star that serves as a photometric and spectroscopic standard for stellar work. Every new piece of information about the Sun contributes to our comprehension of the stars.

Knowledge of the Sun is indispensable to an understanding of the physics of the Earth and other planets. The Sun is the source of virtually all the heat and light a planet receives. Photon flux from the Sun causes dissociation, excitation, and ionization of atoms and molecules in planetary atmospheres, produces ionospheres, maintains planetary heat budgets, causes escape of atmospheres from the planets, is responsible for weather, and controls any life that exists on the planets. Particle flux from the Sun affects planetary ionospheres and magnetic fields, produces auroras, fills the interplanetary region, and changes the chemical abundances by producing nuclear reactions in planetary atmospheres. In addition, solar radiation (photon and particle) affects the Moon, comets, meteoroids, and asteroids, as well as grains in interplanetary space. In fact, the whole solar system can be viewed as being imbedded in the outer solar corona.

The importance of the Sun to life can scarcely be overemphasized: solar energy ultimately sustains practically every living organism—both plant and animal. Radiation from the Sun has also controlled the atmosphere and environment in which life has developed, and thus has been the principal determinant in shaping the course of evolution. Biology is therefore interested not only in the present radiation (photon and particle) from the Sun but also in any changes that may have occurred during the Earth's history. With the dawn of the space age, an upsurge of interest in exobiology—life outside the Earth—has taken place. This has increased the demand for an intimate knowledge of all solar radiation, especially in the ultraviolet and x-ray regions of the spectrum.

A subject deserving special consideration under the general topic of the effects of the Sun on its environment is the effect of the Sun on man. Living as he always has, on Earth, where the atmosphere shields him from the harmful radiations of the Sun, man has tended to look upon the Sun as completely benign. With the coming of the space age and man's emergence from within the protective atmospheric envelope, however, the problem of shielding astronauts from solar radiation must be faced. Solar protons (and possibly x rays) emitted at the time of solar flares pose the most serious danger. If man is to work in space, outside of a heavily armored module, the Sun must be constantly monitored so that the astronaut will have immediate warning of any solar event from which radiation might be sufficiently intense to force him to re-enter his spacecraft. Further knowledge about the nature of flare events, which would enable scientists to better predict their occurrence or better shield the astronaut, would be of substantial benefit.

Ever since Janssen's discovery of helium on the Sun, it has been apparent that the Sun can be used to complement the terrestrial scientific laboratory, for there are available in the Sun combinations of temperature, density, and path length quite beyond terrestrial capabilities. Although its potential value in this respect has not been fully utilized (and is perhaps not clearly appreciated by all scientists), the Sun has been exploited as a laboratory by workers in several branches of physics. The measurement of precise wavelengths of spectral lines from highly ionized elements in the corona was used by Edlén to determine a number of atomic parameters otherwise inaccessible. Recently, both in this country and abroad, scientists have used the Sun as a source for spectra of wavelengths that are not otherwise producible in the laboratory. In the same way, observation of many strong lines near 170 Å in the solar spectrum stimulated efforts in several laboratories to obtain spectra from highly ionized atoms in an attempt to identify these solar lines. As the ultraviolet spectrum of the Sun is examined more carefully, one may expect to gain important new knowledge of atomic physics and spectroscopy. The Sun has also been, and still is, important for studying nuclear reactions and testing theories of nucleogenesis. The solar atmosphere provides—in such features as spicules, the corona, and the convection zone—a large-scale laboratory in which phenomena of aerodynamics and hydrodynamics can be observed. In fact, observations of the outward particle flux from the Sun have stimulated a great deal of important work on the solar wind and interplanetary plasmas. Finally, the Sun provides unique opportunities for the study of plasma physics and magnetohydrodynamics. In addition to the solar atmosphere itself, which can be regarded as a giant plasma, such features as sunspots, prominences, flares, and the corona are examples of the interaction between plasmas and magnetic fields. The Sun is a readily available source in which many waves and oscillatory phenomena of interest in magnetohydrodynamics—acoustic waves, magneto-acoustic waves, electromagnetic waves, and Alfvén waves—occur naturally. As solar research progresses, discoveries of interest to several disciplines will undoubtedly be made, and many new uses will be devised for our convenient astrophysical laboratory.

In summary, we emphasize once again the central position of solar astronomy and the contributions that it makes to such fields as stellar astronomy, radio astronomy, planetary atmospheres, interstellar matter, biology, atomic physics, and meteorology, as well as its influence upon such very practical matters as radio communication on the Earth and the safety of an astronaut.

Although the reasons for observing the Sun from above the Earth's atmosphere have already been listed in many places, we mention them once again in order to remove any existing doubt concerning the benefits to be gained by putting telescopes into space. The most obvious reason is the possibility of observing the ultraviolet and x-ray solar spectrum. Gases in the Earth's atmosphere completely block from our view all radiation with wavelengths shorter than about 2900 Å. Yet this obscured spectral band is vitally important in understanding the Sun for several reasons: the resonance lines of most elements lie within it; it is emitted by the interesting region of temperature inversion just above the photosphere; it contains many of the strongest coronal lines; and the most violent variations of radiation with solar activity are observed within it. The preliminary work that has already been done in the ultraviolet spectral band by means of sounding rockets and satellites has added greatly to our knowledge of the Sun. More complete observations (including x-ray and gamma-ray detection), offer the most fruitful means of understanding the processes taking place in the upper solar atmosphere. It is especially important for observations to be made during the years leading up to and through the coming solar maximum of 1967-1970.

A related advantage, and one that has not often been discussed, is the opportunity to observe the Sun in the far infrared—from 20 microns up to 1 millimeter. Our ignorance of this region is almost complete; we do not even know the energy distribution, much less any details of molecular bands or lines. Although the infrared is probably of less importance than the ultraviolet, and for this reason has often been neglected in space-science planning, there are several interesting observations to be made; for example, the detection of the radiation from the photosphere-chromosphere interface (somewhere between 20 and 200 microns) and observation of the molecular spectra of sunspots. Questions about the temperature structure of cool regions (such as above sunspots and near the temperature minimum) can perhaps be answered by observations at those wavelengths. Again, since the atmosphere of the Earth entirely prevents us from detecting this radiation, such observations can only be obtained from space (perhaps, in this case, airplanes or balloons would suffice).

A crucial advantage that could be obtained from space observation is the increased resolution (clarity) of small features on the Sun. The developments of the past 20 years in the theoretical and observational study of the dynamical and magnetic properties of the quiet solar atmosphere and the many features of solar activity focus increasingly on the smallest observable structures. Every improvement we achieve in spatial resolution reveals new detail of the greatest significance. The size distribution of granulation, the fine structure of magnetic fields, the stranded structure of loop prominences, the local variations in line profiles, the turbulent velocity fields, and the minute structure in flares are examples. Ground-based observation has carried us to the verge of solutions, but in most instances falls short of the theoretically decisive resolution. While it is obvious that there will always be interesting details too small to be seen with any resolution we may attain, many features of the solar atmosphere should have a size scale in the neighborhood of the scale height. Except for the very thin layer at the base of the photosphere, the scale height is of the order of 100 km or more. This corresponds to 0.14 sec of arc in angle. The practical resolution limits of ground-based solar observation are about 0.5 sec of arc for direct photographs and 0.8 sec of arc in the best spectra. Resolution of this grade is the kind one achieves only on very rare occasions in one lucky photograph or spectrogram. The limit is set by poor seeing in the Earth's atmosphere, and could be overcome by space observation.

Another limitation imposed by the Earth's atmosphere is the brightness of the sky. The bright sky near the Sun, at the best observing sites, is rarely less than 10 times the brightness of the inner corona. Nevertheless, ground-based observation has successfully detected the polarized component of the brightest streamers of the white corona out to a height of one solar radius above the limb; however, direct measurement of the electron-scatter brightness and detection of the corona beyond one radius are impossible except during a total solar eclipse. Any continuous watch from the ground for outward-moving plasma clouds is also out of the question. Since the sky is completely dark in space, we can expect to monitor the Sun's corona routinely from space vehicles, which will lead to major advances in our knowledge of coronal processes.

A final important benefit to be gained from observation in space is the continuity in time that can be obtained. Often, critical observations need to be extended for several hours, days, or even months, in order to study the time development of certain features, such as prominences and centers of activity. Such observations must ultimately be made from satellites where they will be independent of terrestrial meteorological and diurnal effects. Real-time monitoring of solar events will be accomplished by such satellites, making communication delays short or nonexistent as telemetry reception becomes possible at the user's site. As space observations become routine, close cooperation between ground and space observations must be maintained for maximum benefit of such solar monitoring.

CURRENT SOLAR PROBLEMS

Solar physics has as an ultimate task of describing completely the structure, understanding thoroughly the dynamics, comprehending fully the origin and development, and predicting exactly the future evolution of the Sun. These ultimate goals, which can never be fully attained, are perhaps best expressed in the form of more specific questions that might be asked about the Sun. The following are some of the major questions with which solar physics is now grappling. What are the details of the processes by which energy is transferred outward from the center of the Sun? What is the source of the sunspot cycle and solar activity? Why, and how, do solar flares occur? How are energetic particles and photons produced? What is the detailed structure of the chromosphere, and what is its connection with the corona and with magnetic fields on the Sun? What is the nature and cause of spicules and prominences? Why is there a solar corona, and how does it produce the solar wind and interplanetary medium? What is the origin and early history of the solar system? What produces the equatorial acceleration of solar rotation? How is the solar magnetic field produced and what is its effect on the solar activity cycle?

Such questions, which have been formulated and listed many times before, are the grand questions toward whose solution all solar physics is directed. But these are too comprehensive to be answered fully or even attacked intelligently. Instead, most solar physics is directed toward the solution of much more specific problems in the hope that the accumulated answers to many smaller problems may eventually provide answers to these larger questions.

The following are some of the more important specific questions with which present solar astronomy is engaged:

Photosphere. What are the size and velocity distribution of the solar granules? What are the source and structure of the weak magnetic fields produced at the surface of the Sun? What is the simplest nonhomogeneous model of the photosphere? What is the relationship between the granules and the super-granulation? Why is there a temperature inversion? What is the detailed variation of temperature with height through the temperature minimum? What is the vertical structure of features observed in this height range? Do these structures vary with the solar cycle?

Chromosphere. What is the morphological specification of the vertical structure (including the size distribution)? What is the velocity distribution in this region and how does it vary both horizontally and vertically? What is the detailed structure of the cells of the chromospheric network? How does the magnetic field vary across the cell boundary? Which features inside the cells are periodic in time? Are any observed features rotating? Is the chromospheric oscillation vertical or horizontal? What determines its period? How does the temperature vary horizontally across the chromospheric cells? How are spicules produced? What is their microstructure? What are the magnetic fields in spicules? Why do spicules seem to favor the edges of the cells? What other aerodynamic phenomena take place in the chromosphere? What is the energy budget of the chromosphere? What types of waves are propagated in the chromosphere and what are their results? What is the temperature gradient as one passes from chromosphere to corona? What causes this temperature gradient? What is the nature of the transition from spicules to the corona? How should we interpret spectral lines formed in the absence of local thermodynamic equilibrium?

Corona. How is the corona heated? Is the corona localized over the edges of the chromospheric cells? Is the corona in equilibrium? What is an M region? What is a coronal streamer? What is the relation of streamers to the solar wind? What coronal phenomena give rise to radio bursts and gusts in the solar wind? What is the magnetic field of the corona and what is its effect on corpuscular streams? How do radio bursts escape from the corona? Is there a difference in chemical composition between corona and photosphere?

Flares. What is the primary flare phenomenon? How do flares occur? What is the source of energetic particles in a flare? Is the energy released primarily over a small hot kernel? What is the relation of the flares to the surrounding magnetic field? How are flares related to the coronal condensations? Are x-ray flares observable at extreme ultraviolet wavelengths? How do active regions with intense magnetic fields arise? What is the physical structure of active regions?

Sunspots. How is the observed brightness distribution across a sunspot maintained? What is the physical structure of sunspots? What is the relation of the magnetic field to the fine structure? How does the granulation behave inside the spots? What is the fine structure of the umbra below the resolving power of Stratoscope I? What system of gas motions exists around the sunspots?

Plages and Faculae. What is the microstructure of plages and of faculae? What is their relation to each other? What oscillation takes place in the faculae? How are the faculae related to the network cells? Is there a difference in coronal heating above faculae and above active regions?

Prominences. Is there a basic lower limit to the size of filamentary structures in prominences? What is the magnetic field in prominences? What is the mass and energy balance between the corona, prominences, and the lower atmosphere? What causes eruptive prominences? Why are loop prominences so extremely hot? What are their special relation to solar flares?

ORGANIZATION OF THE PANEL

To meet its objective, the Working Group divided its assignment into three parts:

1) A critical review of the past and present NASA programs in solar physics and of the Prospectus 1965-1980;
2) An examination of major unsolved problems in solar physics and a specification of the instrumental requirements for their solution;
3) The recommendation of specific experiments that might be initiated in the three time periods 1965-1970, 1970-1975, and beyond 1975.

Certain other matters, such as the role of man in carrying out scientific observations in space and the relation of laboratory work to the NASA mission, were discussed as part of the solution of the problems of solar physics.

There are several alternative ways in which the Working Group could approach its assignments. The physically most meaningful is the problem-solving approach, in which the unsolved questions of solar astronomy (based on the list given above under Solar Astronomy and Solar Space Astronomy) are considered and then observations are sought that would aid in answering them. Experimenters generally prefer an instrumental approach, in which knowledge of the characteristics of an instrument is used to determine what observational data the instrument can obtain. A third possible approach might be termed vehicular, in that the capabilities (size and weight of payload, pointing accuracy, lifetime, orbit, power available, and data-storage and transmission capabilities) of the planned vehicles are first examined and then it is decided which observations could be made from them.

The Working Group favored the first approach, in general, as it felt that this approach would ultimately be most fruitful from a scientific standpoint. Although other viewpoints were adopted at times, an attempt was continually made to relate all discussions to the basic questions of solar physics.

To facilitate the work of the Working Group and ensure complete coverage of the field of solar astronomy, the Working Group was divided into four subcommittees. Since the detection and analysis of solar electromagnetic radiation forms the principal source of information about the

Sun, and since different regions of the spectrum come from different parts of the Sun, a division according to spectral region is to a large extent a division according to region or height on the Sun and hence according to basic questions about the Sun. This division of labor to some extent combines the problem-solving and instrumental points of view. The entire spectrum was therefore divided into sections and a subcommittee was assigned to study each section. Included in the charge given to each subcommittee was the request that it look ahead at least to 1975 and discuss problems, instruments (including the role of man), and vehicles. The assignments were as follows:

a) $\lambda < 500$ Å: Lindsay, Teske, Zirin;
b) $500 < \lambda < 1500$ Å: Athay, Firor, Orrall;
c) $1500 < \lambda < 3000$ Å: Johnson, Smith, Tousey;
d) $\lambda > 3000$ Å: Evans, Howard, Ney.

The reports of these groups were discussed by the Working Group as a whole and are contained in Section 3.

In addition, and partly as a result of the previous work, several informal subcommittees were designated to study and make recommendations about such other topics as the use of rockets, the role of man, the relation of the astronaut-observer to the scientist directing the experiment, the role of the ground-based laboratory in the space effort, and the question of a Moon-based observatory. Discussion and recommendations concerning the findings of all subcommittees are found in Section 2.

RECOMMENDATIONS

The full texts of the recommendations of the Working Group on Solar Astronomy are presented below.

Recommendation 1. (a) That the recommendation of the Iowa meeting (1962) concerning fine pointing be given immediate attention and that highest priority be given to the development of triaxially stabilized rocket attitude controls, leading, as soon as possible, to a fine-pointing system capable of an accuracy of 5 sec of arc and optimally designed for solar use. (This recommendation is essentially a reaffirmation of that made by the Iowa Summer Study, and is restated here to reflect the importance which the Working Group attaches to this matter: "We recommend that the sounding rocket program continue to receive full support; and that both the inertially guided Aerobee with fine pointing at selected stars, and the inertially guided Aerobee with fine pointing at the Sun controlled by an optical sensor be made available at the earliest possible time.")

(b) That other improvements (such as increased payloads and peak altitudes, increased reliability, and more dependable recovery techniques) be made in existing rocket systems;

(c) That the number of rockets available per year for research in solar astronomy be at least doubled;

(d) That funds for payload development be increased to an adequate level, especially when the triaxial pointing controls become available.

Recommendation 2. (a) That the presently approved Orbiting Solar Observatory (OSO) program be augmented by at least four additional launchings during the period 1970-1972 inclusive;

(b) That no decision be made to terminate the OSO program after 1972 without further review at an appropriate time;

(c) That NASA make every effort to implement such desirable improvements in the OSO spacecraft as increased power, offset pointing, localized raster scans, provision for slightly longer instruments, greater data capacity and more flexible data format, and improved pointing accuracy (15-30 sec of arc);

(d) That consideration be given to injection of one or more OSO spacecraft into a polar retrograde orbit in order to provide continuous surveillance of the Sun.

Recommendation 3. (a) That a satellite with Advanced Orbiting Solar Observatory (AOSO) specifications is an indispensable next step in NASA's solar program, and must be flown close to the coming solar maximum;

(b) That the AOSO program be accorded all the priority necessary to maintain the launch schedule shown in the Prospectus.

Recommendation 4. (a) That manned missions in the 1968-1972 time period, such as the Astronomical Telescope Orientation Mount (ATOM) in the Apollo Extension Systems, are desirable to supplement AOSO, but cannot replace it;

(b) That because it offers the prospect of providing answers to critical questions relating to the technology of manned space telescopes and data recovery, the ATOM concept merits vigorous support.

Recommendation 5. That solar space observation be included in the manned space science program of the Apollo Extension Systems in order to develop the technology of manned space astronomical operations. Such observations, which could attain resolving power of 1 sec of arc in the wavelength region 500-3000 Å, mark the next logical step beyond both AOSO and ATOM.

Recommendation 6. That feasibility and design studies begin immediately on orbiting solar telescopes of at least 1-meter aperture designed to obtain a resolution of 0.1 sec of arc at visible wavelengths and 0.5 sec of arc at far ultraviolet wavelengths ($\lambda > 500$ Å). Very large and complex accessory instruments will be necessary to analyze the solar image. Erection, operation, and maintenance of this telescope will require full utilization of astronaut-engineers and scientists.

Recommendation 7. That provision be made for a continuing, uninterrupted experimental program while the more advanced manned flights are in preparation, with many flights of various spacecraft, so that a scientist will have frequent opportunities for observation.

Recommendation 8. That NASA find means to continue a strong program with relatively inexpensive rockets and small unmanned satellites at the same time the large manned projects are under way, since the former are indispensable to the latter.

Recommendation 9. That the relationship between scientists and astronaut-observers be studied and clarified. In particular, we recommend that when a single, large scientific instrument is carried, the scientific observation be designated the primary mission for the flight.

Recommendation 10. That NASA bring more scientists into the space flight program as astronauts or observers.

Recommendation 11. That NASA move to provide additional support for ground-based solar studies. As the flight program grows in sophistication and success during the next several years, the demands on ground-based work will also increase, and NASA should in turn anticipate an increased demand upon its resources for support of ground-based facilities and operations. In addition, in the next few years, NASA should expect, and respond favorably to, proposals for a few major ground-based solar installations.

Recommendation 12. That increased support be given to physical research in the laboratory, as required to develop improved space instrumentation for solar-physics research, to assist in the data reduction, and to make possible a full interpretation of the results.

2. DISCUSSION OF RECOMMENDATIONS

GENERAL REMARKS

Scientific progress is usually achieved by a steady, step-by-step process—each step being built on the knowledge obtained in the previous step. Since this process has been of almost universal validity and utility, we would do well to remember it in connection with space science. Because of the rapid progress of astronomy in the past few years, and because of the truly marvelous opportunities for research and discovery arising from man's conquest of space, both scientists and nonscientists are likely to be rather speculative in their thinking about the opportunities of space. We therefore emphasize that space research should be done, insofar as possible, in the traditional, tested manner of science. Thus, for example, such large and expensive projects as the Apollo Extension Systems (AES) and the Manned Orbiting Telescope (MOT) should be firmly based on instruments and methods that have previously been thoroughly tested and proven. It also seems wise to connect planned space research with specific problems, such as those discussed previously, and with the new questions that will undoubtedly be raised in the course of further exploration.

We find good reason to be optimistic about the progress in space research that has already been made. Even though the United States' space effort is still in its infancy, many solid scientific results have already been produced. NASA has shown a great deal of foresight and imagination in developing the large and sophisticated spacecraft now being flown or built, and in providing the scientific community with opportunities for space observation. We commend NASA for the success thus far obtained.

In agreement with our decision that space science should be done in an orderly, step-by-step fashion, we envision a continuing series of instruments of increasingly greater complexity, range, power, and cost—extending from sounding rockets to OSO to AOSO to AES and finally to MOT. This tidy progression does not, however, tell the full story, and several important factors must be considered in order to understand the comprehensive plan that solar physics must follow in accomplishing its mission.

The Sun is a quasi-periodic variable star, exhibiting an approximate 22-year period of magnetic activity and an 11-year period during which the average number of visible sunspots and the scale of energetic activity follow striking cyclic patterns. A great variety of transient phenomena on the Sun, some of which disturb the atmospheres of planets, also vary in intensity and complexity with this cycle. In fact, the study of solar behavior at different phases of the solar cycle represents a major branch of solar research, its aim being to arrive at an understanding of the physical nature of solar phenomena and a theoretical explanation of the cycle itself.

The Sun's variability is a major reason why a given solar observing technique or instrument does not become obsolete after it has been employed on one or two flights; its minimum useful life is 11 years at least and may be as long as 22 years.

It is certainly true, as pointed out in Section 1, that only by means of such systems as AOSO, AES, and MOT can solar physics achieve the high spatial and spectral resolution required for the solution of many fundamental problems. At the same time, there are and will continue to be many problems that can be attacked with instruments of less power and lower cost. Thus, although the goal of the solar physics flight program must be to develop observing facilities with progressively greater range and power, the precious observing time of such large and costly facilities should not be used to make observations that can as easily be obtained with smaller and cheaper equipment.

This same philosophy has long been followed productively in ground-based astronomy, where small telescopes continue to be usefully employed despite the existence of many telescopes in the 100-200 inch class. It does not, however, seem to be followed in present NASA program planning, as set forth in the 1965 Prospectus. For example, it is apparently assumed that once the AOSO spacecraft is available there will no longer be any need for the OSO; and, indeed, in the 1965-1980 Prospectus there is an overlap of but one year, 1969, in which the last OSO and the first AOSO are to be launched. Similarly, the AOSO is to be phased out in about 1975 with the advent of the MOT. Finally, although the sounding rocket program is planned as a continuing program, its support level is expected to remain essentially constant for the indefinite future. On the other hand, there is every reason to expect a substantial increase in the number of sounding-rocket launches that will be required during the next few years, both to test experiments to be flown later in satellites and to attack solar problems that do not require satellites for their solution.

One of the features of space science which has most discouraged scientists from embarking upon space research programs is the very long lead time between the conception of an experiment and the acquisition of results. Unfortunately, the program outlined in the Prospectus would seem to increase rather than decrease this time interval. The rather minor character of the rocket program outlined in the Prospectus, and the termination of OSO launches by 1970, leaves solar physics only with projects requiring lead times of 3-5 years. This will make it exceedingly difficult to attract university experimenters to the program, except as they may advise or consult with industrial companies or government laboratories. The effects of this trend on the training of graduate students could be disastrous. Thus, the present Prospectus in solar physics, if followed, could well be self-defeating.

The NASA space flight program in solar physics must be viewed as one element in a broader national program of research into the physical nature of the Sun. Other major research activities, in which most of the country's solar physicists are now engaged, are ground-based observations, laboratory experiments, and theoretical investigations. If more solar physicists are to be attracted into the NASA program as experimenters, they must be given an opportunity to remain solar physicists and not be converted into instrument developers. If they are expected to give up a major fraction of their current research activities in favor of space solar physics, they must be able to do space solar physics in a reasonably short time scale. It is true that the design and construction of a large solar observatory on the ground may also require about five more years—but with guaranteed results, whereas space experiments are still fraught with the possibility of failure. Eventually, when a large permanent solar observatory has been erected in space, the problems associated with long lead time will be solved, because observational data may then be called forth by radio command. But, in the interim, the solar physics program must continue to provide opportunities for experiments with rockets and small satellites.

In connection with the difficulty of attracting more investigators into the space science program, it might be well to point out one other important consideration. As larger and more complicated experiments become possible, it is apparent that experimenters must work more and more in groups. The time has, in fact, already passed when a single experimenter could conceive of an experiment, design an instrument, and analyze the results. In order to build a stronger base of space scientists in the university community, it is important that NASA make a firm effort to encourage the development of new groups in plasma physics and atomic physics, but with an interest in space science.

SPECIFIC VEHICLES AND INSTRUMENTS

Balloons and Airplanes

The Working Group did not study the use of these vehicles in detail, for there is little new to be added to that already written in several places. We endorse the statements of the Iowa study, noting that since that time, balloons have carried several valuable experiments and the first observations from the X-15 have just been made. In addition, such other aircraft as the current generation of jet transports (some of which have already been used to study solar eclipses) and the proposed supersonic transport offer both the prospect of longer observing time and the certainty of visibility for future eclipse study. Infrared observations of the Sun (in the wavelength region between 20 microns and 1 mm) constitute an important new area of research that can be done effectively and cheaply from balloons or airplanes, and we encourage the endeavor. These vehicles will continue to be of use in solar-space-science work for an indefinite time in the future.

Rockets

The Office of Space Science and Applications Prospectus 1965, listing program opportunities for 1966-1985 (in a Draft dated June 10, 1965), is based on two possible situations: a constant budget (called the Minimal Growth Program) and a 5% per year increase above this. The proposed program in Physics and Astronomy, however, contains a tremendous increase in manned projects, principally the MOT, and an early termination of OSO, phasing out of AOSO and OAO, and a constant level for sounding rockets and space research and technology studies.

Although such a proposed program represents vigorous progress in the development of spacecraft and vehicles, it also contains a danger to which those planning the program must be alert. A large, complicated effort such as an MOT or even the AOSO requires extensive preliminary study and testing, which must include checking of actual components on rocket flights, in order to insure effectiveness of the final flight equipment. Rocket experiments, ground observations, laboratory studies, and OSO experiments will all be necessary to plan adequately and intelligently a spectroheliograph, for example, which can take advantage of AOSO capabilities. Furthermore, even in an era with AOSO or MOT capabilities, scientific questions will continually arise that can be answered with a single rocket flight. Therefore, if we allow the space program to increase in complexity at the expense of eliminating or making ineffective the more straightforward techniques, we may find ourselves unable to plan adequately the complex ventures.

We can see then that a program of increasing complexity and cost can be conducted well either with a steadily increasing budget, or by allowing sufficient time for the complex projects so that they can build upon, and do not cripple, the simpler techniques.

A particular example of a situation in which a serious imbalance of this sort has developed—that of sounding rockets—was discussed by the Working Group. Improvements are greatly needed in the performance and reliability of sounding-rocket vehicles. Not only will rockets continue to be used for direct studies in solar physics; they will also be required for prototype work on OSO, AOSO, AES, and MOT. Rockets will also be needed for the periodic checking and calibration correction of many long-lived orbiting experiments, and also for carrying out particular researches suggested by satellite results.

The Prospectus simply does not include a sufficient budget for rockets, their improvement, new rocket systems, and instrumentation. It is urged that Aerobee and other rockets, suitably improved, be provided in quantity adequate for all needs in solar research that can be met by their use. It is estimated that 25 to 40 Aerobee-150 rockets, many containing a triaxial stabilization system, will be needed each year from now until 1975, although this estimate might be changed by a significant improvement in rocket performance or reliability.

This need for continued development of rockets was clearly recognized at the 1962 Space Science Summer Study and a recommendation was formulated as follows:

"Recommendation. We recommend that the sounding-rocket program continue to receive full support; and that both the inertially guided Aerobee with fine pointing at selected stars, and the inertially guided Aerobee with fine pointing at the Sun controlled by an optical sensor be made available at the earliest possible time."

Thus, the need for triaxially stabilized Aerobees was clearly defined in the Iowa report, and this recommendation has been restated many times since by the NASA Solar Physics Subcommittee. Unfortunately, progress has been slow or nonexistent. Little or no improvement has been made in rocket performance, reliability, or recovery. Failures in the Aerobee-150 and parachute recovery continue to be numerous, and the opportunity to achieve greater altitude with a small redesign has not been seized. Even now, the "solar-pointed" Aerobee, as envisioned in the 1962 report, is not in sight.

In the meantime, the U.K. has constructed and flown successfully three solar-pointed Skylark rockets. This has already placed them ahead of the United States in one branch of solar research. Stop-gap measures have been resorted to in the USA; for example, the combination of an Attitude Control System (ACS) and Biaxial Pointing Control (BPC), flown by the Goddard Space Flight Center, the small triaxial version of the BPC being constructed by the University of Colorado, and the cross-spin stabilization system of Kitt Peak. This shows how great is the need for the triaxial-solar-pointed Aerobee. It is already too late to use it for testing the design of instruments to be flown in the first AOSO, but perhaps not for the second.

In view of these considerations, the Working Group recommended:

Recommendation 1. (a) That the recommendations of the Iowa meeting (1962) concerning fine pointing be given immediate attention and that highest priority be given to the development of a triaxially stabilized rocket attitude controls, leading, as soon as possible, to a fine-pointing system capable of an accuracy of 5 sec of arc and optimally designed for solar use;

(b) That other improvements (such as increased payloads and peak altitudes, increased reliability, and more dependable recovery techniques) be made in existing rocket systems;

(c) That the number of rockets available per year for research in solar astronomy be at least doubled;

(d) That funds for payload development be increased to an adequate level, especially when the triaxial pointing controls become available.

There is also the question of the continuation of the OSO series. The spacecrafts OSO-I and OSO-II performed almost perfectly. Preparation of the major instrumentation was commenced for OSO-II four years before flight, and was done with the greatest possible care. Nevertheless, one major experiment failed completely, another was only a partial success, and two others failed sooner than they should have. The conclusion is clear—much more work must be done in rockets and in the laboratory, prior to freezing designs of experiments for satellites, and the previously described improvements in rockets are greatly needed. A second conclusion is equally clear: OSO should be continued far beyond OSO-H; it is an excellent spacecraft and it bears a relation to AOSO that is similar to that of the Aerobee-150 rocket to OSO. As with rockets, OSO can be improved at a cost that is relatively small, to become a still more useful spacecraft, by increasing its pointing accuracy; by providing ground control of pointing, offset pointing, and fine raster scanning; by placing it in a Sun-synchronous polar retrograde orbit; by providing space for experiments at least five feet long; and by increasing the power, telemetry capability, and flexibility in data format.

Still other possibilities exist for this versatile spacecraft. It might, for example, be placed in an Earth-synchronous orbit so as to provide continuous real-time telemetry, thus making possible enormous data handling capability, as required to make use of television-type image-forming techniques. A stripped-down version of OSO launchable from a Scout, with far less expense and lead time but providing ample room and pointing accuracy for experiments intended mainly for solar monitoring, would be extremely useful for this purpose.

Additional considerations have already been given above under General Remarks. There is a range of experiments, beyond the capabilities of rockets, which OSO can do more effectively and cheaply than more advanced equipment. Monitoring of various solar features through the coming maximum and into the minimum beyond is such a task.

The Working Group therefore recommended:

Recommendation 2. (a) That the presently approved OSO program be augmented by at least four additional launchings during the period 1970-1972 inclusive;

(b) That no decision be made to terminate the OSO program after 1972 without further review at an appropriate time;

(c) That NASA make every effort to implement such desirable improvements in the OSO spacecraft as increased power, offset pointing, localized raster scans, provision for slightly longer instruments, greater data capacity and more flexible data format, and improved pointing accuracy (15-30 sec of arc);

(d) That consideration be given to injection of one or more OSO spacecraft into a polar retrograde orbit in order to provide continuous surveillance of the Sun.

Advanced Orbiting Solar Observatory (AOSO)

None of the recommended improvements in the OSO program, neither the improvements recommended in the spacecraft itself nor the additional flights recommended, in any way diminish the need for AOSO. The spacecraft specifications for AOSO and the significant improvements of these specifications over those for OSO were discussed at Iowa, and reference is made to that report. The crucial improvements in the AOSO spacecraft over the OSO are two: 1) much greater pointing accuracy and stability (5 sec of arc, about an order of magnitude greater than that obtained in OSO) and 2) much larger volume for optical instruments. In addition, AOSO will provide much greater data storage and telemetry capacity.

It is well to point out that AOSO is an advanced, highly specialized spacecraft designed for a particular mission, and it cannot be replaced during this time period (1970-1975) by any other spacecraft yet foreseen. In particular, it does not compete with the proposed ATOM of the Apollo Extension Systems (discussed below) for several reasons. AES is essentially a temporary flight of a duration of approximately one or two months, with only short intermittent solar observing periods, while AOSO provides continuous observations over many months or years. AOSO also possesses the crucial advantage of a pointing capability of 5 sec of arc, which is clearly beyond that planned for the early manned missions.

Since AOSO is the next logical step beyond OSO and is the only spacecraft available to do the jobs for which it was designed, the Working Group made the following recommendations concerning AOSO:

Recommendation 3. (a) That a satellite with AOSO specifications is an indispensable next step in NASA's solar program, and must be flown close to the coming solar maximum;

(b) That the AOSO program be accorded all the priority necessary to maintain the launch schedule shown in the Prospectus.

Apollo and the Astronomical Telescope Orientation Mount (ATOM)

Some of the early Apollo flights (in the 1965-1970 period) include flights in Earth orbit of a few weeks' duration. These flights provide excellent opportunities both for training astronauts in the techniques of astronomy and for making useful astronomical observations. Since the spacecraft is recovered, photographic film, with its great storage capacity, can be used. Photography is particularly valuable for obtaining high temporal resolution of transient events and for obtaining high spatial resolution in wavelength regions where there is adequate flux. In addition, longer exposures than obtainable by rockets can be made.

The simplest experiments can be performed by one of the astronauts, if an air lock is provided as planned. For example, a small extreme ultraviolet (XUV) photographic spectrograph can be introduced into the air lock, pointed at the Sun by the astronauts, and exposed for an hour or more, instead of two minutes, as in a rocket.

The Astronomical Telescope Orientation Mount (ATOM), as proposed by Ball Brothers Research Corporation in an engineering requirement study, is even more promising. This concept would utilize these extended periods in orbit for astronomical observations from a spar which could be erected from the service module of the Apollo spacecraft in flight. An astronaut would then act as an observer to point and guide the equipment.

The Working Group believes that in addition to the valuable solar data that can be obtained, an important aspect of the ATOM project would be the determination in a very practical way of the usefulness of man as a space solar astronomer. This should be accomplished before the completion of the design and construction of the large observing facilities proposed in the Committee's report.

A number of AOSO experiments, suitably modified, are already under study for inclusion in ATOM. An important additional experiment for ATOM would be a 50-cm-aperture photographic telescope for high-resolution cinematography in white light and Hα. Because the exposure times required are very short (about 10^{-3} sec), guiding would not limit the angular resolution, which could exceed that of Stratoscope by a factor of 2. Furthermore, the observing period would be much longer.

Other suggestions of experiments suitable for ATOM are contained in Section 3.

The Working Group made the following recommendations:

Recommendation 4. (a) That manned missions in the 1968-1972 time period, such as ATOM in the Apollo Extension Systems, are desirable to supplement AOSO, but cannot replace it;

(b) That because it offers the prospect of providing answers to critical questions relating to the technology of manned space telescopes and data recovery, the ATOM concept merits vigorous support.

Apollo Extension Systems (AES)

Flights of the Apollo spacecraft beyond the first lunar landing constitute the AES program. The Working Group considered in detail the AES concept, for it is apparent that such flights could provide great opportunities for solar space science in the period 1970-1975 and beyond.

Certain possible solar experiments for AES are contained in the reports of Section 3 and especially in the discussion of the Manned Orbiting Telescope, below.

The Working Group made the following recommendation:

Recommendation 5. That solar space observation be included in the manned space science program of the Apollo Extension Systems in order to develop the technology of manned space astronomical operations. Such observations, which could attain resolving power of 1 sec of arc in the wavelength region 500-3000 Å, mark the next logical step beyond both AOSO and ATOM.

Manned Orbiting Telescope (MOT)

(a) The problem considered first concerns the solar telescopes that should follow the AOSO program. The scientific needs in the 1970's will probably demand high angular resolution in all types of observation. Here we confine ourselves to the achievement of better resolution at wavelengths for which normal-incidence optics are effective, i.e., $\lambda > 500$ Å. The reasonable next step after AOSO should aim at an improvement in resolution at far ultraviolet wavelengths by a factor of about five, which represents an increase by a factor of 25 in the information detail in terms of elements of area examined. A similar relative gain in resolution at visible wavelengths should also be sought. Therefore, we shall discuss the possibility of obtaining resolution of 0.1 sec of arc at $\lambda > 3000$ Å and 1 sec of arc at $\lambda < 3000$ Å, or better. A further goal is the achievement of photometric accuracy of about 1%. The kinds of observations one can expect to make are summarized in Table 1, in Section 3.

We consider first the feasibility of one universal telescope for the whole spectral range $\lambda > 500$ Å. The aperture must be about 1 meter to attain the 0.1 sec of arc resolution in the visible region. Its focal ratio cannot be less than f/12, the maximum focal ratio now available or planned in concave gratings. The requirements dictate a telescope 1 meter in aperture and 12 meters long. The whole package would have to be about 15 meters long, since the XUV spectrographic equipment must be in line to avoid prohibitive extra reflections.

A more promising approach may be the use of two telescopes in a single package. The first would be a 20-cm f/12 XUV reflector of 2.4 meter focal length, with a mirror figured to the highest attainable accuracy. In the 500-1500 Å region, it would have a theoretical resolution of about 0.1-0.3 sec of arc, which exceeds our requirement of 1 sec of arc. The exposure required in a video system of 0.2 quantum efficiency would be in the 1-10 second range. Interchangeable mirrors for the various spectral regions could be provided. The second telescope would be a compound reflector of 1- or 1.5-meter aperture with an effective focal length of about

50-75 meters. A total folded length of about 5 meters would allow room for the in-line XUV spectrographic equipment. Conceivably, however, the whole instrument could be squeezed down to the 4-meter length which would fit into two compartments of the Apollo service module that may be available by 1970.

Manned operation of this telescope has many obvious advantages, including the recovery of photographic records and the kind of maintenance that leads to a long useful life (provided rendezvous can be contemplated), not to mention a considerable simplification of the original design and installation. Comparison between manned and unmanned operation in terms of cost of observational data can be determined only by a careful study.

(b) A white-light coronagraph is another space instrument of great importance. Such a coronagraph, which is conceived of here as externally occulted, would have a camera lens of approximately 10-cm aperture and a length of at least 10 meters. Appropriate auxiliary equipment, such as polarimetry instruments, must also be provided. Details of such a system are contained in the discussion of the wavelength range above 3000 Å, in Section 3.

(c) The telescopes proposed in (a) and (b), above, would fill an important interim need and would provide further valuable experience in the design, installation, and operation of manned solar instrumentation, probably during the AES period. The next step would be the relatively permanent space observatory discussed in Section 3 (under Wavelength Range above 3000 Å). As pointed out there, a 1- to 1.5-meter telescope could clearly surpass the performance of the best ground-based instruments in the visual region of the spectrum, and, if properly adapted, could be used over the entire electromagnetic spectrum above 1500 Å. Unlike the interim version, the system would not be folded and the telescope would be 50-75 meters in length. A 1-1.5 meter aperture is adequate for solar work because of the great intensity of light from the Sun compared to that from stars. We are here proposing not simply an orbiting telescope, but a fully equipped, and manned, orbiting solar laboratory. Such an orbiting solar space station would be at least semipermanent and would be periodically serviced from the ground.

(d) As a companion instrument to the telescope proposed above for wavelengths $\lambda > 1500$ Å, we require a second telescope of similar aperture, specifically designed to operate in the spectral range 500-1500 Å, as an integral part of the manned orbiting solar laboratory. The required spatial resolution is 0.5 sec of arc, but the pointing accuracy need not be better than a few seconds of arc.

Considerations of the above information and of the information contained below under "Man in Space" led the Working Group to make the following recommendations:

Recommendation 6. That feasibility and design studies begin immediately on orbiting solar telescopes of at least 1-meter aperture designed to obtain a resolution of 0.1 sec of arc at visible wavelengths and 0.5 sec of arc at far ultraviolet wavelengths ($\lambda > 500$ Å). Very large and complex accessory instruments will be necessary to analyze the solar image. Erection, operation, and maintenance of this telescope will require full utilization of astronaut-engineers and scientists.

Recommendation 7. That provision be made for a continuing uninterrupted experimental program while the more advanced manned flights are in preparation, with many flights of various spacecraft, so that a scientist will have frequent opportunities for observation.

Recommendation 8. That NASA find means to continue a strong program with relatively inexpensive rockets and small unmanned satellites at the same time the large manned projects are under way, since the former are indispensable to the latter.

Recommendation 9. That the relationship between scientists and astronaut-observers be studied and clarified. In particular, we recommend that when a single large scientific instrument is carried, the scientific observation be designated the primary mission for the flight.

Recommendation 10. That NASA bring more scientists into the space flight program as astronauts or observers.

MAN IN SPACE

AOSO will point telescopes up to 3 meters in length at the Sun with a precision of about 5 sec of arc. It is apparent from the discussion of specific vehicles and instruments, above, that solar astronomers need to achieve a resolution of 1 sec of arc, and eventually 0.1 sec of arc, in nearly every mode of observation planned for OSO and AOSO. Not only must telescopes be pointed with that precision, but more significantly, considerably longer telescopes are required, as well as corresponding refinement of component specifications, alignment, etc. Two consequences of this desired high-resolution performance of space telescopes are of special importance to the question of man's potential role. First, the data collection rate of large telescopes is vastly greater than that of small telescopes. Second, those telescopes and their accessories must be specialized as to functions, wavelength of operation, etc., so that versatility is achieved only by major subsystem interchange or adjustment. All of these factors must be considered as part of the question of man's usefulness in performing astronomical observations from a satellite.

So far as pointing and tracking a 1 or 0.1 sec of arc telescope is concerned, the physical presence of man is both a great advantage and a formidable handicap. Motion of an astronaut must be severely limited if he is tightly coupled to the telescope (i.e., not dynamically isolated, as, for example, by an umbilical cord or tether). At 0.1 sec of arc, even his breathing and involuntary muscular activity may be a major problem to the engineer developing an automatic stabilization system. Indeed, this one question has to be studied further before any commitment can be made to manual operation of a large space telescope. On the other side of the balance, an astronaut can make a notable contribution by performing manual guiding control, by monitoring a solar region for unpredictable activity, or by providing real-time assessment of instrument performance and quality of data acquired. To serve this useful purpose, the astronaut can still be essentially decoupled from the telescope, utilizing only a television monitor or some simple remote indicator to permit physical separation. In theory, this communication link could as well be relayed to the ground, so that an astronomer at his telescope could be included in the control loop. In practice, such wide-bandwidth, real-time, continuous channel operation requires a prohibitively costly and perilously unreliable ground communications network or relay satellite. With an astronaut present, such a control communications link can be provided through an umbilical cord, or in some instances by direct viewing.

As to the size of a telescope that can be carried into orbit, apertures are limited by diameters of launch vehicles, and over-all length by the accommodations of rocket nose cones. Resolving powers of a million for spectrographs and 1.0 or 0.1 sec of arc for telescopes require apertures of a few meters, which can be accommodated by modest vehicles like the Thor or Atlas. However, solar telescopes must operate at small aperture ratios of the order f/25 to f/100, for such reasons as thermal loading, diffraction-grating aperture, and the constraints of linear resolution of photographic films and image tubes. Therefore, solar telescopes for 1 to 0.1 sec of arc resolution would range up to several tens of meters in length. Such extended structures very likely will require assembly in orbit, after being launched in a stowed condition. For example, an enclosing tube, constructed in sections, might be literally telescoped and later extended in orbit. A mirror or grating probably should be packed in a special support to protect it from the shock and vibration of launch, with provision to install and adjust it as part of the assembly procedure. In conjecturing on such operations, the possibility of either fully automatic or remotely controlled operation must be considered. However, the astronaut himself is obviously well suited to perform such complex one-time operations in which very high reliability is mandatory, particularly in view of his ability to cope with unforeseen problems like fouling lines, sticking components, etc. Doubtlessly, his greatest asset is his ability to monitor and control, as in focusing and squaring on an objective, choosing a wavelength in a spectrum, etc.

Thus, it is likely that a man must be counted on to erect large solar telescopes and spectrographs in orbit. Beyond this initial operation of installing and commissioning a large instrument, a man must be relied upon to maintain and repair it. Large telescopes will be exceedingly costly to create, so that a useful lifetime of many years must be planned to fully amortize the investment. The finite lifetime of components (storage batteries and transistors, motor bearings, optical surfaces in the meteoroid environment, etc.) as well as the exhaustion of expendables (reaction motor fuel, for example) also demand periodic servicing of major orbiting observatories. For this task the versatility and flexibility of a man compared to a robot or automatic equipment will be critical in defining the feasibility of large space telescopes.

In the operation of large telescopes, man has several potential functions. First, he can perform major configuration changes—for example, converting a spectrograph into a spectroheliograph, altering the wavelength setting of a heliograph monochromator, interchanging gratings of different rulings, etc. During individual observational projects, man provides the ability to perform instantaneous analysis of the output data in order to modify the subsequent observations. An example would be to monitor an active region, and to start a series of high-rate spectral or cine observations at the inception of a flare. Automatic equipment to do the job accurately and reliably probably cannot compete with human judgment, since the complex activities of time correlation, field search and event localization, and very sensitive threshold judgments must be made simultaneously, and both accurately and quickly, under very poor conditions of low signal-to-noise ratio.

In connection with data collection, the 20-million-bit-per-orbit capacity of AOSO is recognized as a severe limitation on the performance of certain classes of observation in that spacecraft. Monochromatic imaging, coronal mapping, and spectral scanning in the normal-incidence region all permit data collection rates, even at 5 sec of arc angular resolution, in excess of the tape recorder capacity of AOSO. As a release from this constraint, photographic data recording appears highly desirable. Film permits parallel-channel recording over the equivalent of 10^4 to 10^6 channels simultaneously, and total data capacities of roughly 5 x 10^9 bits per 100-ft roll of 35-mm film. (For example, AOSO's tape recorders store 2 x 10^7 bits per orbit, with recording of the equivalent of only 10^2 - 103 parallel channels.) Recovery of photographic data by return of the astronaut thus appears attractive as one way of breaking through the data barrier. Alternative methods must be considered, however. One is on-board data processing—for example, transmission of a scene and then, for several frames, only time changes in the scene. Another suggestion is the return of a film package by a re-entry module from an unmanned satellite. For AOSO, it appears that a week's data could be recovered in this way from each mission, with about a 50% chance of recovery. This is clearly a very limited return payload. Other alternatives to film recovery with an astronaut are real-time video-bandwidth telemetry by relay to a high altitude communications relay satellite and use of an Earth-synchronous orbit.

It is clear that men can perform many useful functions in connection with the assembly and operation of large solar instruments in space. Perhaps other advantages of manned operation will appear as man gains experience in space work.

GROUND-BASED SOLAR ASTRONOMY

The study of the Sun is carried on in a variety of ways, and this variety will be increased and not diminished by the advent of sophisticated space techniques. The great range of wavelengths emitted by the solar atmosphere, the changes with time, the structural detail that can be observed, and the tremendous range of brightness encountered all force the solar astronomer to consider many techniques when attempting to solve a problem. Even in studying a particular problem at a particular time several complementary methods are frequently employed. For example, at the present moment solar flares are under observation in visible and radio wavelengths from the ground, in x-ray, ultraviolet and very long radio wavelengths from space, and occasionally in x rays from balloonborne equipment. As a different example, the electron corona of the Sun has recently been observed in the same wavelength but at a variety of distances from the edge of the Sun by photoelectric techniques from the ground, by a balloonborne coronagraph, and by a coronagraph aboard a satellite. These measurements overlap, but more importantly they complement each other to allow more rapid progress in understanding the Sun. And, to repeat, this situation will certainly continue.

Measurement of emissions from the Sun by ground-based equipment will continue for years to come to provide exciting progress in understanding the Sun. In addition, these measurements will be essential in planning space experiments and in providing vital information for the interpretation of the space measurements. The complementary roles of ground-based and space solar astronomy have already been emphasized earlier in the report. Therefore, it is the strong opinion of this Working Group that ground-based solar studies must be vigorously pursued, and that more extensive and elaborate solar ground-based astronomical facilities are required than those described in the recent study "Ground-Based Astronomy—A Ten Year Program," (National Academy of Sciences—National Research Council, 1964) commonly known as the Whitford Report.

Solar telescopes differ in an important way from the nighttime instruments. Most solar work has been done not on large universal instruments, but on more specialized telescopes designed for narrow ranges of problems. This point is illustrated by the fact that although the cost of the primary collecting surface (lens or mirror) for a nighttime instrument may represent up to one-third of the total investment, for solar instruments this optical element is a minor fraction of the total cost. Much more of the solar instrument consists of secondary optics, photoelectric guiders, filters, and other auxiliary equipment.

Because of this difference, a report on ground-based solar astronomy over the next ten years would not exactly parallel the Academy report, which lists the large universal instruments to be built. However, it seems very important that the genuine requirements of ground-based solar studies be described and the kinds and types of specialized facilities needed be outlined so that users of the existing Academy study may have a balanced picture of the needs of astronomy. This panel urges the Academy to arrange for an early formal report on the needs of solar astronomy over the next ten years.

The Working Group also discussed sources of support for solar observations from the ground. Like research in other fields, basic research in solar astronomy enjoys a variety of sources of support in this country today. This diversity is healthy and fully in keeping with the attitudes of the society in which we live. Now NASA has a special interest in ground-based work since the planning, support, and interpretation of the flight experiments in various spacecraft are tied closely to observations made on the ground. It is therefore reasonable to expect NASA to be one of the sources of support of a growing program of solar astronomy from the ground. The Working Group therefore recommends:

Recommendation 11. That NASA move to provide additional support for ground-based solar studies. As the flight program grows in sophistication and success during the next several years, the demands on ground-based work will also increase, and NASA should in turn anticipate an increased demand upon its resources for support of ground-based facilities and operations. In addition, in the next few years, NASA should expect, and respond favorably to, proposals for a few major ground-based solar installations.

LABORATORY RESEARCH

Active research in many fields of physics is essential to the program of solar physics research from space vehicles, to astronomical research in general, and to the entire space program. In spite of the fact that this is extremely obvious, it is sometimes overlooked. Actually a breakthrough in the laboratory may open new possibilities for solar physics that could not be realized, even with the most sophisticated and costly vehicles. An example is the 80% reflecting coating, $Al + MgF_2$, which now makes possible the use of complicated optical systems at $\lambda > 1100$ Å. This may be contrasted with the region $\lambda < 500$ Å, in which normal incidence reflectances are still very low, and only simple instruments can be used. Looked at another way, an increase in efficiency of an instrument by a factor of 10 may mean obtaining the same information in one-tenth the time in space or obtaining ten times more information during a particular flight. Indeed, at shorter wavelengths it is not now possible to study with sufficient time resolution the distinct development stages of a flare, either with spectrographs or with monochromatic cameras. Until more efficient optics are developed, the long exposure times required with such instruments preclude these critical observations.

In the following paragraphs the present status and needs for laboratory research directly connected with observational aspects of the solar physics program are summarized. Generally, the money that can be spent here is at least an order of magnitude less than that spent in vehicle development.

Diffraction Gratings for the Extreme Ultraviolet (XUV)

Research to improve diffraction gratings is being conducted by Bausch & Lomb, with NASA funds, and directed by NRL. Progress has been made in the use of the electron microscope technique to observe the form of grooves and the surface roughness within the groove, both too fine to see optically. It is expected that gratings with higher efficiency will become routine, rather than the exception. It is also believed that gratings ruled with as many as 4800 lines per mm will become possible; this will expand the potential of the normal-incidence XUV spectroheliograph. At the University of Michigan, a radically new type of grating ruling engine is under development, with good expectations of making possible the ruling of gratings of greater precision and up to 30-inch size. Such large gratings will be required to achieve resolutions of the order of 10^6 in future large satellite spectrographs.

Effects Produced by the Space Environment

The degradation of optical and photoelectric components in the high vacuum of space, produced by evaporation effects, by corpuscular and hard electromagnetic radiations, and by the impact of the residual atmosphere, merits much study on a systematic basis. For example, it has not yet been shown that replica diffraction gratings are satisfactory for use in orbiting spacecraft; therefore, use of original gratings is mandatory. Because the production of original gratings is severely limited in quantity, it is important to develop replicas that are safe for use in orbiting space vehicles. The production of several originals for OAO, for example, delayed grating production for many other purposes for months. Replicas offer also the great advantage of providing several gratings of exactly the same characteristics. Replicas should be perfected such that they can be used in all cases instead of originals. Long-delayed testing of replica gratings has recently been undertaken at Bausch & Lomb, and at the University of Colorado, to determine their stability in a simulated space environment.

Thin-Film Metallic Filters for the XUV

The thin $A\ell$ filter has made possible the simple XUV spectroheliograph for the range 170-800 Å, and has also made grazing-incidence spectrographs useful for recording the XUV solar spectrum by photography. Other such filters, passing different wavelength regions, are greatly needed, but difficult techniques are required for producing them. Beryllium, for example, transmitting at $\lambda > 110$ Å, would be extremely useful. But much research is needed to produce this film unsupported by a backing material. Elimination of pinholes is another problem, often of extreme difficulty. At present, some research is being conducted at NRL, at the University of Colorado, at the University of Southern California, and at the Northrop Space Laboratories.

Photographic Emulsions

To obtain photographic emulsions of high sensitivity in the extreme ultraviolet it is necessary to make use of the delicate Schumann-type emulsions. There is no other demand for this material; hence their production and improvement is entirely a prestige endeavor by the manufacturers. The value of Schumann emulsions for solar physics, on the other hand, is too great to measure; experiments and vehicles depending on this single component have already cost millions of dollars. Eastman Kodak first made available its SWR emulsion for rocket work some 20 years ago; their efforts in the next 10 years succeeded in largely freeing it from blemishes. Meanwhile, Roger Audran, of Kodak-Pathé, in France, made a real breakthrough, producing a Schumann-type emulsion 10 times more sensitive than SWR. This has made possible most of the advances in photographic solar XUV spectroscopy since about 1959. Gradually, Kodak-Pathé has improved the material by devoting a team of two or three very talented men to both research and production. This type of research should be supported and encouraged on an increased scale. There are many problems waiting to be worked on, in order to make this emulsion of even greater usefulness. It is essential to learn how to process it so as to make it as useful for intensity measurements as the more usual photographic emulsions. The proposed photographic instrumentation for major manned projects will very soon require specialized developments relating to photographic technology. Examples are cassettes to handle Schumann emulsions, protection of sensitive materials from space radiation, etc.

Photomultipliers

Photomultipliers constitute one of the critical elements in all orbiting solar physics experiments. It would be impossible to devote too much effort toward their improvement. There are many directions for research: improved quantum efficiency as a function of wavelength; fatigue effects; use in the photon-counting mode; polarization properties; noise and dark current suppression.

For XUV use, open photomultipliers of various types are required. For example, the Channel Photomultiplier of Bendix was used in the OSO-II spectroheliograph, but its performance in space has shown that much more needs to be learned about its characteristics. Arrays of these tiny photomultipliers offer great promise for image forming.

Video Systems

In lieu of photographic techniques, certain observations require video detection and wideband data systems. Improved video cameras sensitive to the UV, XUV, and x-ray regions of the spectrum and with long-time storage of images without deterioration should be developed. Improved image intensifiers, with better resolution and stable high gains, are needed. Along with these improvements, data compressive techniques should be investigated.

Research in Laboratory Spectroscopy

The XUV spectrum of the Sun has shown the existence of a great gap in our knowledge of the spectra of highly ionized atoms. For example, most of the observed solar lines from 171 to 500 Å have defied identification. A promising start on identification has been made in the United Kingdom, using the high-temperature plasmas (zeta- and theta-pinch) at Harwell and Culham, by introducing materials to study their spectra under conditions similar to those in the Sun's corona. Limited work is also going on at Los Alamos, NRL, and the High Altitude Observatory, and, with hot spark sources, in Sweden, Israel, and the USSR. It is important to assure the vigorous continuation of this work, persuading more spectroscopists to enter the field and using computer methods of analysis, until the energy level systems of the common solar elements are completely known in all stages of ionization encountered in the corona.

Another example of the important part played by laboratory spectroscopy is the study of molecular spectra under solar conditions, as conducted in the shock-tube laboratory of the Harvard College Observatory. Band heads in the XUV absorption spectrum of CO heated in shock tubes were found to match features in the solar spectrum from 2000 to 1500 Å, as recorded with rockets. This result established that CO is a strong contributor to the absorption in the critical region of the solar atmosphere between the photosphere and the chromosphere, where shock-wave mechanisms set in and transfer energy to produce the high temperatures in the corona.

There are many other topics in spectroscopy that deserve more attention; among them are: the rare-earth spectra, which appear in certain stars, and also with strangely great intensity just above the Sun's limb; transition probabilities; transitions of the autoionization type under conditions prevailing in the Sun; and live emission resulting from dielectronic recombination.

Technical Problems Associated With Space

Some major problems here are: lubrication of moving parts, deterioration of optical surfaces by evaporation or micrometeorite impact, radiation damage to optical and electronic components, fogging of photographic film, and electrical problems peculiar to the space environment, as encountered in OSO-II.

The solution of difficulties known to exist in certain space experiments is essential before the design of similar experiments for orbiting vehicles is frozen.

Miscellaneous

Ground equipment used in support of space experiments must not be neglected. For example, improvements are possible and are greatly needed in filters that permit monochromatic solar monitoring. Lyot birefringent filters are costly and fragile, but have proved to be powerful tools in the visible spectrum. Much progress has been made in Fabry-Perot type filters, so that a 2 Å bandpass filter is now available. Even narrower filters of this type are desirable, and merit aggressive development.

The infrared is a portion of the solar spectrum in which much remains to be learned. Rockets and satellites are not always required, since balloon altitudes are sufficient for many purposes. Detection problems on the other hand are very difficult. It is urged that breakthroughs in infrared technology be made available for use in solar-physics research at the earliest possible time.

Recommendations

In the light of the foregoing discussion, the Working Group makes the following recommendation:

Recommendation 12. That increased support be given to physical research in the laboratory, as required to develop improved space instrumentation for solar-physics research, to assist in the data reduction, and to make possible a full interpretation of the results.

3. COMPLETE SUBCOMMITTEE REPORTS

WAVELENGTH REGION BELOW 500 Å

The extreme ultraviolet and x-ray spectra ($\lambda < 500$ Å) are a particularly important part of solar radiation for observing and studying:

(1) Solar activity such as flares and other transient events.
(2) Active regions of the chromosphere and corona.
(3) The quiet corona against the disk.

In the extreme ultraviolet and x-ray spectral region, additional observations are required to learn more about the spectral lines present, and their identifications and intensities. Along with spectral data, spectroheliograms with low spatial resolution are needed not only to learn more about active regions and transient solar events but to gain information that will make it possible to optimize the design of future experiments. These requirements are natural tasks for rocket experiments and future OSO's.

As in other spectral regions, future requirements in wavelengths < 500 Å are for high spatial resolution. Two important steps appear in future resolution improvements. With the Advanced Orbiting Solar Observatory (AOSO), a resolution of 5 sec of arc will be attainable, which will permit resolution of the chromospheric network structure, general features of active regions, and flares. For the post-AOSO period, there is a need for higher resolution (1 sec of arc or better) to permit observations and study of individual spicules, granules, elements of the network structure, prominences, flare nuclei and microstructure of the corona. These short-lived features require high time resolution, which can be met only with TV systems having large data storage and wideband communication links or else by the use of photography with recovery of film from orbit.

Although the spatial resolution would be less than that of AOSO, for some observations the ATOM on Apollo could be used with photography to obtain high time resolution. In particular, broadband x-ray images of solar activity and coronagraph observations could be obtained with the ATOM.

The requirement for high spatial and time resolution goes beyond the capacity of either AOSO or ATOM and indicates the need for more advanced systems associated either with AES or with large observatories using man for servicing and film recovery. Telescope requirements also go beyond any now planned for AOSO, and consideration of possible designs point out the need for research and development in several areas. The instruments proposed for post-AOSO observations in the XUV and x-ray region of the spectrum consist of either normal or grazing-incidence primary optics coupled with normal or grazing-incidence grating spectrometers or crystal spectrometers. In the spectral region $500 > \lambda > 170$ Å, either a normal-incidence primary or a grazing-incidence primary with a grazing-incidence grating spectrometer have approximately the same over-all efficiency, the lower effective area of the grazing-incidence primary being balanced by the higher reflectivity. Neither of these systems produces stigmatic monochromatic images so that photoelectric detection in conjunction with a raster scan is required to build up images. This mode of operation limits the time resolution that can be obtained. To illustrate, the reflection efficiency for a normal-incidence primary is approximately 0.01 in this wavelength region, the grating efficiency in grazing incidence is about 0.10, and the detector efficiency using SrF as a cathode is 0.20, resulting in an over-all efficiency of 2×10^{-4} counts/sec per photon/cm^2/sec incident on the primary optics. For the Fe 170 Å line, the flux measured by OSO-I for the quiet Sun was approximately 2×10^8 photons/cm^2/sec. With a 1-meter primary, 65 counts/sec would be recorded on the average from 1 square sec on the Sun's disk. When about 10% of the Sun's disk is occupied by active regions, one can expect, according to OSO-I data, a 50% increase in the 170 Å flux. The 1-meter instrument in this case would record approximately 375 counts/sec for 1 square sec of the active region. These counting rates are somewhat typical of what will be encountered in the XUV and x-ray regions with state of the art systems. It is important that instrumentation be improved, and, in particular, that optical systems for forming stigmatic, monochromatic images be developed in order that high time resolution may be realized by photography.

The possibility of recording high-resolution spectroheliograms on film with the use of normal-incidence concave gratings should be explored. The NRL results from rocket measurements might be improved by large concave gratings with greater dispersion (> 4000 lines/mm) and better pointing. Broadband imaging for AOSO, however, is already available. As an example, with a 25-cm-diameter normal-incidence primary mirror and a filter transmitting the band 170-240 Å, one can possibly photograph the Sun in 1 sec with a flux of approximately 100 photons per sec of arc squared. At 44 to 60 Å, the situation with grazing-incidence optics is similar. Important observations of flare activity would also be possible with photographic techniques, larger primary optics and improved narrow-band filters.

Suggested observations in the XUV may be outlined as follows:

Flare Activity. Observations with the aim of providing a N_e, T_e model of flares, surges, prominences, etc.

a. Spectral lines. Lines of high ionization potential important with lower ionization stages needed to complete the structure. X-ray lines and continua should be emphasized. Time-history of spectrum at wavelengths shorter than 20 Å during active events is important.

b. Resolution. High spatial resolution with medium time resolution ($\leq$ 1 min) for studies of the history of chromosphere and corona at time of the flare (with spectral resolution $\lambda/\Delta\lambda$ 10^2). Lower spatial resolution with high time resolution ($\Delta t \leq 10$ sec) for detection and analysis of rapid events at flare time.

The subcommittee suggested that for bursts of hard x rays observations with high time resolution (< 1 sec) at $\lambda < 0.5$ Å might be combined with observations having high space resolution at $\lambda \sim 10$ Å, $\Delta\lambda \sim 5$ Å. The real need, of course, is for observations in x radiation (1-40 Å) with both high spatial and temporal resolution. It was also suggested that the possibility be studied of observing the polarization of x radiation, by use of grazing-incidence polarization.

Active Centers. Observations are needed upon which to base a model of the distribution of N_e and T_e in the active chromosphere and corona and to study variations of the model with time. This includes identification and analyses of small, high-temperature regions ("hot spots"). Time resolution should be adequate to study the development of active centers.

a. The most important spectral lines for this study are the Fe coronal lines in the interval 170-360 Å, which permit study of the variation in the degree of ionization from Fe IX to XVI. The chromospheric lines of O I-O V, which show the variation in chromospheric excitation, and the coronal lines of O VI, O VII, and O VIII are also extremely important. Because of its high ionization potential, the C VI line at 33 Å is also useful for identifying very hot regions. Spectral resolution of 10^3 and spatial resolution of 10 sec of arc are needed. Time resolution of minutes to hours is sufficient for the slow variations.

b. It is possible that the highest possible resolution will reveal slowly varying hot spots of scale < 1 sec of arc. Once that resolution is available, only stable pointing and longer integration times will be needed. Very high spectral resolution is desirable to resolve the profiles of a few lines, such as C VI at 33 Å.

c. For studies of active regions, only relative intensities are necessary, since our chief concern is with the changes in relative ionization.

d. It is especially important to study active regions in the poorly known interval $1 < \lambda < 20$ Å with $\Delta\lambda = 0.01$ Å, angular resolution of 5 sec of arc or better, and $\Delta t \sim 1$-150 sec. The energetic radiation in this region is particularly sensitive to very high temperatures. In ground-based studies, the maximum observable ionization potential is that of Ca XV (814 volts). Hence, we cannot say whether regions with temperature greater than 4,000,000°K exist.

Quiet Corona. Spectral and spectroheliographic studies of the same lines mentioned above under Active Centers will help to reveal the nature of the faint corona outside the active regions. It is of particular interest to connect the quiet corona structure with the chromospheric network and the spicule bushes, to determine the nature of the chromosphere-corona interface.

a. Important spectral lines for this study are the Fe series mentioned above, also Ne VII (465 Å) and Ne VIII (780 Å); Mg IX (368 Å) and Mg X (610 Å); and Si X (254 Å, 272 Å); Si XI (303 Å); Si XII (499, 521 Å). Comparisons of the intensities of the resonance lines with those of subordinate lines in the 100 Å region provide a good measure of the electron temperature. Departure of the resonance doublets of the Ne VIII-Mg X-Si XII sequence from the 2:1 ratio is a sensitive measure of optical depth.

b. Spectral resolution of about 10^3 is all that is needed, except for strongly blended lines. Resolution of 5 sec of arc will determine the gross correlation of the network structure with that of the corona, but 1-2 sec of arc is necessary to reveal the details of the interface.

Quiet Chromosphere. Most of the important lines are at longer wavelengths, but attention should be called to the lines He II 304 Å and 256 Å. We need spectral observations of the over-all and detailed distribution of these lines to determine the height variation of temperature and density of the chromosphere as well as the general structure. Low-noise observations are necessary to search for a possible uniform hot chromosphere over the center of the network cells in addition to the hot elements at the edges. Reasonable time resolution may be obtained by restricting observations to selected parts of the disk.

a. Spectral lines of interest include the He II 304 Å and 256 Å lines, the He II continuum, the C and O chromospheric ions, and isoelectronic sequences such as C II-Ne III-O IV. Spectral resolution of 10^3 or better is needed.

b. Spatial resolution of 5 sec of arc near the limb, with less near the center of the disk, will resolve the network; 1 sec of arc over an area of 30 square sec will permit study of the detailed dynamic structure.

c. Time resolution should be less than the chromospheric oscillation period (300 sec for 10 sec of arc resolution), but high spatial resolution (1 sec of arc) will also require higher time resolution (10 sec) because smaller chromospheric structures vary more rapidly than large ones.

The survey of the solar spectrum in the extreme ultraviolet is still far from complete. Only a small fraction of the lines has been identified, mostly because of the low resolving power of the spectrum. It is extremely important that a resolution of $(\lambda/\Delta\lambda) = 10^4$ be obtained in this region so that line identification may proceed. Large rocketborne spectrographs appear ideal for this purpose. At the same time, laboratory investigations must be encouraged. The value of this is shown by the recent success of the group at the Culham Laboratory of the UK, in identifying some of the lines in the 170-200 Å range.

There also is a large gap in the calibration of intensities in the extreme ultraviolet. Present intensity calibrations are reliable only as far as 304 Å, and both identifications and determinations of relative intensities of different lines are dependent on some sort of calibration; the same can be said of more-accurate wavelength standards in this region.

The need for laboratory research in instrument development should be reiterated. In particular, as is mentioned above, instruments with large aperture that produce stigmatic-monochromatic images of the Sun are not yet within the state of the art. Such an instrument is needed to make observations with high spatial resolution and high time resolution. For both the AOSO and post-AOSO eras, better reflecting surfaces and detectors are required. Improvements in these areas translate directly imto improved time resolution and in certain cases into improved spatial resolution. Improved narrow-band filters will increase the usefulness of instruments like the AOSO x-ray telescope.

WAVELENGTH RANGE 500-1500 Å

No Fraunhofer lines have been observed below 1525 Å; radiation observed at shorter wavelengths originates in the Sun's atmosphere above the temperature minimum. Thus, in this section, we are concerned with the chromosphere, the chromosphere-corona interface, and the corona, as well as with the structures that occur within these regions, such as plages, faculae, flares, coronal condensations, and prominences. We have set 500 Å as the lower wavelength limit only because below this, normal-incidence spectrographs become less useful than grazing incidence instruments—but otherwise the limit is rather arbitrary.

In answering the questions of solar physics, as listed in Section 1 under Current Solar Problems, there are several reasons why this wavelength range is of fundamental importance:

a. It contains the resonance lines of the most abundant elements and of the coronal ions. Much of our knowledge of physical conditions in the Sun's outer atmosphere must come from the interpretation of observed emissions that arise in regions that depart severely from thermodynamic equilibrium. This interpretation must then be made in the light of a sound theory of ionization, excitation, and line formation. The present vigorous attempt to develop such a theory will stagnate unless comparison can be made with observation. The most sensitive and clearcut tests of such a theory can be made in the XUV resonance lines. For this purpose, we require spectra with high resolution over the entire range.

b. We have at present no satisfactory understanding of energy balance in coronal and chromospheric structures nor of the mechanisms that heat the corona and drive the solar wind. Quantitative theories of energy supply to the corona, chromosphere, and flares must be compared with observed energy losses. Since most of the energy loss is by radiation in the resonance lines of the abundant ions, we need accurate absolute spectrophotometric observations in the XUV.

c. The fine structure of the photosphere and chromosphere can be photographed in the visible part of the spectrum with high resolution against the Sun's disk, but no such picture of coronal structure can be gotten from the Earth's surface. Under the very best observing conditions, some coronal structure can be observed at the limb with low contrast in the light of the forbidden coronal lines, but at the limb, simultaneous observation of the underlying chromosphere is not possible. Radio observations in the decimetric range show coronal condensations against the disk, but with very low resolution. However, permitted XUV lines of the coronal ions are bright against the disk and it should be possible to obtain high-resolution spectroheliograms of the inner corona in the light of these lines. Such observations could reveal the form and development of coronal condensations over centers of activity and would show how the corona differed above chromospheric structures such as spicules and the network.

d. We have at present no quantitative measures of magnetic fields in the corona, but, given sufficient spectral resolution and sensitivity, it should be possible to measure the Zeeman splitting of the permitted coronal lines. These observations require four basic types of instruments. These are: spectrographs (or spectrometers), which record the spectrum from some given region on the Sun; spectroheliographs (or monochromatic filters) to produce a picture of the Sun in the light of some specific radiation; monitors, which record a specific radiation from some given place on the Sun as a function of time; and magnetometers, which measure the Zeeman splitting of spectral lines. We consider these below, in order.

Spectrographic Observations

Spectra have been obtained over this entire wavelength range from rockets with spectral resolution of about 10,000. This resolution is sufficient for spectral surveys and also for line profile measurements of the strongest chromospheric lines such as Lyman-α. The next task is to obtain such surveys of the spectra of specific regions such as plages, coronal condensations, areas of the undisturbed disk, prominences, and flares. The photometric accuracy should be no less than 10% and the guiding accuracy 0.5 to 1 min of arc. Such observations could be made from rockets with a three-axis pointing control, or from an OSO-type spacecraft with command offset pointing.

With the higher pointing and guiding accuracy of the AOSO spacecraft (about 5 sec of arc) the same surveys should be made in smaller structures. Less averaging over various structures would occur and thus make higher photometric accuracy (say 3%) desirable. Line-profile measurements of emission lines from both chromospheric and coronal structures are of great importance. For coronal and prominence lines and for the weaker chromospheric lines, resolving powers of at least 100,000 are required. Profiles of all observable lines of an isoelectronic sequence (such as the lithium sequence) or of all ions of a given element (such as oxygen, for example) would be invaluable. With 5 sec of arc guiding accuracy, observations of the hydrogen and helium lines made from center to limb would become feasible. To make these observations in a reasonable time, a telescope of about 40-cm aperture would be required.

The interpretation of spectra is always made difficult by the fact that we observe the integrated emission over the line of sight rather than the intrinsic emission from a uniform element of the atmosphere. This is unavoidable, but if we could observe the variation of brightness across the smallest features in prominences, flares, or the corona, it would be possible to infer the intrinsic emission as a function of position and so deduce the actual conditions in the emitting regions. The resolution required for such measurements would be 0.3 to 0.5 sec of arc. This angular resolution would require a telescope of about 1-meter aperture if photometric observations of 1% accuracy are to be obtained in a reasonable time. Furthermore, the guiding accuracy required is beyond the capability of any presently planned vehicle.

Spectroheliographic Observations

The spectroheliograph enables one to look at the Sun (or a portion of the Sun) in a very narrow band of wavelengths. When proper filters are available, the effect of a spectroheliograph can be obtained simply by imaging the Sun through a narrow-band filter and observing the structure and time changes of the Sun as seen in the light of the chosen wavelengths. More versatile instruments can be constructed, however, around a spectrograph in which the desired wavelength band can be adjusted for the particular problem at hand.

Preliminary spectroheliograms have been made from rockets using a slitless spectrograph and depending upon the brightest emission lines to produce separate solar images in the focal plane. These photographs have shown that there is large-scale structure on the Sun in these wavelengths and have indicated the approximate light levels one has to deal with. Much better images are within the capabilities of the OSO spacecraft having a raster scan mode, and pictures with about 1 min of arc resolution can be obtained in a few minutes of time. Such pictures, if made in several selected lines (in particular, Lyman-α and the coronal lines of Mg X and

Si XII) should reveal the chromospheric and coronal response to the slow changes in active regions and should, in addition, give an impression of the center-to-limb variation in each of the selected lines.

Precise planning of an AOSO spectroheliograph should await the OSO results, but some general statements can be made now. From observations made in visible wavelengths, we know that chromospheric and coronal structures have detail smaller than 1 min of arc in size, so that the increased angular resolution available with the AOSO will show new aspects of the structure and behavior of the solar atmosphere. The AOSO spectroheliograms should show detail down to approximately 5 sec of arc and thus be similar to many of the flare-patrol pictures made in Hα. The great value of the AOSO picture over the ground-based observations, however, will be in the fact that they can be made in light arising high in the atmosphere, so that it should be possible to follow chromospheric structure into the corona.

Beyond the AOSO, plans are necessarily more tentative. We expect to find additional important fine structure in the solar atmosphere down to a fraction of 1 sec of arc, and knowledge of this structure is fundamental to understanding the processes governing the chromosphere and corona. Present thinking places the position of the initial energy release of a solar flare high in the corona, and our opportunity to observe whether this is indeed so will come with a spacecraft of the AOSO class or beyond. The instrument to observe the early stage of a flare must combine high time rate, rapid data handling, and sizable aperture. This can be seen to be a difficult assignment. At 700 Å, a useful emission line may yield a flux near the Earth of 250 photons/cm^2/sec from an area on the Sun of 0.25 sec of arc squared. If we must scan in only one dimension with a spectroheliograph and must cover an area 2 min of arc wide on the Sun, we will have 350 elements, or slit positions. Changes in active regions occur in less than a minute, so, setting our repetition rate at one picture every 10 seconds, we find that to reach an accuracy of a few percent we must have a telescope aperture of something more than a meter. Line profile work with a spectral resolution of 10^5 and 0.5 sec of arc spatial resolution places similar requirements on the space telescope.

Our goal in the 500-1500 A region, then, is a spacecraft with a pointing stability of 0.5 sec of arc or better, a pointing accuracy of a few sec of arc, and capable of carrying a telescope with an aperture of at least 1 meter. We will need both spectral and spectroheliographic observations, so that some versatility must be built into the instrument.

Radiation Monitors

In addition to spectrographic and spectroheliographic observations, detectors monitoring specific radiations are of great value in this spectral range. These detectors are needed, first of all, to monitor the radiation from the entire disk, especially for aeronomy. This should be done in Lyman-α and in broad wavelength regions throughout the entire wavelength range with an accuracy of 1%. But, more importantly, for solar physics one should monitor carefully chosen emissions from specific regions on the Sun as a function of time. Such observations would yield the rate and rate-of-change of energy loss from transient phenomena such as flares and sporadic condensations. These observations can of course be made with the same scanning spectrometer used for the spectrum survey by simply arranging to stop the scan at any wavelength or at certain selected wavelengths.

Magnetometers

The development of a magnetometer for use in this region of the spectrum will probably be difficult, but it would provide a direct way of measuring magnetic fields in the corona. The difficulties are low photon counts, small Zeeman splitting compared to the visible, and lack of suitable polarization analyzers. Such an instrument will probably have to await technical advances and larger spacecraft.

WAVELENGTH RANGE 1500-3000 Å

The wavelength region 3000-1500 Å divides itself naturally into two parts: 3000-2085 Å, which is the extension of the near ultraviolet and visible and is mainly photospheric; 2085-1500 Å (or 1200 Å), where the spectrum is completely different in character and comes, for the most part, from the transition layer and low chromosphere.

Spectral Mapping

One important task is spectral mapping with intensity precision and line detectability exceeding that obtained by the NRL echelle spectrograph, from 3000-2085 Å. These latter spectra become faint at the short wavelength end, and stray light, for which no satisfactory correction can be made, fills the cores of all the Fraunhofer lines. Lines fainter than -1 on the Rowland scale are scarce, and -3 lines are nearly completely absent. This task requires a conventional spectrograph using a concave grating at normal incidence, with about the resolving power provided by a 21-ft grating. Predispersion is required to produce a spectrum that is free from stray light. The goal is $\lambda/\Delta\lambda \sim 10^5$ or 2×10^5 and $\Delta I/I \sim 1\%$ over a spectral span of 10 to 100 Å, and scattered light $<1\%$ of the continuum intensity. Spectral mapping would be carried out for a region restricted to a diameter of less than 10 min of arc near the center of the Sun. This mapping must be done when the Sun is relatively quiet in order to obtain a definitive atlas for reasonably standard solar conditions. The same instrument would be used to continue the solar map to shorter wavelengths, at least to 1500 Å, and probably to about 1000 Å, so as to provide the increased resolution over the present value, 10^4, necessary to separate blends.

A satisfactory instrument of this type should be possible within a 5-ft length but this may require a sophisticated design. One min of arc pointing accuracy in yaw and pitch would suffice. Roll control is not needed, but is desirable when many active regions are present. Because of the size of this instrument, existing biaxial pointing control units are inadequate. A stabilized Aerobee-150 would be satisfactory, but the ACS system will not provide sufficiently accurate guidance. The United Kingdom (Elliott Brothers) control adapted for an Aerobee appears to be the quickest way to produce this vehicle.

Some of the results to be expected from this spectral mapping would be: separation of Fraunhofer from emission lines; detection of chemical elements not yet discovered on the Sun and the determination of their abundances; observation of molecular lines and thus the study of dissociation-ionization equilibrium, and measurement of molecular abundance; improved values for the abundance of poorly known elements from a study of the profiles of Fraunhofer lines more favorable than those hitherto used; profiles of chromospheric lines and, hence, data on the chromospheric model; and knowledge of the atmospheric structure above sunspots.

Spectrum of Selected Regions

Another problem is spectral mapping of specially selected regions on the Sun. For example, the change in the character of the spectrum from the center to a point beyond the limb could be studied. This requires a solar image a few centimeters in diameter, with predispersion. The basic instrument described in the previous Section with certain modifications would be satisfactory. For center-to-limb mapping, an area of the Sun about 5 sec of arc wide radially would be covered by the slit, which might be curved. The essential difference between this and the previous use of the instrument is that it would be flown in a rocket that provides 5-10 sec of arc pointing accuracy and comparable stabilization in roll.

The basic instrument could be modified to accept light from a small area of the Sun and thus to study spot spectra or prominence spectra. By placing the slit beyond the limb, chromospheric and coronal emission lines could be detected. This might require, however, the use of an external occulting disk to reduce the stray light from the disk as a whole.

An Aerobee-150 would probably provide enough space for this instrument using folded optics. It is certain, however, that a rocket with greater payload and space potential is highly desirable. Physical recovery by parachute of the instrumentation and photographic film is necessary and this rules in favor of a rocket rather than a satellite. For use in rockets, the full capability of this instrument requires programmable offset pointing to 5 sec of arc accuracy.

This instrument should also be considered for flight in an early AES using a stabilizing system of the type proposed by Ball Brothers (ATOM). Observations with this instrument would be an excellent task for a scientist-astronaut. He might, for example, secure a time sequence of high-resolution flare spectra by pointing the instrument at the flare as soon as it was detected by other means. Spot spectra and prominence spectra could also be obtained by manned pointing of the instrument.

A high-resolution solar spectrum recorded from disk center to beyond the limb would be of great value in perfecting the photospheric model and extending the model across the transition region. It would also allow the study of possible abundance anomalies. The spectrum of active regions could be followed from beyond the limb in the low chromosphere across the transition region and into the photosphere to determine how the structure of normal chromospheric features and active regions is modified with increasing depth in the atmosphere. Certain lines in this region are also formed in the chromosphere and even up to the chromosphere-corona interface, and information about these lines would help in a better understanding of these regions.

Line Profiles

The third and long-range phase is the study of certain selected lines with still greater spectral resolving power ($\lambda/\Delta\lambda \sim 10^6$), greater spatial resolving power (0.1 sec of arc), greater sensitivity to small variations in intensity ($\Delta I/I \sim 0.1\%$), and greater time-resolution (so as to provide cinematographic presentation). Not all of these requirements would be met in a single experiment; in fact, many combinations are envisioned. Various types of instruments would have to be developed as no single instrument could accomplish all these objectives.

Some of the most obvious lines for early study are Si III (1892 Å); Si II (1817, 1808 Å); C I (1657 Å); He II (1640 Å); C IV (1548, 1551 Å); and Si IV (1340, 1403 Å).

Preliminary work could be carried out in a very large stabilized rocket providing recovery of the photographic film (perhaps a triaxially stabilized Aerobee-350 would be sufficient).

As larger and larger orbiting telescopes become available, the resolution in wavelength, intensity, angle, and time can be improved, but the completion of the full task will be one of the goals of a completely equipped manned orbiting laboratory with a telescope of at least 1-meter aperture.

The results of such observations would add greatly to our understanding of how granulations disappear, turbulent effects arise, spicules enter, and active regions are formed as one looks higher and higher in the solar atmosphere. Another aspect of the investigation would be the time variation of certain features of these line profiles, which are formed in the chromosphere and low corona. These lines should be observed and analyzed in a manner analogous to that employed with the wiggly line spectra from the visible part of the spectrum. An additional purpose for this observation would be to study the structure of the chromosphere and chromosphere-corona interface.

Monitoring the Total Solar Flux at Low Spectral Resolution

The flux from the entire Sun should be monitored at low spectral resolution over the wavelength range 3000-1200 Å, with an accuracy in intensity measurement such as to detect variations of less than 1% over long periods of time. The absolute accuracy need be no better than 5%. The spectrum would be monitored in sections of 10 to 100 Å in length, and these measurements should be carried on over an entire solar cycle.

These data are of interest both to aeronomy and to solar physics. For the benefit of aeronomy, for example, a band centered at 2550 Å, the peak of the ozone absorption, should be monitored with approximately a 10 Å bandwidth. For solar physics, the continuum just short of 2085 Å should be monitored, since here the radiation comes principally from the transition region between photosphere and chromosphere. Another region of the continuum from 1520 Å to about 1475 Å should also be monitored as a large fraction of the continuum in this range is produced by the recapture spectrum of Si II and originates in the chromosphere.

Such monitoring experiments must be carried on from an instrument that is kept pointed at the Sun in pitch and yaw but need not be roll-stabilized. A suitable vehicle is OSO. However, it would be highly advantageous to place OSO in a polar orbit so as to obtain continuous monitoring of the Sun. Alternatives are a solar Explorer or an Orbiting Geophysical Observatory (OGO). The high pointing accuracy of AOSO is not required, though this measurement should be considered as a suitable complement to first-priority AOSO experiments.

Spectroheliogram Studies

A huge number of useful spectroheliographic observations—ranging from the very simple to the most sophisticated, from the one-shot type to the monitoring kind with cinematographic display—can easily be conceived.

The vehicles required, likewise, range from the present biaxial-pointing-control Aerobee rocket to the largest imaginable Orbiting Solar Observatory. A few of the most obvious types that are of interest are noted below:

a. Lyman-α spectroheliograms with a spatial resolution of 1 sec of arc recorded in the light of the whole line every 10 sec; also, spectroheliograms at 5 sec of arc spatial resolution monitored every 10 to 30 sec, but with velocity discrimination produced by selecting different parts of the line. Experiments of this type are being constructed for AOSO by the Harvard College Observatory and by NRL. If these experiments show new features, they should be carefully monitored over long periods of time. Finally, it may prove desirable to obtain 0.1 sec of arc spatial resolution, which could only be done with a large orbiting telescope.

b. Various emission lines—for example, He II (1640 Å); C I (1657 Å); C IV (1548, 1551 Å); Si II (1817, 1808 Å); Si III (1892 Å); and Si IV (1340, 1403 Å).

c. The autoionization lines of aluminum at 1935 Å.

d. Selected spectral regions that are largely continuous radiation, but in which the radiation comes from various heights in the solar atmosphere. Some examples are: radiation at about 2700 Å, which comes from the visible photosphere; radiation at 1950-2050 Å, which comes from the very outer layer of the photosphere; radiation from the capture continuum of Si II in the range 1490-1525 Å, which originates in the low chromosphere; and radiation in the range 1275-1375 Å with the emission lines excluded by means of a masking arrangement that would permit detection of only the continuum and the very faint emission lines (there is reason to believe that this radiation may originate in the photosphere or in the transition region).

e. 1715 ±3 Å to map the Sun in the molecule CO. This would perhaps greatly advance our understanding of sunspots and the surrounding chromosphere.

It is obvious that experiments of these types can be carried out in the solar-pointed triaxially stabilized Aerobee-150, especially after its pointing accuracy is perfected to 5 sec of arc. Experiments can also be designed to advantage for AES, for in this program, the astronaut could activate the equipment during a solar flare so as to record changes taking place in the particular wavelength range being studied and of the type observed by Moreton in H-α. Equipment of this sort would certainly be incorporated in any large manned orbiting observatory.

Magnetometry

The Zeeman splittings of the spectral lines originating in the chromosphere and lower corona should be investigated to ascertain whether use of these lines to study the magnetic fields from these regions is feasible.

Since the region of the spectrum with $\lambda > 3000$ Å is the domain of ground observations by well-equipped observatories all over the world, the only purpose of space observation here is to surpass the atmosphere-limited ground capabilities quite emphatically. This means at the outset that the observing equipment in space must generally have larger apertures than the largest useful apertures of ground-based instruments.

We list below some of the features of the Sun for which the spatial resolution of a space telescope (0.1 sec of arc, say) would mean a significant advance in knowledge crucial to the basic current problems, discussed in Section 1.

1. Broadband Features

a. Granulation in white light or broad bands. Determine contrast, evolution, shapes, size distribution, possible ordered arrangements, systematic changes in and near active centers.

b. Sunspot structure. Determine photometric characteristics and evolution of penumbral filaments, umbral granulation, and the foreshortening effects near the limb.

c. Faculae. Structure and evolution.

2. Spectroheliograms and sharp-band-filter observation

a. Structural details and their evolution in the plages, active center whorls, spicules at the limb and on the disk, prominences, and active coronal regions.

b. The still unresolved pebbly looking background seen in the best H and K line spectroheliograms and large-scale filtergrams.

c. "Moustaches" or "points" (which are comparable in size to granules).

d. Photospheric structures at different levels shown in spectroheliograms in medium and weak Fraunhofer lines.

e. Velocity and magnetic spectroheliograms.

3. Spectrographic observations at $\lambda < 1$ micron

a. Line profiles of very small features (granules vs. intergranular spaces, spicules on the disk and at the limb, prominence strands, flare details, etc.).

b. Distribution of velocities in the line of sight (shown as wiggly lines) to the highest resolvable spatial frequencies.

c. Point-to-point variation of flare and prominence spectra over the whole observable wavelength region (with a universal spectrograph of moderate dispersion).

The problem in observing the corona at large R is not one of resolution, but of freedom from scattered light originating in the Sun. Coronal brightness starts near the limb at about 10^{-6} times that of the photosphere and decreases steadily outward to the level of the zodiacal light, about 10^{-10} times the photospheric brightness. Externally occulted coronagraphs in space could work effectively at these levels. On a continuous basis, observations of the white corona are perhaps our only hope for seeing directly the outward motions of the plasma clouds detected near the Earth and relating them to the various types of radio bursts. Such observations are very important.

In the infrared, the present need is for any information whatever at wavelengths between 3 and 300 microns. The receivers and graded filters for the determination of the energy distribution with a spectral resolution of about 10 are available. The observations could best be made from balloon or airplane altitudes and do not require the continuity of satellite vehicles. The satellite should be used, however, to study the infrared characteristics of large sunspots and plages. (The resolution of the 1- to 1.5-meter telescope proposed below would be just about sufficient at these wavelengths.) Until we have this kind of start, it is difficult to predict the next logical steps.

Because the variety of possible significant observations is very large, we believe that a solar space observatory should be a versatile multipurpose instrument, basically similar to a well-equipped ground observatory, prepared to perform as many as possible of the different functions we can foresee, as required. A most important feature of this concept is that once a really versatile observatory is in orbit, it is a ready facility capable of quick changeover to deal with different observational problems. The lead-time for a given experiment could be reduced from years to weeks. If an experiment fails it is not a catastrophe—little more than some observing time is lost. Such a capability would virtually eliminate the most serious deterrents to potential space experimenters in solar research, and we could expect a very large increase in the participation in the solar space program.

We feel that spatial resolution in the visible spectrum better than 0.1 sec of arc, guiding on any point of the Sun accurate to 0.05 sec of arc, and acceptable light efficiency from 3000 Å to about 1 mm, are the minimum requirements to achieve the significant gains over ground-based performance that would justify a space solar observatory. These requirements automatically specify a reflecting telescope of 1- to 1.5-meter aperture. An acceptable image scale, with the $\lesssim 0.1$ sec of arc resolution element exceeding 25 microns in the solar image, requires an equivalent focal ratio $\lesssim f/50$. If the reflecting optics were coated with aluminum, the range of the telescope would actually extend down to about 1500 Å, which increases its utility very considerably. The telescope would have to be a very long structure (50 to 75 meters), which might conceivably be assembled in space or, less desirably, a shorter compound system with its more severe thermal problems. As proposed in Section 3, under Wavelength Range 500-1500 Å, Radiation Monitors, this telescope would be supplemented by one of similar aperture designed to operate in the spectral range λ 500-1500 Å, but with lesser spatial resolution of 0.5 sec of arc.

The telescopes would be designed to feed any of a number of attached accessories. They include:

a. The most powerful stigmatic spectrograph possible, with facilities for spectrum scanning in a double-pass arrangement and for photography.

b. A large spectroheliograph, adjustable to any desired wavelength, with adjustable band pass down to about 0.02 Å.

c. A stigmatic universal spectrograph for recording a long section of the spectrum in a single exposure.

d. A stigmatic infrared spectrograph (the characteristics of which will be less obscure when balloon experiments give us more information).

e. A camera for direct white-light or broad-band-filter photography.

f. A similar camera with appropriate scanning equipment for the infrared.

g. Other fundamental accessories that solar astronomers will doubtless propose.

A second important, but far simpler, instrument would be an externally occulted white-light coronagraph. (An internally occulted coronagraph is of no use in space; since no one has succeeded in reducing the scattered light to less than about $5 \times 10^{-6}\ I_{\odot}$ (intensity of the disk) or 5 times the brightness of the inner corona, it works nearly as well from the ground.) The bulky part of this instrument is a simple ≥ 10-meter boom that positions the occulting disk with respect to the relatively compact camera. The geometry is such that a zone of the lower corona is vignetted, since the occulting disk and corona cannot simultaneously be in focus. The vignetted zone is reduced by reducing the camera aperture (which increases the depth of focus) or by lengthening the boom (which moves the occulting disk toward optical infinity). Since the corona varies in intensity from $10^{-6}\ I_{\odot}$ at $R = 1R_{\odot}$ (solar radius) to about $10^{-10}\ I_{\odot}$ at R about 20 $R_{\odot}$, two or three coronagraphs on a common boom would be required to achieve the necessary dynamic range. The bright inner corona would be observed with a long camera of 0.5- to 1.0-cm aperture with a very narrow vignetted zone, while a short 10- or 15-cm aperture camera with a broad viginetted zone could reach the outer corona.

If the space telescope is to be 50 or 75 meters long, it would serve as a most admirable coronagraph boom, although some provision would be necessary to resolve occasional conflicts in pointing between the telescope (which may be offset or scanning) and the coronagraph (always pointed at the center of the Sun). The corona would be accessible from about $R = 1.1\ R_{\odot}$ on out. The shorter compound telescope would be less favorable, but could carry an attached 10-meter boom, with which the corona would be accessible from $R = 1.3\ R_{\odot}$ out with longer exposures.

The often suggested possibility of an independent orbiting occulting disk at a great distance from the telescope is very attractive optically, but may present difficult problems of control; however, such a system may be possible within a finite number of years. If so, the problem of attaining a sufficiently large ℓ is replaced by that of using a sufficiently large occulting disk. If we consider a diameter of 5 meters reasonable, $\ell \approx$ 500 meters. With a guiding accuracy of $\Delta\theta = 10^{-3}$rad, we find that the corona is observable at $R > 1.25\ R_\odot$ with a 15-cm aperture capable of resolving 1 sec of arc and fast enough to detect the outer corona.

The telescope, its accessories, and the coronagraph constitute a fairly complete observatory, capable of dealing with most of the problems that are beyond the reach of ground-based observation. As described above, this is at best a very complicated array of instruments, the effectiveness of which will depend on the precision of mutual adjustment of individual components and of the telescope to its accessories. Furthermore, a long useful life is an important consideration for a project of this magnitude. It is clear that men in space, who could assemble things in place and perform periodic maintenance and servicing would enormously simplify the initial design of the system and prolong its operating lifetime. We then have the possibility of minor modifications (like replacement of gratings with better new ones, as the ruling art improves), attachment of new small accessories (a video tube of better discrimination, or an image dissector, for instance), the replenishment of expendables, alterations in setup for the next series of observations, and recovery of data films and tapes. Although continuous observations throughout the life of the system require ground-controlled unmanned operation as the normal, a man might be able to make some special observations during routine visits.

While this is an exceedingly attractive picture, we should consider the alternatives that are open if manned maintenance and servicing turn out to be impractical. A most important function of the man would then be lost, namely, the recovery of photographic film. Various photographic emulsions are capable of recording and storing from 10^6 to 10^9 bits per cm^2 simultaneously, and the area that can be used is practically unlimited. For unmanned observations we must either plan on recovery of film canisters (or the whole observatory) or find a substitute for photographic film that is within the communication capacity of the vehicle. Single-point scanning is clearly impossible. For example, a coronagraph of 1 min of arc resolution working to 20° elongation can record a picture photographically in about 1 min, while the single-point recording of the same data would require a day. A spectroheliogram that takes a minute to record with 0.1 sec of arc resolution would require 9 hours to record point by point over a 10-square min of arc area. However, image storage and wide-band video recording are a likely substitute for photography, and the recovery of film canisters might be substituted for manned operation in space. A third possibility is on-board development of film followed by broad-band video transmission, a system used in the lunar orbiter. The independent development of video methods seems very likely, and the recovery of canisters would appear to be less difficult than safe manned operations in space. We should regard these alternatives as serious

Table 1. Characteristics of Various Observing Techniques

	System	Potential Lifetime	Spatial Resolution*	Continuity of Observation	Wavelength Region Accessible	Recording† Method
I	Ground-based telescope	∞	Limited to 1 sec of arc by seeing, regardless of aperture	Determined by weather, day and night station network density and distribution	3500 Å- 1 micron plus windows	All
II	Balloons or high-flying aircraft	1 day per flight	Limited to possibly 0.15-0.2 sec of arc by seeing	Costly for continuous observation	3000 Å- 1 cm	All
III	Nonrecovered satellite	About 1 year	Diffraction-limited: < 0.1" for 50" aperture	Determined by telemetering capability**	All < 1 km	Video transmission of photographs or real time in cryogenic bolometers
IV	Recoverable satellite	Determined by equipment life and film supply or data system	Diffraction-limited: < 0.1" for 50" aperture	Almost continuous, depending on orbit***	All < 1 km	All except bolometer (films and tape recovered)
V	40" to 50" telescope in orbit and serviced by men	1 to 10 years	Diffraction-limited: < 0.1" for 50" aperture	Almost continuous depending on orbit***	All < 1 km	All

*Resolution is at λ5000 Å (see Figure 1 for resolution limits).
**Includes the problem of station network density, bandwidth of the telemetering, and possibly of wideband transmission to one station real time from a synchronous orbit or by the use of a relay satellite.
***For an OSO type orbit two-thirds of the time is on sunlight.
†All means + video + photomultiplier + He.

competitors with manned operations, and see what losses to the program could be expected. Table 1 summarizes the situation as it looks now.

It is evident that unmanned balloons and aircraft at 70,000 ft and recoverable satellites could perform very well, but with a more limited repertoire than the manned station. All these methods of observation, however, must have the same telescope aperture as the manned operation for use in the visible spectrum. The complexity of the unmanned satellites would be much greater than that of the manned station, their life expectancy considerably less, and their reliability very much less. Balloons or aircraft platforms appear to be the equal of the manned station for programs that can be accomplished by one-day flights if sufficient guiding accuracy can be had, and would probably be preferable for most of the infrared work.

Obviously, such a man-maintained space observatory would be very attractive to a solar astronomer. A great deal can be done with any of the systems, however, provided the limited life for such a large piece of equipment is acceptable.

MANNED ORBITING TELESCOPE (MOT)

In this section are collected certain technical considerations relating to the collection, storage, and transmission of data by a telescope of very

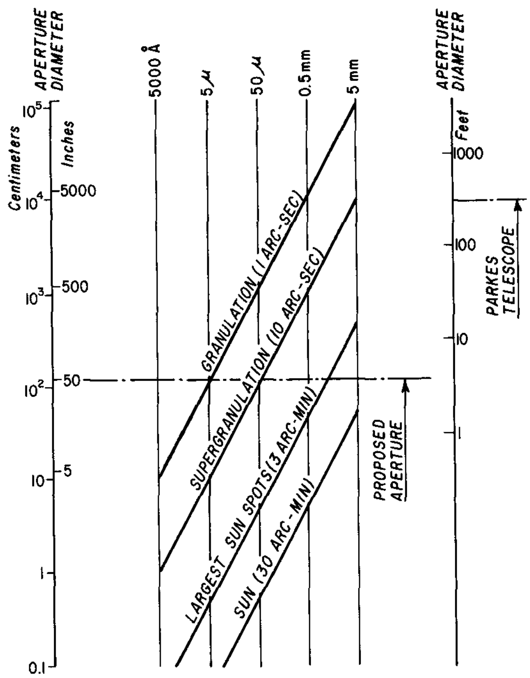

Figure 1. Resolution limits of instruments of various apertures, operating at wavelengths between 5000 Å and 5 mm.

high resolving power. Figure 1 shows the resolution limits of instruments of various apertures operating at wavelengths between 5000 Å and 5 mm as these limits relate to solar features of special interest.

In light of Table 2, we examine the feasibility of current scanning methods, in which the elements of the solar surface or the spectrum are examined individually in succession, or with point-by-point scanning. The photons from a single element enter the telescope aperture and are converted into counts of some sort. The required photometric accuracy of 1% leads to a required signal-to-noise ratio of 100, which is achieved by a count of 10^4. The total point-by-point scanning time, T, required to cover

Table 2. Information Obtained by a Large Solar Telescope

Mode of observation	n	Counts ϵn	Δt(sec)	N	T (=N Δt)
XUV spectroheliograms	$\sim 10^7$	4×10^4	0.25	3×10^4	10^4
XUV spatially resolved spectra	$\sim 10^7$	$4 \times 10^4 \delta\lambda$	$0.25/\delta\lambda$	180k	$50k/\delta\lambda$ (5×10^5)
$\lambda > 1500$: broad-band photographs	10^{11}	10^{10}	10^{-6}	3×10^6	3.0
$\lambda > 1500$: spectroheliograms	$10^8 \Delta\lambda$	$10^7 \Delta\lambda$	$10^{-3}/\Delta\lambda$	3×10^6	$3 \times 10^3/\Delta\lambda$($3 \times 10^4$)
$\lambda > 1500$: spatially resolved spectra	$10^8 \delta\lambda$	$10^7 \delta\lambda$	$10^{-3}/\delta\lambda$	1800k	$1.8k/\delta\lambda$(2×10^4)

n = number of photons incident on 1-meter aperture per second from one element of area on the solar surface with a resolution of 1 square sec of arc in the XUV and 0.01 square sec of arc in the visible.

ϵn = number of counts per second resulting from n photons.

Δt = time required to accumulate 10^4 counts at a rate of ϵn/sec.

N = number of resolved elements in scanned area of 10 square min of arc.

T = time required for a point-by-point scan of N elements.

$\delta\lambda$ = wavelength resolution (bandwidth) of a spectrum scan in angstroms. A value of $\delta\lambda$ = 0.01 Å would be about right.

$\Delta\lambda$ = bandwidth in the visible spectroheliograms, perhaps 0.1 Å.

k = $(\lambda_1 - \lambda_2)/\delta\lambda$, where λ_1 and λ_2 are the limits of a spectrum scan, which might be 10^3.

10 square min of arc of the solar surface is calculated very roughly and listed in the last column of Table 2. Although the calculated T's could easily be wrong by a factor of 10, it is quite evident that the point-by-point scanning times are far too long to permit reasonable time resolution (1 min of arc, say), and it will be necessary to do our recording either by photography or by a video receiver. The video receiver can theoretically record all elements of the observed area simultaneously, and reduce the time required by a factor of about 10^4 in the XUV and 10^6 in the visible. Photography should be about 20 times slower. Either method of recording largely removes the quantum limitations of the system.

4. REVIEW OF RESULTS OF SOLAR SPACE RESEARCH

A great deal is now known about the solar spectrum, from the atmospheric cutoff, about 3000 Å, through the extreme ultraviolet (XUV), and into the x rays. Generally, the shorter the wavelength, the more there is that remains to be done. A start has been made on spectroheliographic work in the XUV. Intensity measurements are far from being sufficiently exact. Monitoring work has been carried out in Lyman-α, in XUV spectra, and in various broad x-ray bands. Most of the results have been obtained with the Aerobee-150 vehicle and biaxial pointing control (BPC). Results of the monitoring type have been secured with OSO-I and OSO-II, and with the small Solrad satellites.

However, the United States has neglected much-needed vehicle developments for solar work. The only new vehicle for solar research to appear on the scene since OSO-I (1962) is the U.K. stabilized recoverable Skylark rocket. This promises to be of great importance for solar physics. The first three flights (of the prototype device) were successful, providing pointing in yaw and pitch to better than 10 min of arc, and comparable roll stabilization. The next units are expected by Elliott Brothers to point with less than 10 sec of arc noise in yaw and pitch, and $<1^\circ$ noise in roll. This rocket has already made it possible for the United Kingdom to secure XUV spectra of the chromosphere and corona separated from the photosphere, from 1000-3000 Å. For this purpose the spectrograph was equipped with a mirror, servo-pointed to ±2 sec of arc, which kept the Sun's limb positioned at 10 sec of arc from the slit.

In the following sections the present status of solar XUV accomplishments will be summarized in more detail, topic by topic; needs for future progress are included. Tables 3a and 3b present a summary analysis of the spectrum as known at present.

Table 3a.

λ(Å)	Instrument	Wavelength Precision (Å)	Resolving Power (λ/Δλ)	Lines and Identification
3000-2085	NRL echelle	0.01-0.02	10^5	≈ 1/2 of 7000
3000-1000	UK, 10" above limb	~ 0.1	~ 5×10^3	new coronal lines
2000-1200	NRL crossed-dispersion	0.05	10^4	≈ 1/3 of 1000
1200-500	NRL crossed-dispersion	0.03	10^4	most of 200
1200-250	AFCRL photoelectric	~ 1	~ 5×10^2	
500-149	NRL photographic	0.03-0.1	$1\text{-}2 \times 10^3$	1/5 of 250
300-149	AFCRL photoelectric	0.1-0.2	~ 5×10^2	
340-170	NASA OSO-I photoelectric	~ 0.3	~ 2×10^2	
149-55	AFCRL photoelectric	~ 0.2	~ 10^2	
80-33	NRL photographic	~ 0.05	2×10^2	most of 60
25-13	NRL Bragg spectrometer	~ 0.1	~ 10^2	13 of 14

Table 3b.

λ(Å)	±ΔI/I (rel. reλ)	±ΔI/I, abs.	Notes	Needs
3000-2085	≈ 10%	≈ 20%	Stray light fills cores	Large grating spectrograph, center-to-limb precise pointing
3000-1000				
2000-1200	≈ 15%	≈ 30%	Semiquantitative center-to-limb	Large grating spectrograph, improved center-to-limb precise pointing
1200-500	≈ 30%	-	Semiquantitative center-to-limb	Large grating; increase reflectance, improved center-to-limb precise pointing
1200-250	10-25%	10-25%		Higher altitudes
500-149	≈ 30%	≈ 50%	A few spectroheliograms	Larger spectrograph, higher altitudes, more exposure, observations during active Sun
300-149	≈ 50%	x or ÷ 2		Higher altitudes, solar activity
340-170	≈ 30%	-	Time studies	Greater wavelength coverage
149-55	≈100%	x or ÷ 4	Signal too low	Higher altitudes, increased solar activit
80-33	≈ 50%	x or ÷ 2		Larger spectrograph, greater exposure, and solar activity
25-13	≈ 25%	≈ 25%	Contrast of plages vs. disk	Larger spectrometer, more time

MAPPING THE SPECTRUM

Here the emphasis is on precise wavelength measurements, recording as many lines as possible, and identifying them. So far, this work has been done with small grating instruments, shorter than about 2 ft in length, and with photographic recording for the most part. Attainments are listed in Tables 3a and 3b.

From 3000-2085 Å, the NRL echelle has produced spectra of the central region of the Sun, with a wavelength precision and resolving power that are sufficient for most purposes, except at $\lambda < 2200$ Å, where the intensity becomes low. The spectra suffer, however, from stray light inherent in the echelle, which limits severely the detection of lines of intensity -2 and -3, and fills in the cores of Fraunhofer lines. Although the dispersion is high, the spectra are narrow; hence intensity discrimination is limited by noise from granularity. The need in this photographic range is for a large grating spectrograph that is equipped with predispersion, and for freedom from stray light. A second need is for measurements of the change in spectrum from center to limb. For this purpose a well-stabilized Aerobee-150 is essential for further progress.

As mentioned earlier, the U.K. stabilized Skylark has already flown successfully and resulted in obtaining low-dispersion spectra just above the limb. A number of new coronal emission lines were discovered in the wavelength range 2000-3000 Å in the recent flight in April 1965. This vehicle appears to have a potential ability to meet some of the needs for future solar research.

From 2000-1200 Å, the spectrum has been well recorded with the NRL crossed-dispersion spectrograph. The wavelength precision and resolution can be improved, with the existing instrument, by a factor of 2, to about 0.02 Å and 2×10^5, respectively. Center-to-limb data are obtained, but are qualitative for lines, because of the narrowness of the spectra, and are limited to 1 min of arc spatial resolution at present. The need for future research is for a larger stigmatic grating spectrograph, similar to that discussed in the paragraphs above for the 3000-3085 Å region. Remarks similar to those in the same paragraphs apply also to the vehicle, as well as to the results obtained by United Kingdom investigators. Guidance to 5-10 sec of arc is essential.

The range 1200-500 Å has been covered with the NRL crossed-dispersion spectrograph, with just about all the wavelength precision and resolution it can produce. This instrument is intensity-limited below 1000 Å, however, and somewhat longer exposures would probably reveal ten times more lines. This range has also been covered with photoelectric grazing-incidence spectrographs, by the AFCRL; the resolution and wavelength precision are considerably less, but the intensity measurements are more accurate than for the NRL results. This is a spectral region of great interest, containing important continua and chromospheric and coronal lines. To make further progress a larger instrument is needed for increased wavelength accuracy and resolution, with longer exposure times and/or increased reflectance to record the spectrum more completely. Here again, the stabilized Aerobee-150 is greatly needed.

The range 500-149 Å has been recorded photographically with grazing-incidence spectrographs (equipped with aluminum filters 1000 Å thick to eliminate stray light) and photoelectrically with the AFCRL and GSFC scanning monochromators. 500 Å is, of course, an arbitrary division point, below which grazing-incidence instruments usually become faster than normal incidence. With the small NRL photographic instruments, a wavelength precision of 0.03 Å and resolution of 0.1 Å have been attained over part of the range, and are expected in the future over the entire range. The aluminum filter sets a limit at 149 Å, but the solar spectrum itself becomes weak below 171 Å and very weak below 149 Å. For further progress here, larger grazing-incidence instruments are required. Again, the solar-stabilized Aerobee-150 is needed, with a peak altitude of 275 km. Longer exposure times are also needed. An astronaut-operated grazing-incidence instrument permitting exposing for long time periods is being constructed for Apollo. Following this the Apollo Extension Systems offer even greater promise. Much research in the laboratory is required to develop satisfactory methods of measuring intensities.

From 149-80 Å, the spectrum has been recorded photoelectrically by AFCRL, but photography of this region awaits the perfection of metallic filters other than aluminum, for example, beryllium. The intensity of the signals obtained has been very low, so that the wavelength precision is greatly reduced by statistical noise and one can be certain of only a few strong lines. No doubt, when the Sun once again becomes active, the many lines that are surely present will be recorded with precision sufficient to make certain of their wavelengths and identifications. Except for a few stronger lines, one can say little more than that radiation is present, at an intensity level given by an averaged curve. For future progress, work on the beryllium filter is badly needed. Long exposure times will be required for photography. The photographic experiment is an excellent one for Apollo and the Apollo Extension Systems, and the photoelectric version will certainly find a place in a large orbiting telescope.

From 80-33 Å, the spectrum has been photographed with the NRL grazing-incidence instrument. This was possible because aluminum becomes transparent once again in this range, as the K edge at 8 Å is approached. The photoelectrically recorded AFCRL spectra extend to 55 Å, but the intensity is extremely low, and there is only qualitative agreement with the photographic spectrum. Some further progress here can be made with existing vehicles, since considerably improved grazing-incidence spectrographs can be mounted within the existing biaxial pointing controls (BPC). Longer exposure times than rockets permit are greatly needed; therefore, the use of Apollo and of the AES is strongly recommended. Larger instruments will also be needed to obtain the high resolving power that will be required to resolve the many closely spaced lines that are certainly present. Here, the spectrum will change greatly with solar activity.

Below 33 Å it is still entirely feasible to obtain spectra with grazing-incidence instruments; longer exposures and a grazing angle of 2° rather than 5° are required. The Bragg crystal spectrometer is a powerful instrument for this range. The first spectra here were obtained by such an instrument, flown in 1963 by NRL, and covering the range 13-25 Å. For future work with Bragg spectrometers, the triaxially stabilized Aerobee is greatly needed. This spectrometer could also be flown on AES, and operated by the astronaut. This would be of interest when solar activity is high, and especially during a flare.

The conclusions from this summary of spectrographic results and needs are:

a. The triaxially stabilized Aerobee-150 is greatly needed. Even with 1 min of arc pointing it would be valuable, but 5 sec of arc pointing is required for much of the work. If this is not made available soon, the British will leave us behind in XUV solar research.

b. Apollo will be quite useful for small spectrographic experiments, because of the long exposures available, with the recovery of film, and the possibility of exposing during a flare. The AES provides an opportunity to extend this capability to large spectrographs.

c. Much research in the laboratory needs to be done to increase spectroscopic capabilities in the XUV.

LINE PROFILES

From 3000-2200 Å, profiles having $\lambda/\Delta\lambda = 10^5$ are available from the echelle spectra. They suffer from stray light that fills in the cores of the strong lines, and they apply to the central region of the Sun only. The profiles of the H and K lines of Mg II are fairly satisfactory as to spectral resolution; the intensity data are probably good to about 5% across these lines.

Beyond 2000 Å, the only lines as yet studied at high spectral resolution are Ly-α and Ly-β of H. Ly-α was photographed on July 21, 1959, and on April 19, 1960, at 0.03 Å resolution; Ly-α and Ly-β were photographed on August 22, 1962, at 0.1 Å resolution. Spatial resolution was about 1 min of arc.

There are many chromospheric and coronal lines which should be studied at high spectral and spatial resolution. Larger instruments, triaxial stabilization, and longer exposures are required. Much could be done with a 5 sec of arc stabilized Aerobee. For time sequence studies the AES should be considered.

This type of work complements spectroheliographic studies and is necessary for deciding how to conduct them.

SPECTROHELIOGRAMS

The first detailed XUV spectroheliograms were those of Ly-α of H, obtained in 1959 by NRL. The spatial resolution was 30 sec of arc or better, and has not yet been surpassed. Since this time, photographic spectroheliograms using lines farther in the XUV have been obtained, using a single grating at normal incidence, and an aluminum filter. At their present size, 2 mm in diameter, it is difficult to make quantitative measurements. They do serve very well to show that He II (304 Å) is emitted strongly from the entire disk, and also from active regions, whereas Fe XV (284 Å) and Fe XVI (335, 361 Å) are emitted almost entirely from the active regions. Monitoring of the XUV spectrum by GSFC from OSO-I led to a similar conclusion, based on the changes in line intensities recorded from the entire disk as the Sun's rotation carried active regions into and away from view. To separate other than the strongest isolated lines from the other lines 171-500 Å by such a simple means would require far greater dispersion than is now attainable. More complicated spectroheliographs will be required. Partial spectroheliograms have been obtained by NRL with C III (977 Å), Lyman-β (1026 Å), and O VI (1032, 1038 Å). Recently, GSFC has obtained a spectroheliogram in Mg II (2800 Å).

To shorter wavelengths, pinhole-camera photography is now producing valuable pictures of the Sun in various x-ray bands, defined by filters. The first such photographs, obtained by NRL, suffered from rotation of the image produced by precession of the Aerobee. The U.K., however, thanks to its three-axis stabilized Skylark, has already obtained sharp rotation-free images in several bands. Quite recently, the Kitt Peak National Observatory has obtained the highest-resolved x-ray image with a pinhole camera mounted with a BPC on an Aerobee that was prevented from precessing by a new cross-spin stabilization.

Excellent x-ray images have also been obtained by GSFC in collaboration with American Science and Engineering, Inc., Cambridge, Massachusetts, with a cylindrical parabolic grazing-incidence lens. Two rocket flights have been made in which the Sun has been photographed in wavelengths below 10 Å, between 8-15 Å and between 44-60 Å. The spatial resolution of a March 1965 flight was approximately 1 min of arc, and from a cursory look at the photographs it is obvious that most of the radiation in wavelengths shorter than 15 Å is emitted by plage groups. Limb brightening can be seen in the 44-60 Å photograph, with this radiation being observed at least 50,000 km above the disk. Grazing-incidence optics now exist that should allow resolutions between 5 and 10 sec of arc.

A monitoring type of spectroheliograph, constructed by NRL for OSO-II of NASA, was placed in orbit in February 1965. The wavelengths monitored successfully were Ly-α (1216 Å), He II (304 Å), with a 1 min of arc resolution. Several satisfactory images were obtained. This equipment failed rather early, due to high-voltage breakdown problems; the source of the difficulty has not been established, but it appears possible that the instrument may have acted as a trap for solid particles which eventually built up to the point where they short-circuited critical parts of the electronics.

In order to make progress in spectroheliography it is absolutely essential to have an Aerobee-150 rocket, or the equivalent, which is stabilized to 5 sec of arc in yaw and pitch, and to 0.25° in roll. Three-axis stabilization is required, to make use of long exposure times; at least 5-ft length is necessary to produce solar images large enough to use for quantitative photographic photometry.

Future spectroheliography from OSO and AOSO satellites holds great promise, but much research may be required before the difficulties associated with the operation of optical photomultipliers at high voltage in space vacua are understood and eliminated.

INTENSITY MEASUREMENTS

Measurements of intensity in the XUV, even on a relative basis, or over a short wavelength range, are notoriously difficult. Much work needs to be done in the laboratory to develop standards of intensity, methods of calibration, and so on. This kind of work carries little glory, and has lagged.

The solar intensity distribution is thought to be determined from photographic spectra with an absolute accuracy ±10% from 3000 to 2500 Å. At shorter wavelengths the accuracy gradually deteriorates to perhaps ±20% at 2100 Å, and to ±50% at 1300 Å; Ly-α is, of course, variable with the solar cycle, and this is probably true of many other lines, and the chromospheric continua.

From 900 Å to 170 Å the most accurate intensity measurements are those of AFCRL, made photoelectrically. In the x-ray range various investigators have performed careful calibration of photon counters, but here the solar output varies greatly with activity.

SOLAR MONITORING

This has already been touched on in the preceding sections. Obviously, it is work that must be carried on in orbiting vehicles; except for relatively short-time monitoring, the vehicles must be unmanned.

The first and most extensive series of monitoring experiments are those of NRL, which have been conducted periodically from small satellites since 1960. Generally, hydrogen Ly-α, and several x-ray bands are recorded. Excellent results are obtained, but at present data transmission is limited to real-time telemetry. Therefore monitoring is far from continuous. OSO-I and OSO-II have, however, provided continuous monitoring of Ly-α, XUV spectra, and x-ray emissions from the Sun, except when in the Earth's shadow. OSO is an ideal vehicle, since it provides solar pointing and data storage. If placed in a polar orbit, it would appear to meet all monitoring needs.

Lyman-α from Flares

An ionization chamber sensitive to Ly-α was flown on OSO-I. During the lifetime of the experiment, several flares were observed. These showed enhancement of the Ly-α flux of 2-10% which, when the area involved is considered, indicates that the brightness enhancement in Ly-α from the localized region as compared with the background could be as much as 100 times greater.

Solar XUV Spectra

The use of the OSO-I as a stable platform permitted the acquisition of solar XUV spectra which can be tentatively associated with a corona disturbed to varying degrees by visible centers of activity. Periods of relatively low solar activity were followed by periods during which active centers appeared on the solar disk. The increases and decreases in flux can be associated with the appearance and disappearance of these centers. Analysis of the observed emission lines demonstrates that the lowest counting rates of the period were observed when the sunspot number was near zero and the calcium plage area on the Sun was also at a minimum. However, it is also clear that no exact correlation can be assumed to exist between the XUV fluxes and ground-based observations. Fe XV (λ284) radiation has a different time dependence on the age of active regions than has 2800 MHz microwave radiation. It increases more slowly than does the microwave radiation as the active center develops, but remains intense even after the sunspots and flare activity have disappeared and the microwave radiation is decreasing. The continued enhancement of the Fe XV line after all sunspots have vanished may be an indication of remaining coronal structures. In any event, these observations suggest that it is necessary to have knowledge of the recent past history of solar activity as well as current data in order to make a correlation of XUV radiation with other data.

The XUV emission lines display fluctuations which differ from one line to another. In particular, one may observe fluctuations in the helium line which are not found in the other lines. These short-lived variations can sometimes, but not always, be associated with the brightening of existing plages and the occurrence of radio noise storms at 169 MHz.

The coronal lines of Fe XV and Fe XVI are strongly associated with plages, but do appear to have residual intensities even if the Sun shows no sign of activity. A quiet Sun component does exist when one extrapolates the Fe XV counting rate to zero plage area. Assuming that the regions of increased Fe XV emissions are equivalent in area to the plages, one obtains a plage-to-quiet Sun Fe XV ratio of between 200-300:1, considerably beyond the latitude of photographic film. This quiet Sun component may perhaps be associated with coronal fine structure rather than being uniformly distributed over the solar disk.

We observe that those ions that exist at electron temperatures below about 1 x 10^6 °K (and these include the lower stages of ionization of iron as well as ions of Si VIII through Si X and Mg VIII and IX) show little association with active regions, while those ions existing above 1 x 10^6 °K show a strong association with plages and active regions. The fact that all lines show an increase with activity during the first two weeks in March merely indicates an increase in density in and around the active region. It is not clear from the data whether the large increases in Fe XV and XVI are due to a combined increase of electron temperature and density over plages, or whether localized regions in which these emissions might occur merely increase in number over plages. All lines are observed to fluctuate, if only slightly. The smaller the fluctuations, the less well they correlate with solar activity. However, since the experiment lacked spatial resolution, it is not possible to state how the smaller enhancements in intensity are distributed on the solar disk.

X-Rays Below 11 Å

An ionization chamber was flown on OSO-I to monitor 1-11 Å x rays. Because at the time of launch the solar cycle was approaching minimum, the sensitivity of the experiment was set to observe the quiet Sun. Full-scale sensitivity was 1.8 x 10^{-3} erg/cm^2-sec. This would allow observations for quiet periods, but was somewhat of a handicap in that when the Sun was relatively active the reading was off scale. The smallest measured value occurred on April 6, 1962, at which time a value of 1.8 x 10^{-4} erg/cm^2-sec was observed. The flux values for 2.5 solar rotations varied with plage activity on the disk of the Sun and were the first observations to demonstrate that practically all of the x rays in this wavelength region when no flares are present are emitted from the plage regions. The data also showed that x radiation from the Sun is quite variable and that completely quiet periods are rare. Only six orbits were found in which the flux did not vary by more than 5%.

Strong correlation was found between 1-11 Å x rays and the 9.1-cm radio fluxes. Calculations made using temperature and density data supplied by the High Altitude Observatory showed that the x radiation could not be a continuum. Since the calculated values were an order of magnitude too low, it was concluded that line emission must be a major component. This has been substantiated by subsequent NRL observations.

Attempts were made to correlate x-ray flare data with H-α flare observations; of a sample of 22 events, 11 correlated well. However, there were six full-scale x-ray events where one would have expected an H-α flare to be observed (based upon the fact that smaller x-ray events were observed for which H-α flares were reported), but for which no H-α flare was seen. The question of correlation is still open since it may be that if increasingly smaller events were classified as flares, one might find that all of these events would have been observed in H-α. On the other hand, one might well ask, is there a type of event in which x rays are emitted without a counterpart in H-α?

In attempts to compare the x-ray data with other measurements such as optical data and radio data, one concludes that x radiation is the most sensitive detector of solar activity. As an example of this, several events were found in which the x-ray experiment observed a 100% increase in flux, whereas for the 9.1-cm radio data, the event would not have been detected unless one knew at what time to examine the data closely. The data tracing varied approximately the width of the plotting pen line.

HARD X RAYS

Attempts to detect x rays of energies greater than 100 keV from the nonflare Sun have been made from balloons. The detector consisted of a scintillation counter collimated with an active shield consisting of a secondary scintillating material. Although the signal-to-noise ratio was very good, only an upper limit could be determined: for energies above 130 keV, an upper limit was 0.137 ± 0.053 photons/cm^2-sec, and for energies above 300 keV, 0.067 ± 0.044 photons/cm^2-sec. These numbers represent the excess counting rate looking at the Sun as compared with that looking away from the Sun. It is impossible to determine whether the excess count is genuine or not because there may be terminal effects that caused the count to differ. However, instruments have now been built so that in-flight calibration can be made.

An experiment to observe x radiation from the Sun of energies between 20 and 100 keV was flown on OSO-I. As a consequence of the large amount of data obtained, it is possible to set an upper limit of 3.4 ± 0.95 photons/cm^2-sec for nonflare solar fluxes. Eight x-ray bursts associated with solar flares were observed. This doubled the number of observations of this type of burst that had been observed up until that time. For one of the events, there was a good ground-based observation in H-α and it appears that the x-ray event was associated with the explosive phase of the visual flare. Two of the events were of particular interest because they exhibited a double peak in x radiation which raises interesting questions concerning the source of radiation.

Monitoring of the outer white-light corona cannot be done from the Earth's surface, except at the time of a total eclipse. This is a project for orbiting vehicles. The first noneclipse photographs were obtained on June 28, 1963, by NRL, from an Aerobee rocket. Balloon altitudes are nearly sufficient, however, and the High Altitude Observatory has obtained coronal photographs with externally occulted balloonborne coronagraphs.

A white-light coronagraph constructed by NRL was operated successfully in OSO-II for the first 1040 orbits. Data reduction has hardly begun, but it appears that the corona was monitored satisfactorily as to its general form from R = 3 to 7 solar radii in both radial and tangential polarizations. The intensities were about twice those observed during eclipses, as was the case with the rocket coronagraph. This is ascribed to dust near the Earth and the fact that during an eclipse sunlight is prevented by the Moon from illuminating much of the relatively nearby atmosphere. It is hoped to detect streamers and changes in the K corona from the data obtained from OSO-II, but the noise in the signal was high, probably partly a result of the dust near the spacecraft. Much smoothing of the data will be required.

The improvements needed for corona monitoring are: a greater separation of the external occulter from the rest of the instrument, a cleaner environment near the spacecraft (though this requirement is not absolutely established), more telemetry, and video techniques. These improvements are planned in the AOSO coronagraph being constructed by the High Altitude Observatory. Another possibility is the use of a manned spacecraft with a guidable occulter system located at a relatively great distance.

APPENDIX: LIST OF PARTICIPANTS

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IV
Radio and Radar Astronomy

1. INTRODUCTION AND SUMMARY OF RECOMMENDATIONS

TASK OF THE WORKING GROUP

The Working Group attempted to carry out the following tasks:

(a) To review the present status of and known plans for the development of radio and radar astronomy in space.
(b) To foresee and outline the major instrumental and observational advances which should take place over the next 15 years.
(c) To form opinions on the use of man as part of the space experiments foreseen.

GENERAL REVIEW OF THE STUDY

The work of the Group can be divided by subject matter into three main subject headings:

(a) Long-wave radio astronomy. Here the Group considered observations made in the frequency range from 10 MHz (30 meters wavelength) to a few hundred kHz (300 kHz corresponds to 1 km wavelength).

(b) Millimeter and long-wave infrared astronomy. The wavelength range here was taken to be from 10 mm wavelength (30 GHz) to a few tens of microns. The short wavelength end of this spectral range was also discussed in the Working Group on Optical Astronomy.

(c) Radar astronomy. The wavelength range here did not require definition; most observations fall in the range between about 6 meters (50 MHz) and centimeter wavelengths. The subject of solar radio and radar astronomy was studied and is reported under this section of the work.

The background information and other considerations under these three headings, and under a fourth general category where techniques and specific questions involving man in space and Moon-based experiments were discussed are set out in detail later in Sections 2 through 5 of this report. Those sections also contain suggestions which the Group wished to make, but which were not of a nature suitable for formalizing into recommendations. The recommendations are listed in the next paragraphs. In this listing an attempt has been made by the Group to mark their most important recommendations by placing an asterisk against them. Such a choice is clearly difficult to make; it is intended to show those recommendations which, if followed, are in the opinion of the Group likely to give the greatest yield in new results.

RECOMMENDATIONS

Long-Wave Radio Astronomy

Recommendation 1. The Radio Astronomy Explorer (RAE) satellite series should be continued and expanded. In addition to having inherent value it is a prerequisite for the instrumental developments proposed in Recommendations 2 and 3. We foresee that, when running at peak rate, launches of at least one a year will be needed. Universities, NASA, and other research institutions should be encouraged to develop payloads and every effort made to shorten the lead times required for approval of a program.

The importance of solar radio astronomy during the maximum phase of solar activity requires that at least two RAE satellites instrumented for long-wave solar radio observations be flown near sunspot maximum to provide data on the low-frequency characteristics of solar radio bursts.

*Recommendation 2. A space radio telescope with an aperture of the order of 20 km appears to be close in size to the ultimate for observations between a few MHz and a few hundred kHz. With larger apertures, effects due to irregularities in the interplanetary medium will, according to our best present knowledge, become the factor that limits the resolving power of the telescope. We recommend that the National Academy of Sciences appoint a panel now to study possible conceptions of such a radio telescope and to initiate studies of the scientific and engineering problems connected with its construction.

*Recommendation 3. Since the study recommended in Recommendation 2 is for an ultimate space radio telescope, work should be started now that will lead to the use in space, within about ten years, of a high-resolution broad-band antenna system for radio-astronomical observations over the frequency range 10 MHz to a few hundred kHz. The task should start with a design study, of perhaps two years' duration, undertaken by a group with scientific and engineering competence. Such a group will require excellent leadership and should foster and develop cooperation among all those interested in the development of space radio astronomy.

The following guidelines are suggested for starting the work of the proposed group:

(i) Determine whether an antenna covering about 10 MHz to 500 kHz and having a beam area of about 100 square degrees at 1 MHz is feasible and meets the scientific needs.

(ii) Consider alternative design concepts, among which should be the possible use of a simple or compound interferometer system to give added resolution within the main beam area.

(iii) Consider and compare locations for such an instrument, particularly as between a high orbit (which would almost certainly be higher than a synchronous orbit) and a lunar base.

(iv) Consider this antenna as a possible payload for the Apollo Extension Systems.

Millimeter-Wave and Long-Wave Infrared Astronomy

Recommendation 4. The exploitation of millimeter-wave and far infrared observing capabilities from ground-based, aircraftborne, and balloon-borne facilities should be encouraged and supported by NASA.

Recommendation 5. Good telescopes for millimeter and far infrared wavelengths with apertures up to several tens of feet are feasible on Earth. However, much larger apertures of the order of 100 feet will be needed for future advances. Such instruments should avoid the gravity environment on Earth and avoid the Earth's atmospheric shielding. It is recommended that NASA make studies of the technology, feasibility, and cost of building and operating such telescopes in space. The launching and maintenance of such a telescope could possibly be a part of the Apollo Extension Systems.

Radar Astronomy

Recommendation 6. Since ground-based radar astronomy studies of the Moon and planets form a natural part of the NASA mission, they should be vigorously supported by NASA.

Recommendation 7. NASA should include in the Voyager series planetary orbiters containing a swept- or multifrequency radar system designed to detect and measure any planetary ionosphere and, from the radar scattering properties of the planetary surface, to derive information as to its nature and topography. Components of the same system can easily be used to make radiometric studies of the planetary surface and atmosphere.

In view of the great interest of the magnetosphere, ionosphere, and atmosphere of Jupiter, the first Voyager to approach that planet should carry a variety of radar and radiometric experiments designed to study these regions of the planet.

Recommendation 8. NASA should conduct, in the Voyager series, investigations of planetary surfaces and ionospheres by means of bistatic radar systems with one element of the radar on Earth and the other on the probe.

A similar type of experiment in which the probe, carrying either a transmitter or a receiver is sent on a trajectory such that the probe is occulted by the planet, is recommended as a powerful technique for the study of planetary atmospheres and ionospheres.

Recommendation 9. NASA should use lunar orbiter missions to test the instruments to be employed in planetary probes needed to conduct the experiments suggested in Recommendations 7 and 8, and also to add to our understanding of the nature of the lunar surface. These missions would be possible payloads for the Apollo Extension Systems.

Recommendation 10. A substantial extension of existing radio and radar observations of the solar corona and interplanetary medium is recommended. This requires that large ground-based antenna systems for transmitting and receiving be built, together with the necessary high-power transmitting equipment. New advances in antenna and computer technology should be fully exploited.

This equipment would make solar radar and radio astronomy observations. Suitable interplanetary probes carrying multifrequency receiving and transmitting equipment should be launched so that observations of the signals propagated between the ground station and the probes can be used to study the solar corona and the interplanetary medium.

The ground-based equipment should be regarded as a national facility, and its construction should be given high priority so that observations may be commenced during the coming sunspot maximum.

Miscellaneous

Recommendation 11. This Working Group recommends that NASA devote a much larger fraction of its resources to the construction of ground-based deep-space telecommunication terminals. The object is to increase the amount of information that will be returned from solar and planetary spacecraft now proposed.

2. LONG-WAVE RADIO ASTRONOMY

THE GOALS OF LONG-WAVE SPACE RADIO ASTRONOMY

The goals of long-wave radio astronomy may easily be defined. There is a region of the radio-frequency spectrum where the Earth's ionosphere acts as a shield to prevent ground-based observations of the radio emissions that reach the vicinity of the Earth. This region of the spectrum extends from frequencies of a few MHz downward to a lower frequency limit, where observations would prove to be impossible because of the effects of the interplanetary plasma.

The upper-frequency limit of this blocked spectral region varies as the electron density in the ionosphere varies. Thus, occasional observations may be made from Earth at frequencies as low as 1 MHz, as Grote Reber and G.R. Ellis have shown. Consistent ground-based observations on frequencies as high as 10 MHz are still, however, most difficult to achieve, partly because the critical frequency of the F region often exceeds this value and partly because, even on those occasions when radiation can penetrate the F region from directly above, long distance oblique incidence propagation is still possible. Thus many, ground-based radio transmitters can send strong interfering signals to a radio-astronomy antenna and prevent radio-astronomical observations from being made.

Above the F-region ionization maximum, however, the electron density steadily falls, to merge eventually with that of the plasma surrounding the Sun. In this region of space, long-wave radio astronomy is practicable. An antenna placed here can both receive signals from outside the Earth and, by the shielding of the ionosphere which is now between it and the ground, be freed from interference by man-made signals generated on Earth.

With such an antenna all the kinds of observations which radio astronomers have made from the Earth become possible for this new region of the spectrum. Eventually all these observations will be made; until then their relative scientific value can only be predicted. At present, space radio-astronomy experiments in this new spectral region are being chosen to combine the greatest scientific interest with the greatest instrumental simplicity.

The Brightness of the Radio Sky

As an example, all radio-astronomy experiments that have been flown in satellites have tried to measure the average brightness of the radio sky at a few places in the spectral range from 10 MHz to 725 kHz. Such experiments can be made with essentially nondirectional antennas and are thus simple to perform. Yet the scientific results are needed to understand the mechanisms of emission and absorption of radio waves within the galaxy. The spectrum of this galactic background radiation shows a continuous increase in sky brightness as the frequency is reduced until about 2-3 MHz is reached; here the spectrum turns back to lower values. This has been interpreted as the effect of absorption due to ionized hydrogen in the galaxy. Further experiments can test the truth of this interpretation and then, with improved angular resolution, study the distribution and properties of ionized hydrogen in the Galaxy.

Solar and Planetary Radio Emissions

Another goal, soon to be reached, is the extension into this new frequency range of the observations of radio waves emitted by the Sun and the planet Jupiter. Both of these are remarkable and sometimes energetic radio sources. Although radio waves from the Sun have been studied for twenty years and a wide variety of bursts observed and classified, it is true that the origins of these phenomena are still far from fully understood. Equally, the relations between these kinds of solar activity and terrestrial effects still present many problems. Study of the low-frequency solar bursts will give information on the plasma in which they originate; at these frequencies this is several solar radii out from the Sun. The radio waves from the undisturbed Sun also require study in this new spectral range.

Jupiter is the source of large radio bursts, already well observed from the ground in the frequency range from about 50 MHz to the Earth's ionospheric limit near 10 MHz. The planet also emits, probably because of a radiation belt system, strong nonthermal radio radiation at microwave wavelengths.

The currently accessible spectral range, within which the bursts may be seen, is clearly limited by the Earth's ionosphere at the lower-frequency end. An extension of observations here will help in understanding both how the bursts originate and perhaps also in showing whether Jupiter itself is subject to the same sort of Sun-induced effects as the Earth.

These solar and Jovian emissions are very powerful and can be studied with relatively simple satelliteborne antennas. Some degree of directivity is desirable, as that being designed into experiments such as the Radio Astronomy Explorer (RAE) series of satellites.

Galactic and Extragalactic Radio Astronomy

The goals described so far are important, and they are discussed first because they can be achieved with fairly simple systems. To carry out the most important studies in radio astronomy, the study of radio waves from all the diverse kinds of radio sources already known, requires a major instrumental step. If that step could be taken, so that reasonable directivity, gain, and ability to study polarization were available in a radio-astronomy experiment in space, a wide variety of new questions could be studied.

The sky background could be mapped with resolution adequate to see the distribution of ionized hydrogen in interstellar space. Measurement of its absorption may lead to new insight into the mechanisms of star formation. The contributions to the sky background from our own galaxy and from the extragalactic medium can be separated. Polarization studies may improve knowledge of the magnetic fields in interstellar space although, possibly, large Faraday rotation effects may make the results difficult to interpret.

Studies of the burst phenomena of the Sun and of Jupiter would be continued and extended. Similar phenomena from other planets may be discovered in the new spectral region.

As soon as the stronger of our known radio sources, among which are the supernova remnants, the strong radio galaxies, and the quasi-stellar sources (quasars), can be detected at lower frequencies and isolated from the galactic background, many fruitful researches should emerge.

The fluxes of these sources must be measured and their known spectra extended. In this low-frequency region it is possible that the radio-wave absorption, which occurs perhaps in or near the source or within our galaxy, can be studied and the effects of self- and galactic-absorption separated. Detailed data on the low-frequency spectra can give knowledge of one or more of the following characteristics of quasars and radio galaxies: the density of cosmic-ray electrons, the magnetic field strength, the plasma density, and the low energy cutoff of the cosmic ray electrons. In both these kinds of sources unknown yet highly efficient mechanisms of energy conversion are at work.

At high frequencies, measurements of polarization in sources already suggest the presence of a galactic or a source magnetic field. There is also some evidence that the radio flux from quasars varies with time. These phenomena should be studied at low frequencies also.

Although angular resolution will be practically difficult to achieve directly at these long wavelengths, the technique of getting effective resolution by observing sources as they are occulted by the Moon should be most valuable. Even on Earth, where the Earth-Moon source geometry is not subject to control, occultation work is of major value. In space it should be a most powerful technique. So also may be the application of interferometric or aperture synthesis techniques.

POSSIBLE LIMITATIONS AND DIFFICULTIES

Before proceeding to suggest experimental techniques in long-wave space radio astronomy, some limitations must be discussed.

Although removing the antenna from the Earth does remove the problem of ionospheric shielding, it still leaves the antenna embedded in an ionized medium. This can cause at least two fundamental but different kinds of lower limit. First, the antenna can be used only for frequencies significantly above the local plasma frequency. Second, it is known that the interplanetary medium is irregular in density; the irregularities can produce scintillations in the intensity and oscillations in the position of small-diameter radio sources.

The effects of the average density of the interplanetary medium depend only on the electron density. Something is known of this as a function of distance from the Earth; more knowledge is needed and will come from high-altitude rocket, probe, and satellite experiments. It has been estimated that the plasma limit for observations at a height of 6000 km could be 0.15 to 0.30 MHz, but might be as high as 0.50 MHz. Measurements on Elektron II showed 0.30 MHz at 10,000-15,000 km. As more results appear, the dependence on height of the lowest useful observing frequency will become better known. At present it seems reasonable to say that quite high orbits, probably above the synchronous orbit, may be needed for major low-frequency space antennas.

The effects of plasma irregularities on the scintillation of small-diameter radio sources have become the subject of a number of ground-based studies. The results already show that irregularities, of the scale of the order of at least 100 km in size, are distributed in the plasma and solar wind around the Sun out to well beyond the Earth. Studies of these irregularities are valuable scientifically and will continue; here we are mainly concerned with the limits they impose on the resolution of a long-wave radio telescope. These limits have been calculated approximately by W.C. Erickson and suggest that, for a frequency of 1 MHz (300 meters wavelength), observations with finer angular resolution than 24 minutes of arc would be impossible even with an antenna pointed away from the Sun. This limitation may be thought of as similar in effect to the atmospheric seeing limit for optical telescopes. It suggests that, to the best of our present knowledge, this effect will set a limit to the size of a long-wave space radio telescope beyond which it would not be useful to go in pursuit of better angular resolution. At 1 MHz the angular limit quoted above implies that the telescope should not be more than 43 km in aperture.

Radio Noise Limitations

Although the ionosphere will shield a space antenna from the Earth, such a telescope still may suffer from ionospheric effects. The USSR Elektron II and IV satellite experiments gave evidence of sporadic radio emission from the ionosphere. At 0.725 MHz this had the character of bursts of radio noise, though sometimes it was just a general rise of the noise level. The sporadic noise was sometimes two orders of magnitude above the cosmic noise being measured.

Man-made noise will be serious on those occasions when the ionospheric critical frequencies fall, but may also be serious if the use of the radio-frequency spectrum in space grows. This problem is already under study by a committee of the National Academy of Sciences; the present report will be brought to the attention of that committee.

The Immediate Future of Long-Wave Space Radio Astronomy

The likely scientific gains and the possible fundamental limitations on growth of this science were discussed by the Working Group and have been summarized above. The immediate plans in this country for space radio astronomy rest at present with the groups at the University of Michigan, Harvard College Observatory, and the Goddard Space Flight Center. Much of the scientific success soon to be achieved depends on the success of the GSFC Radio Astronomy Explorer (RAE) satellite program. In discussing this program, several points emerged:

(i) The results achieved so far demonstrate the potential value of the observations. Sky brightness can be measured. The dynamic spectra of solar, terrestrial, and planetary radio bursts should be observed during the program. The results in all these fields will be valuable scientifically and essential for the design of future space radio-astronomy experiments.

(ii) The present program is one which could be an excellent training ground for graduate students and recent graduates. However, lead times for approval of programs for the RAE series of satellites must be reduced not only to attract and keep good students but also to allow for a rapid feedback of new techniques and results into new experiments.

(iii) Rocket probe experiments will continue to be important as rapid tests of new techniques and ideas. There is no vehicle that could fill the gap between the Astrobee 1500 and the Scout (for instance, capable of taking 50-70 lb of payload to 20,000 km) except perhaps the Air Force Blue Scout, Jr.

(iv) The RAE satellites are very suitable for observing the phenomena of solar bursts, using either a swept frequency or a multifrequency radiometer. The next solar maximum falls at about the time when it should be possible to have such instruments flown; plans should be made soon to fly solar radiometers at or near the solar maximum.

(v) The rate of launching RAE satellites could grow to be about one per year and the series might well continue for some time.

The group, in making Recommendation 1, had these considerations in mind.

Recommendation 1. The Radio Astronomy Explorer (RAE) satellite series should be continued and expanded. In addition to having inherent value it is a prerequisite for the instrumental developments proposed in Recommendations 2 and 3. We foresee that, when running at its peak rate, launches of at least one a year will be needed. Universities, NASA, and other research institutions should be encouraged to develop payloads and every effort made to shorten the lead times required for approval of a program.

The importance of solar radio astronomy during the maximum phase of solar activity requires that at least two RAE satellites instrumented for long-wave solar radio observations should be flown near sunspot maximum to provide data on the low-frequency characteristics of solar radio bursts.

Large Space Radio Telescopes

If radio astronomy in the long-wave part of the spectrum is to have the capability of observing reasonable numbers (perhaps a hundred or more) of discrete radio sources, antenna systems of large size will be needed. As an example, at a frequency of 1 MHz (300 meters) a filled-aperture antenna needs to be 10 km across in order to give a beamwidth of 2 degrees. Antennas of this size would still have a very poor angular resolution when compared with most present-day ground-based instruments. Since the flux from radio sources is high in this long-wave region the large aperture required for resolving power need not be fully filled to provide a large collecting area. The degree of filling required depends on the type of observation.

The long wavelengths also give considerable relief in the structural and dimensional antenna tolerances which are needed. These, as in ground-based telescopes, need to be maintained to only about $\lambda/16$. The pointing precision, or knowledge of the direction in space toward which the radio beam of the telescope is pointed, again needs to be known to only about one-twentieth of the radio beamwidth.

Since any antenna system used at these wavelengths is always receiving signals from the sky background, which has a brightness temperature of the order of 10^7 °K, the requirement for low-noise radiometers is not stringent. Problems connected with the behavior of the antenna impedance, pattern, and collecting area, both because of the need for operation over a wide frequency range and because of its immersion in a plasma, are quite serious. They are, however, presently under study and are partially understood.

The mechanical problems of designing, launching, unfolding, or erecting, and using a large radio antenna in space could be discussed by the Group only in general terms. It seems certain that a structure made basically of wires held in place by some stabilizing system could meet the needs and be practical. Stabilization with a few small vehicles carrying thrust devices or possibly with gravity gradient methods might work.

Any antenna system of large size could be used over a period of several years for many different programs. It would be very reasonable to use the ability of men to erect, visit, adjust, modify, or repair the system from time to time.

These considerations led the Group to formulate Recommendations 2 and 3:

Recommendation 2. A space radio telescope of aperture of the order of 20 km appears to be close in size to the ultimate for observations between a few MHz and a few hundred kHz. With larger apertures, effects due to irregularities in the interplanetary medium will, according to our best present knowledge, become the factor which limits the resolving power of the telescope. We recommend that the National Academy of Sciences appoint a panel now to study possible conceptions of such a radio telescope and to initiate studies of the scientific and engineering problems connected with its construction.

Recommendation 3. Since the study proposed in Recommendation 2 is for an ultimate space radio telescope, work should be started now which will lead to the use in space, within about ten years, of a high-resolution broadband antenna system for radio-astronomical observations over the frequency range 10 MHz to a few hundred kHz. The task should start with a design study, of perhaps two years' duration, undertaken by a group with scientific and engineering competence. Such a group will require excellent leadership and should foster and develop cooperation among all those interested in the development of space radio astronomy.

The following guidelines are suggested for starting the work of this group:

(i) Determine whether an antenna covering about 10 MHz to 500 kHz and having a beam area of about 100 square degrees at 1 MHz is feasible and meets the scientific needs.

(ii) Consider alternative design concepts, among which should be the possible use of a simple or compound interferometer system to give added resolution within the main beam area.

(iii) Consider and compare locations for such an instrument, particularly as between a high orbit, which would almost certainly be well above the synchronous height, and a lunar base.

(iv) Consider this antenna as a possible payload for the Apollo Extension Systems.

It seemed reasonable to start planning for the instrument that Recommendation 2 suggests: a telescope whose performance would be approaching the limit set by irregularities in the interplanetary medium. It might have a beam area of only a few square degrees at 1 MHz, and thus would be hundreds of square kilometers in area. It would be a long-term national effort, probably requiring special thought on the problems of administration and management as well as on the scientific and engineering aspects. For these reasons, a start from a National Academy group seemed logical.

The antenna suggested in Recommendation 3 is still large, but probably within the abilities of an effort which drew on the best competence of those already in or willing to enter the field. It is to be hoped that proposals for a start on the instrument described in Recommendation 3 might be made to NASA without any more formal planning, and that a suitable proposal for a design study might receive support.

3. MILLIMETER-WAVE AND LONG-WAVE INFRARED ASTRONOMY

TECHNIQUES AND LIMITATIONS

The extension of astronomy into the wavelength region between 1 cm and a few microns (1 micron = 10^{-4} cm) has in the past been limited by lack of detectors of high sensitivity and by the obscuration of the Earth's atmosphere. In the last few years, the development of better detectors has allowed some work to be done in the 1-mm and 11-micron regions (using a supercooled germanium bolometer) and at 8 mm and 3.5 mm with improved radio techniques. Although the atmosphere of the Earth is an absorber throughout the whole wavelength range, there are regions of the spectrum where the absorption is low enough to permit observations from suitably chosen sites on the ground. So far, these observations have been mainly at 8 mm, 3.5 mm, 1 mm, and 11 microns.

Radiometer Development

The germanium bolometer, as developed by F.J. Low, has proved to be a very valuable detector over the wavelength range from 1 mm to 11 microns. Although it is not the only detector of high sensitivity, it is probably the best, and its performance is summarized below to show what it can now achieve.

The device is basically a simple bolometer, which detects radiant energy by absorbing it. The temperature of the bolometer element rises and thus its resistance changes by an amount depending on the incident radiation. The novelties of the new bolometer are in the choice of the bolometer material, the choice of a very low (liquid helium) working temperature with its very low noise level and enhanced change of resistance with incident energy, and in the techniques for making the whole detector. Problems of excluding unwanted energy and of defining a bandwidth of the device have been overcome.

At 1 mm the bolometer has been used on telescopes as large as the 200-inch Palomar instrument. The worst atmospheric effect is the noise fluctuation arising from water vapor irregularities in the air. This effect has been considerably reduced by using a technique that switches the telescope beam on and off the object being observed. The path through the disturbing part of the atmosphere hardly differs in the two beam functions, so that use of a switched radiometer technique can reduce the atmospheric effects to a small value by cancellation. The method is, of course, of no value for observations of extended sources.

The use of this technique at 1 mm with the best bolometers has reduced the radiometer fluctuations, measured as a root-mean-square (r.m.s.) input temperature fluctuation, to 15×10^{-3} °K with a time constant of 10 sec. The bandwidth of the system was about 10%. (In frequency 1 mm corresponds to 300 GHz.) The total absorption in a dry atmosphere (total precipitable water = 2 mm) at 1 mm is 15%. Using such a bolometer on the 200-inch telescope (which was designed for optical work and is therefore not a very efficient reflector at 1 mm) gives in practice a limit for the observable flux from a radio source at 1 mm of about 10 flux units [1 flux unit = 10^{-26} watts $(\text{meters})^{-2}(\text{Hz})^{-1}$].

Further development of bolometers can be foreseen such as reducing the working temperature from its present value, about 2°K, to perhaps as little as 0.25°K. This, together with other improvements, may increase the over-all sensitivity by a factor of 10.

The bolometer performs very well in a radiometer, partly because it accepts such a wide band of frequencies. Nevertheless, it can be used for spectral studies provided that the features being studied are not too narrow and that signal levels are not too low.

With techniques that are somewhat more conventional, at least to the radio astronomer, most work at 8 mm and 3.5 mm has been done with crystal mixer radiometers having total bandwidths in the 1 GHz region. With such radiometers, RMS fluctuations of about 0.1°K using a 10-sec integration time can be achieved.

Although it is tempting to assume that low-noise devices such as masers, parametric amplifiers, or perhaps tunnel diodes could be used in the millimeter-wave region, the technological difficulties have so far prevented any progress.

Ground-Based Antennas

The antennas are listed in Table 1, together with the shortest wavelengths at which they have been used or appear to be usable, to show the extent of millimeter-wave antenna technique.

Table 1. Reflectors Used for Millimeter-Wave Radio Astronomy

Reflector diameter	Location	Shortest wavelength at which it should be usable
16 ft	U. of Texas Austin	3 mm
15 ft	Aerospace Corp., El Segundo, Calif.	1 mm
200 in	Mt. Palomar	Optical
36 ft	For National Radio Astronomy Observatory, on Kitt Peak, Ariz.	Under construction; limit should be 1 mm

Observations at 11 microns have been made using a variety of reflectors, including the 84-inch at Kitt Peak National Observatory and the 200-inch at Mount Palomar.

RECENT RESULTS

The past three years have produced many new results. These are summarized below because of their importance in showing the directions of future research.

The Sun

The Sun shows blackbody temperatures of 5800°K at 1 mm and 6400°K at 3.5 mm. There is no limb brightening or darkening at 1 mm, and no signs of solar activity have yet been seen at 1 mm. At 3.5 mm, active regions have been observed and on four occasions a region showing enhanced 3.5-mm radiation has later been the source of a solar flare. Some regions that are 200-300°K cooler than the rest of the Sun appear to be areas in which the magnetic field is low or vanishes.

The Moon

The Moon has been studied extensively at all accessible wavelengths, during entire lunations and at 3.2 mm during a total eclipse. The results are numerous and show temperature differences associated with surface features and differences in temperature behavior at different parts of the surface. The observations give information which depends for its interpretation on a number of physical properties of the surface materials and thus cannot alone uniquely determine any single one of these properties.

The Planets

Venus, Mars, Jupiter, and Saturn have been observed at 1 mm and at 11 microns. The blackbody disk temperature of Venus is now well established over a wide frequency range.

Radio Sources, Quasi-stellar Sources (Quasars), and Red Stars

The Omega Nebula (Messier 17) has been detected with the 200-inch telescope at 1 mm. The Crab Nebula (radio source Taurus A) has been observed at 3.5 mm.

Quasars represent a very important task for millimeter-wave and long-wave infrared observations. The B component of 3C 273 has been observed in the 1-11 micron range. The spectrum may show an irregular behavior. It has also been seen with the 200-inch telescope at 1 mm and the flux appears to be higher than the value suggested from extrapolation of the known parts of the spectrum.

Since some quasars vary in optical intensity, and at least four show variability at radio wavelengths, they are of great interest for study in this 1-mm to 11-micron region. The spectra of quasars must also be determined in this very short-wave end of the radio spectrum.

The present results are somewhat tentative but of great interest. They raise many questions and suggest many new experiments. Measures of diameter and structure by interferometers or lunar occultations might eventually be possible even at millimeter wavelengths. Polarization measurements are of the greatest importance and are needed to understand the geometry of the magnetic fields in the quasars and radio sources. Possible irregularities or abnormalities in spectra should be looked for.

A few stars with surface temperatures as low as 700°K have been observed in the long-wave infrared region. Perhaps 200 such cold objects have been found photographically. Betelgeuse has a large emitting envelope at 11 microns; so do other M-type stars. The neon emission line at 12.8 microns has been observed and identified.

FUTURE PLANS

Ground-Based and Airborne Experiments

Ground-based research will grow considerably within the next few years. Observations can be made of bright objects with dishes carried by airplane or balloon to heights of 45,000 feet or above in some spectral ranges such as 100 to 300 microns. Such observations of the Moon may lead to a knowledge of the variations of the dielectric constant of the surface with frequency. The value of the scientific results together with the comparative simplicity of the techniques led to the adoption of Recommendation 4.

Recommendation 4. The exploitation of millimeter-wave and far-infrared observing capabilities from ground-based, aircraftborne, and balloon-borne facilities should be encouraged and supported by NASA.

Space Experiments

The possibilities and difficulties of a large millimeter-wave telescope in space were discussed at some length. No millimeter-wave telescope larger than 16 feet in diameter has yet been proved on Earth, but one of 36 feet is being built. This is probably about the limit of size at which uncompensated structures can be built on Earth and still maintain reasonable shape as their orientation with respect to gravity is altered. The thermal gradient problems will also prove to be close to the limit of manageability, even if such a telescope is protected by an astrodome.

A space radio telescope for these wavelengths would still be a parabolic dish. It might be made as a complete surface or, as is done with the elements of the Hanbury-Brown and Twiss stellar interferometer, as a set of independent reflecting surfaces with a common focus. To give a suitably large step in gain over ground-based instruments, a diameter of as much as 100 feet should be considered.

Problems of gravity deflections in space do not exist. There are problems of making such a dish on Earth and erecting it in space, since positional accuracies of the surface of 50 microns or better (one or two-thousandths of an inch) are needed. Such a dish would be a diffraction-limited antenna at 1 mm and a valuable energy collector in the micron range.

Thermal deflections will present a much more serious problem. Even the best thermal stabilizing paints now known will probably not solve this problem. It may be possible, however, to find techniques whereby thermal equalization over the structure to better than 1°C (which is about what is needed for a steel structure) could be achieved.

If such an antenna were built, it could be placed in a low orbit below the ionosphere and above the main atmosphere. Pointing precision of about 1 sec of arc would be needed. The problems of maintaining radiometer performance suggest that cryostats capable of holding the temperature to about 0.25°K would be needed.

The problems of servicing and maintaining equipment and probably the task of erection, suggest that such a system would require regular visits by men. The antenna would be used on a variety of programs over a period of several years.

Since such an antenna represents a considerable technological step, yet one which is certainly only of the same order of difficulty as those which are being solved in the Apollo program, it seems reasonable to start studies for such a telescope. These considerations led to Recommendation 5.

Recommendation 5. Good telescopes for millimeter and far-infrared wavelengths with apertures up to several tens of feet are feasible on Earth. However, much larger apertures of the order of 100 feet will be needed for future advances. Such instruments should avoid the gravity environment on Earth and avoid the Earth's atmospheric shielding. It is recommended that NASA make studies of the technology, feasibility, and cost of building and operating such telescopes in space. The launching and maintenance of such a telescope could possibly be a part of the Apollo Extension Systems.

4. RADAR ASTRONOMY

THE PRESENT STATUS AND RECENT RESULTS FROM GROUND-BASED WORK

Ground-based radar astronomy observations have now been made in some detail on quite a number of the objects within the solar system. Such observations will certainly continue to expand, although there does not seem to be much hope of extending them to the outermost planets and there is certainly no present hope of extending them to interstellar distances.

The techniques are well established and well understood. The refined systems which have been used within the past few years have brought the Sun, the Moon, and three planets under good observation. These techniques allow information to be collected about a number of physical properties of the object being studied. Table 2 shows a summary of the measurable quantities and the information which can be derived from them.

All these techniques have been applied to the Moon, and the detailed information now available is very considerable. Mercury, Venus, and Mars have been detected and studied. One group claims to have detected Jupiter, but another group working at a much lower frequency has not confirmed this result. The rotation of Mercury has recently been measured by radar; the result, a period of 60 ± 5 days, is very different from the previously accepted optically based value of 88 days. Observations of the Sun fall into a special class, since it is both a very noisy object and also one whose behavior may be expected to be extremely variable. Observations since 1961 have been made, using a special radar system on 38 MHz at El Campo, Texas, and already show important correlations between radar cross section and solar activity. Sufficient sensitivity is available to get some range-Doppler results, but the system (although very large physically) has neither enough angular resolution to resolve the Sun nor the steerability to track the Sun.

Table 2. Information Available from Radar Observations of the Sun, Moon, and Planets

Property Measured	Gives Information on
Total delay time of the echo	The orbit, for a Moon or a Planet
Doppler shift of returned signal	Planetary radii, movements of the plasma envelope; propagation within the planetary atmosphere
Total returned power	Reflectivity of the surface; composition of the surface
Dispersion in the delay time of the echo	Roughness of the surface, slopes of elementary parts of the surface
Degree of polarization of the returned echo	Roughness of the surface
Dispersion in frequency of the echo	Roughness of the surface; the rotation of the planet
Delay time and Doppler shift of frequency together	Range-Doppler method for getting detailed maps at high resolution of the surface scattering properties

These ground-based radar-astronomy techniques can and will be improved. Greater signal-to-noise levels at the receiver have to be obtained. This may be achieved with monostatic systems on or near the Earth by building larger antennas, by using higher-powered transmitters and, to some extent, by increasing the radio frequency at which the observations are made. The increases which are required may be achieved on the ground; there seems little need or hope for achieving them in a radar astronomy system both terminals of which are erected in space in the vicinity of the Earth.

The results which have been obtained so far, particularly on the properties of the Moon, have been directly useful to the NASA mission. It may be expected that the planetary results also will have a direct value. These were the considerations which led to the adoption of Recommendation 6.

Recommendation 6. Since ground-based radar astronomy studies of the Moon and planets form a natural part of the NASA mission, they should be vigorously supported by NASA.

SPACE RADAR ASTRONOMY

Although there is little to be gained in space radar astronomy by putting both the transmitter and receiver in space near the Earth, very considerable advantages arise if either or both is carried to the vicinity of the planet or other object being explored. A convenient distinction in radar may be made by calling "monostatic" the case where transmitter and receiver are close together, and "bistatic" when they are widely separated.

Monostatic Space Radar Astronomy

The power received in any monostatic radar system depends on the inverse fourth power of the range to the target. Thus for planetary targets, very large antennas and transmitters have been built on the Earth to bring the signal from the target above the receiver detection limit.

However, if both terminals of such a radar-astronomy system are placed near the planet to be studied, only quite modest powers and antenna sizes are needed to get valuable results. For example, a swept-frequency or multifrequency radar system, resembling the topside sounder Alouette but operating over a wider frequency range, would be an excellent instrument to be placed on a planetary orbiter. Such a device would detect the existence of an ionosphere and measure the electron distribution as a function of height. Such observations give valuable information concerning the density and nature of the atmospheric constituents. In addition, if the higher frequencies employed do penetrate the ionosphere then the same instrument gives a considerable amount of information about the nature and topography of the planetary surface.

Such experiments are of obvious value when applied to planets such as Venus and Jupiter. They have the additional advantage that if the radar equipment is properly designed, it can very simply be used to make radiometric measures of the surface as well. Although the Voyager program is still only in its early stages, from estimates of the weight and complexity of this type of equipment the suggestion is made that such a system could be well within the limits of Voyager's capability.

A Possible Specific Mission to Jupiter

One possible Voyager mission to Jupiter was discussed. The remarkable magnetosphere, ionosphere, and atmosphere of Jupiter is a very important, and possibly unique, field for study. A mission to do this, in addition to making measurements of the interplanetary plasma and magnetic field, should carry radar and radiometric probes, for example:

(1) A topside swept-frequency 1-20 MHz sounder.
(2) Downward-looking millimeter and decimeter-wave radiometer systems.
(3) A swept-frequency decameter-wave radio monitor.
(4) A very low-frequency radio receiver.
(5) Equipment for observing from the Earth occultations of the vehicle by the planet. Three radio frequencies in the 50-5000 MHz range should be chosen.
(6) Optical photometers probably in the H-α line, to observe possible night-side auroral activity.

Such a probe would be ideal as an orbiter in a near-polar orbit; a fly-by mission would be of some, but less, value.

These considerations led to Recommendation 7.

Recommendation 7. NASA should include in the Voyager series planetary orbiters containing a swept- or multifrequency radar system designed to detect and measure any planetary ionosphere and, from the radar scattering properties of the planetary surface, to derive information as to its nature and topography. Components of the same system can easily be used to make radiometric studies of the planetary surface and atmosphere.

In view of the great interest of the magnetosphere, ionosphere, and atmosphere of Jupiter, the first Voyager to approach that planet should carry a variety of radar and radiometric experiments designed to study these regions of the planet.

Bistatic Radar Experiments

The use of only one of the elements of the radar near the target also allows higher signal levels to be achieved in the system. It has an additional difference from the monostatic case, which is often of value, in that the scattering geometry changes from the simple back-scattering case. Thus information different from, and of greater variety than that of the monostatic experiment is obtained.

The close approach of one of the elements of the system to a planetary surface allows a very considerable gain in angular resolution, so that such experiments on planets could be expected to give results as good in resolution as those now achieved from the Earth on the Moon by range-Doppler techniques.

The same techniques as are used in bistatic radar can be applied in the case where the exploring probe is occulted by the planet. Observations of the phase and amplitude of signals from the probe can yield a complete profile of electron density in the planetary ionosphere and also give information on the density of the neutral atmosphere. At least two radio frequencies, with suitable modulation techniques, should be employed simultaneously in such measurements.

Such bistatic radar experiments can, if they employ two suitably chosen and modulated frequencies, yield information about the integrated electron density between the Earth and the probe and, in addition, can be used to refine trajectory information.

A similar refinement, useful for the control of terminal maneuvers, can be provided from the fact that both mono- and bistatic space radar systems give good measures of the distance of the probe from the planet.

So far, only relatively simple experiments of this kind have been conducted. The Mariner IV fly-by of Mars was a particularly successful example of the occultation type experiment. Much more information can be obtained from two frequencies. Experiments of the kind to fly on Pioneer out to 1 or 1.5 AU, in which phase-path and group-path are measured on two different frequencies, will measure the plasma electron density and its variations in interplanetary space with great precision.

These considerations led to Recommendation 8.

Recommendation 8. NASA should conduct, in the Voyager series, investigations of planetary surfaces and ionospheres by means of bistatic radar systems with one element of the radar on Earth and the other on the probe.

A similar type of experiment in which the probe, carrying either a transmitter or a receiver is sent on a trajectory such that the probe is occulted by the planet, is recommended as a powerful technique for the study of planetary atmospheres and ionospheres.

Lunar Orbiters

The surface of the Moon has already been well studied from the ground, and such studies, together with more direct observations which will certainly take place, have provided and will continue to provide much knowledge of the lunar surface. However, if monostatic or bistatic space radar systems are used to observe the Moon, they will first add considerably to our knowledge of the lunar reflection properties and second, perhaps more important, they can serve both as a test bed for equipment and as a source of ideas for the planetary exploration program.

These views are summarized in the following recommendation:

Recommendation 9. NASA should use lunar orbiter missions to test the instruments to be employed in planetary probes needed to conduct the experiments suggested in Recommendations 7 and 8, and also to add to our understanding of the nature of the lunar surface. Instruments for these missions could be possible payloads for the Apollo Extension Systems.

Special consideration was given to the problems and the future of solar studies. These have been referred to at various places in this part of the report and are summarized in the Appendix, Section 6. This paper was the result of a meeting at which members of the solar astronomy group, together with Dr. J. James from the M.I.T. El Campo station and Dr. K. Bowles from the Bureau of Standards, discussed the future requirements of this branch of the science. These discussions were led by Dr. A. Maxwell. As a result, Recommendation 1 (already quoted) includes the reference to the needs for solar observations in the RAE satellite series, and Recommendation 10 was agreed upon by the Working Group.

Recommendation 10. A substantial extension of existing radio and radar observations of the solar corona and interplanetary medium is recommended. This requires that large ground-based antenna systems for transmitting and receiving be built, together with the necessary high-power transmitting equipment. New advances in antenna and computer technology should be fully exploited.

This equipment would make solar radar and radio astronomy observations. Suitable interplanetary probes carrying multifrequency receiving and transmitting equipment should be launched so that observations of the signals propagated between the ground station and the probes can be used to study the solar corona and the interplanetary medium.

The ground-based equipment should be regarded as a national facility, and its construction should be given high priority so that observations may be commenced suring the coming sunspot maximum.

5. RELATED SUBJECTS

TECHNIQUES

During the work of the Group several items where technical advances are needed were discussed. They are collected here for convenience.

Long-wave antenna structures. The antennas for long-wave radio astronomy will fairly certainly be systems of light conductors extended over large distances. The problems of erection, stabilization, and pointing control clearly are not well defined or understood.

Antennas for 10 centimeters to a few meters. Although there is no immediate desire to see large antennas for this wavelength region in space, the studies for ground-based structures of 600 feet or more which are now starting might well indicate ways of building even larger space structures. Similarly, work on the general concepts of large steerable antennas in space could well assist the designing of the ground-based instruments. It would be well to try to establish a sensible boundary line between building on the ground or in space.

Millimeter-wave antennas. The main problems here would appear to be in the control of thermal differences, in achieving pointing control and in finding erection techniques capable of obtaining the high dimensional accuracy needed.

Novel antennas. Ideas by which resolution may be obtained without large size may, in some cases, be physically correct. Studies of such systems, which, for example, would include nonlinear elements, could be valuable.

Radiometer techniques in the millimeter-wave region. Thermal detection radiometers, utilizing the germanium bolometer or one of several other possible low-temperature devices, provide high sensitivity and broad bandwidth at millimeter and submillimeter wavelengths. Further improvements in existing designs for ground-based observations and adaptations to the requirements of space telescopes should be undertaken. In addition to the broadband radiometry, it will be necessary to have spectrometers that can provide a wide range in resolution and spectral coverage. Furthermore, the need to obtain the most information possible from the spectrum at $\lambda 0.02$ mm to 2.0 mm will require the development of spaceborne cryostats which maintain temperatures as low as 0.25°K.

THE SIZE OF NASA GROUND-BASED ANTENNA SYSTEMS

Although this subject is clearly not within the scope of this Working Group, the optimum size of NASA ground antenna facilities for transmission of commands and reception of telemetered data was considered. The data rates and signal levels available with the currently available or planned antennas are low, and especially low for planetary missions. The cost of the entire set of missions is large. It appears probable that in future missions the gains from using larger ground-based antenna systems would outweigh the relatively small extra cost.

This led to the following recommendation:

Recommendation 11. The Working Group recommends that NASA devote a much larger fraction of its resources to the construction of ground-based deep-space telecommunication terminals. The object is to increase the amount of information that will be returned from solar and planetary spacecraft now proposed.

THE APOLLO EXTENSION SYSTEMS (AES)

It will be evident from the report that, starting from a consideration of the scientific needs, the Group has suggested several projects in radio and radar astronomy. None of these can be defined closely enough to know whether it would fit with the Apollo Extension Systems, but it seems possible that the antennas proposed for long-wave work (Recommendation 3) and for millimeter-wave work (Recommendation 5) are of the size which would need AES. Similarly, lunar orbit missions in AES could with value carry the space radar astronomy experiments suggested in Recommendation 4.

There are useful millimeter-wave experiments that could be done before the construction of a large dish, which would need collecting areas only 10 or 20 square feet in size, capable of working down to 20 microns. These experiments, though of priority below those suitable for the large millimeter-wave dish or the long-wave antenna, would nevertheless be valuable.

LUNAR EXPLORATION SYSTEM AFTER APOLLO (LESA)

The Group did not find it possible to come to any significant conclusions on LESA. From a strictly scientific point of view, despite the possible attraction of the back of the Moon as an interference-free site, all the future plans that the Group considered appeared to be more easily and better done in orbit than on the Moon.

Another point of view is possible: if experiments are to be conducted from the Moon, what should be done there? When read from this point of view, the LESA reports were less strongly criticized; some of the Group nevertheless considered that the reports underestimated the size of antennas needed for long-wave radio astronomy. The reports also did not give enough consideration to the importance of radio-astronomy observations in the wavelength range below 1 mm.

The present report of the Group may be of some help in developing the LESA plans. The Group has tried to say what should be done. If in fact for other reasons any of the experimental objectives that the Group has suggested could be more easily met by a radio observatory on the Moon, then the choice is easy and obvious. But if there are no other reasons which press for the use of the Moon as a radio-astronomical base, the Group would wish the main effort to go into designing the experiments to be put in the most suitable orbits. Studies of the lunar environment for its mechanical suitability should be made. Similarly, whether or not it is required for radio astronomy the control of the use of the radio spectrum should be well planned and well executed. This, of course, we recognize, after seeing the situation on Earth, as a task which is certainly beyond NASA and perhaps beyond the abilities of the human race.

APPENDIX 1: LIST OF PARTICIPANTS

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APPENDIX 2: SOLAR RADIO AND RADAR ASTRONOMY

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NEW NATIONAL FACILITIES

The technology is at hand to mount a major program for combined active and passive radio-wave probing and observation of the solar corona and interplanetary medium, from the lower corona to well beyond the orbit of the Earth.

There would be four principal categories of investigation in such a program. They are based on:

(i) Ground-based solar radar, with separated transmitting and receiving installations.
(ii) Ground-to-space and space-to-ground transmission experiments utilizing various solar and deep space vehicles.
(iii) Sweep-frequency observations of solar radio noise bursts.
(iv) Observations of the scintillations of cosmic and space-probe radio sources caused by plasma irregularities in the solar wind.

Various research groups have already expressed interest in substantial expansion of programs in each of these fields. It is suggested that a major effort should now be made, possibly leading to construction of new national facilities, to provide extensive new ground-based equipment for these programs. This should be used cooperatively by various university and government research groups. Such a combination of programs is important on both scientific and practical grounds, for the following reasons:

(i) The scientific objectives of these four areas are intimately related, and this combination of observations would represent a closely coordinated study of quiet and active solar and interplanetary phenomena of a kind that has not been possible heretofore.
(ii) There is very substantial overlap of equipment needs, particularly for the very large and costly antennas and powerful transmitters.

SOLAR RADAR

The feasibility of using ground-based radar as a new technique to study the Sun has been conclusively demonstrated. Routine observations since 1961 have produced data on the distribution in range and Doppler frequency of returned energy from the echoing regions in the solar corona.

The radar Sun is highly variable from day to day, with its long-term average cross section being closely correlated with sunspot number.

Range-Doppler information has been interpreted in terms of large mass motions in the corona, and a general outward flow of gas whose velocity increases with distance from the photosphere.

Present studies at 38 MHz at El Campo, Texas, are severely hampered by limitations in system sensitivity (only one out of every four 16-minute echoes contains sufficient energy to make a range-Doppler display), antenna beam steerability (only one measurement is obtained per day), and angular resolution (the radar Sun is not resolved in angle).

Considerably more system sensitivity (15 to 20 dB), beam steerability (sufficient to track the Sun for about 6 hours daily), and angular resolution (about 10 min of arc) are required to realize the potentialities of radar for detailed studies of the quiet and active Sun in the echoing regions from perhaps 1.2 to 3 solar radii. A transmitting site for floodlighting the Sun, and receiving antenna array at a separate site for angular resolution, would be required. Such an upgraded system will require no new technology.

PROPAGATION PROBES (BISTATIC RADAR EXPERIMENTS)

Radio-wave propagation between the Earth and space vehicles provides a method of making sensitive observations of the radial distribution, irregularities, and dynamics of the interplanetary plasma, from the inner corona out to many AU. Current and prospective programs incorporating this type of experiment include the Pioneer and Sunblazer series of spacecraft, and the Voyager series of planetary spacecraft. In the relatively modest Sunblazer project, pulsed transmissions from the spacecraft to the ground are employed, while the first Pioneer propagation experiment provides for powerful ground transmissions to be received in the spacecraft where the measurement results are encoded onto the telemetry channel. Each approach has its advantages and disadvantages, and it is not yet clear which will be favored for more advanced missions.

Ground-based equipment is at hand for the first Pioneer experiments, and relatively inexpensive phased arrays are proposed for the first Sunblazer. But for future missions designed for more detailed measurements, and for experiments conducted over considerably greater distances, larger antennas and more powerful transmitters will be required. Major components of the ground-based facilities suggested above for advanced solar radar observations could be used to fill this need.

Recommendation. The Committee recommends a substantial extension in existing radio and radar observations of the Sun. This will necessitate the construction of large, new, versatile instruments, exploiting to the full recent advances in antenna techniques and computer technology. Such new equipment should be regarded as a national facility, and its construction should be given high priority, so that observations may be started during the coming sunspot maximum.

SOLAR RADIO EXPLORER SATELLITES

Observations of the dynamic spectra of solar radio bursts should be extended to wavelengths beyond the long-wave limit imposed by the terrestrial ionosphere. The immediate observational requirements in this spectral region (approximately 0.25 - 16 MHz) can be met with a satellite of the Explorer size. The survey character of these observations suggests that a series of several satellites will be required to provide proper coverage during the solar cycle.

Recommendation. This Committee recommends that a series of Solar Radio Astronomy Explorers be flown during the coming sunspot maximum, to provide data on low-frequency characteristics of solar radio bursts.

V

X-Ray and Gamma-Ray Astronomy

1. INTRODUCTION AND SUMMARY OF RECOMMENDATIONS

X-RAY AND GAMMA-RAY ASTRONOMY: AN INTRODUCTION

If one rates scientific discoveries by the novelty of the phenomena they disclose, then certainly the observation of strong x-ray sources beyond the solar system has been the outstanding discovery of space astronomy to date. In three years, the observations have progressed from the first evidence of a localized flux to the detection of about a dozen discrete sources. It took radio astronomy a dozen years to progress from Jansky's original discovery to the detection of the radio source Cygnus A.

The x-ray flux in the 1 to 10 Å region from the strongest of the x-ray sources is about one-tenth of that from the quiet Sun. The fact that none of the celestial phenomena known previously had led astrophysicists to predict the existence of x-ray sources even remotely approaching the strength of those observed justifies the expectation that x-ray astronomy will play a fundamental role in advancing our understanding of the Universe.

Most, if not all, of the observed x-ray sources appear to be within the galaxy. Present plans to extend the observations include the use of larger rockets and balloons which will carry larger and improved detectors, and satellites which will provide longer observing times. Both approaches will almost certainly bring a large number of weaker galactic sources within range of study and permit the observation of strong extragalactic sources. A diffuse x-ray flux, which may be the integrated effect of the contributions of all external galaxies, has already been observed. Every ingredient exists in the young field of x-ray astronomy to guarantee its development in a manner that may soon lead to results comparable in importance to those of radio astronomy.

The future of gamma-ray astronomy (above 1 MeV) is not as clearly defined as x-ray astronomy at present, because as yet no cosmic gamma-ray sources have been detected with any certainty. The first observations of gamma rays from space vehicles have been sensitive enough to discover surprises of the magnitude revealed by x-ray astronomy, but similar discoveries have not turned up. There is some evidence from satellite observations, however, for the existence of a diffuse gamma-radiation background from space. Radio-astronomical data provide almost certain evidence that discrete sources of gamma rays exist in the 100-MeV range. Current theories of nucleogenesis indicate that there should be significant amounts of gamma line emission from the decay of heavy radioactive elements, even in a supernova remnant as old as the Crab Nebula. But the gamma-ray flux from these processes is estimated to be two or more orders of magnitude less than would be detectable by instruments already flown or scheduled for flight. The fundamental knowledge to be gained from studies of gamma-ray processes is so important that every effort must be directed toward achieving the necessary levels of detection sensitivity.

Astronomy reveals a host of phenomena in which nature provides remarkably powerful mechanisms for the acceleration of elementary particles to relativistic velocities. Through x-ray and gamma-ray astronomy we have an indirect means of detecting the existence of these energetic particles and unraveling the processes that produce them. The charged particles themselves travel through vast regions of space but their directions are so altered by collisions with magnetic fields and matter that all trace of their origin is destroyed. On the other hand, the x-rays and gamma-rays that have their origins in the same primary events travel in straight lines from source to observer and may survive billions of years without deflection.

X-ray and gamma-ray astronomy provide tools for probing many of the most fundamental problems of cosmology. These include the mystery of the origin of cosmic rays, the density of cosmic rays in galaxies and in the Universe, the strength of galactic and intergalactic magnetic fields, the hypothesis of continuous creation of matter, and the temperature, density, and composition of galactic and intergalactic matter. Already x-ray astronomy brings into question some of our fundamental concepts of stellar evolution leading to the collapse of stars and the supernova catastrophe.

Before presenting any specific conclusions or recommendations, the Working Group wishes to emphasize its strong feeling that x-ray and gamma-ray astronomy must be assigned a priority comparable with that accorded older established fields of space astronomy. Surely, no one will deny that the exploration of an entirely new field is likely to produce scientific results as important, to say the least, as the refinement and extension of observations in established fields. Thus, the fact that NASA has very appropriately committed large resources to the needs of the classical branches of astronomy must not prevent it from providing adequate support to a new branch of observational astronomy, as soon as its potential value becomes established.

OBJECTIVES OF OBSERVATIONAL X-RAY ASTRONOMY

With regard to x-ray astronomy, the following problems are singled out as being both of great scientific significance and accessible to experimental investigations, now or in the near future (the order of listing does not reflect their relative importance):

(i) search for weaker discrete sources (10^{-3} to 10^{-6} times the strength of those observed so far);
(ii) precise determination (to 1 min or better) of the location of the discrete sources in order to make possible identification of the x-ray sources with optical or radio objects;
(iii) study of the structure of the discrete x-ray sources with a resolution of better than 5 sec, or establishing an upper limit of this order of magnitude for their size;
(iv) study of the spectral distribution of the radiation from the various discrete sources; search for emission lines, absorption edges, and the long wavelength cutoff expected from interstellar absorption;
(v) search for polarization of the x radiation from discrete sources;
(vi) directional and spectral study of the diffuse radiation, with the aim of establishing its galactic or extragalactic origin, and investigation of the properties of the media in which it arises and through which it passes;
(vii) search for time variation of both long and short duration in the intensity of the discrete sources.

OBJECTIVES OF OBSERVATIONAL GAMMA-RAY ASTRONOMY

With regard to gamma-ray astronomy, the less certain nature of the observations to date suggests a somewhat less specific set of problems:

(i) study of the directional and spectral characteristics of the diffuse gamma-ray flux in both the 1-MeV and >100-MeV regions, with the aim of establishing its real intensity (as distinguished from an upper limit), its origin (whether galactic or extragalactic, or both), and production mechanisms;
(ii) study of the gamma-ray flux from strong radio sources with instruments of vastly improved sensitivity and angular resolution;
(iii) study of nuclear gamma rays from supernova remnants, such as the Crab Nebula, with instruments capable of improved background rejection.

EXPERIMENTAL APPARATUS AND TECHNIQUES

The Working Group discussed the experimental techniques available or under development, in terms of their promise for future observations. The following conclusions emerged:

(i) X-ray detectors, provided with mechanical collimators (the only type of instrument used so far for nonsolar x rays), will continue to play an important role in the investigation of most of the problems mentioned above, especially if the detector areas can be substantially increased. An active research program using detectors of this type, carried aloft by balloons, rockets, unmanned satellites, and manned space vehicles, should be strongly encouraged. Controls for stabilization and orientation must be developed to the limits of present technology.

(ii) Occultation methods have provided some of the most definitive measurements of source structures in radio astronomy. The only identification of an x-ray source with an optical or radio object has been made utilizing this method. Lunar occultations will continue to provide fine resolution of size and accurate positional data, and for these measurements large area detectors are essential. Such detectors could be employed effectively on the lunar surface or on a lunar orbiter.

(iii) Total-reflection telescopes, similar to those already successfully employed for taking x-ray pictures of the Sun, offer great promise for nonsolar x-ray astronomy. Existing telescopes carried by rockets could photograph the strongest discrete sources, if the rockets were kept pointed in the right direction with an accuracy of 1 min of arc for periods of the order of one minute of time. Larger telescopes, such as could be accommodated aboard unmanned or manned satellites, appear to be the only tools capable of many of the refined observations that will be needed beyond the early exploratory stage. The angular resolution provided by x-ray telescopes (about 5 sec of arc) is greatly superior to that obtainable with mechanical collimators. Telescopes used as concentrators afford the possibility of polarization experiments and of spectral measurements by means of dispersive techniques that would not be feasible otherwise. Moreover, telescopes will be competitive with large area detectors for the discovery of very weak sources. Therefore, a program of x-ray astronomy using total-reflection telescopes should be started at the earliest possible time and pushed vigorously to exploit its ultimate capabilities.

(iv) Studies of both diffuse and point sources of the 100-keV to 10-MeV range will require the continued development of actively shielded collimated detectors, as well as the development of techniques using Compton coincidence telescopes or pair spectrometers. Since this is an energy region where line emission is expected, energy resolution of high order will be required. Solid-state detectors should prove useful. Initial observations and background studies may be made with balloons. However, sky surveys at sensitivities of possibly 10^{-5} cm^{-2} sec^{-1} and employing detection systems with angular resolution of about 1° will require the long exposure time available only from a satellite.

(v) Detection of more energetic gamma rays from discrete sources will require instruments of large collection factor (i.e., area exposure time), angular resolutions of better than 1°, and, *most important*, a *proven* capability for background rejection. Both spark chambers of wide angular sensitivity (but with 1° resolution or better), and gas Čerenkov detectors of narrow angular sensitivity, both merit and require development. Above 300 GeV, the radiation can be detected with ground-based instruments.

(vi) Instruments of the type discussed under (v) above may, if their background rejection properties are adequate, be suitable for further clarification of the diffuse gamma-ray problem. Probably more suitable, however, are instruments especially designed to detect diffuse radiation. Proposals should be judged on the basis of their promise for achieving significant increases in sensitivity beyond that of instruments already flown, or now being prepared for flight.

VEHICLES

The Working Group finds that the implementation of this program will require the full utilization of currently available vehicles, as well as the development of new vehicles with special characteristics. Specific needs have been found for the following:

Balloons

Balloons will play an essential role in observations in the energy range above 15 keV. In addition, they will be necessary for testing prototype instruments designed for x-ray and gamma-ray observations from satellites. In order to carry out the present and foreseeable objectives, four recommendations are made:

Recommendation 1. A substantial increase in the number of balloon flights should be authorized for x-ray and gamma-ray astronomy. Considering the number of investigators who are presently carrying out or planning balloon experiments in this field, the Group foresees a need for at least 40 balloon flights per year.

Recommendation 2. Two new types of reliable balloons are needed. One type would be capable of lifting 250 lb of scientific payload to an altitude in excess of 145,000 ft, while the other would be capable of lifting payloads of 2,000 lb to 130,000 ft. Attainment of these objectives will require fundamental advances in balloon technology. The developmental program should be the responsibility of an experienced group of balloon scientists and engineers, preferably in the National Center for Atmospheric Research (NCAR).

Recommendation 3. A program should be funded for the engineering development and construction of a prototype of a controllable star-guided orientation system, with an accuracy better than 1 min of arc.

Recommendation 4. To meet the present demands for balloon flights for astronomical and other purposes, as well as to accommodate the expected increase in these demands, the physical plant and personnel of the NCAR balloon launching facilities should be expanded, as appropriate.

Rockets

Rockets will continue to provide a means for performing experiments complementary to those carried out from satellites, particularly exploratory experiments, as well as providing the experience for successful performance of satellite experiments. In particular, the following recommendations are offered:

Recommendation 5. A twofold increase in the number of rocket flights (from the present number of about 6, to about 12 per year) is required for x-ray astronomy. This will provide for new groups entering the field and will facilitate accumulation of data at a proper rate.

Recommendation 6. Weight-lifting capacity and volume in excess of those provided by the Aerobee vehicle are required for certain applications.

Recommendation 7. Pointing accuracies considerably finer than those now available are essential to take full advantage of the inherent precision of existing x-ray optics. An accuracy of 15 sec of arc to 1 min of arc is required, with small drift or jitter rates. While developmental pointing systems with the desired characteristics have been flown, they are not available to experimenters at this time. Pointing jitter should eventually be reduced to 5 sec of arc, to match the resolving power of focusing x-ray optics. This aspect of rocket technology should therefore be pursued with particular emphasis.

Satellites

X-ray astronomy has advanced to the point where x-ray Explorer satellites and a substantial fraction or all of an Orbiting Astronomical Observatory (OAO) are justified and needed. The volume and payload capacity of the Apollo Extension Systems (AES), with the potential advantage of participation by a man, can also be fully utilized for x-ray and gamma-ray astronomy. The following recommendations reflect some of these considerations:

Recommendation 8. Proposals for x-ray experiments suitable for inclusion as primary OAO experiments should be considered as possible substitutes for already accepted OAO experiments in the more conventional fields of astronomy.

Recommendation 9. The first unassigned OAO should be set aside for x-ray observations, to be supplemented if possible by much lower priority gamma-ray observations.

Recommendation 10. The OAO pickaback opportunities should be recalled to the attention of the scientific community and suitable experiments in x-ray or gamma-ray astronomy should be accommodated. If at all possible, spacecraft components should be rearranged in minor ways in order to permit use of optical focusing devices with focal lengths comparable to the length of the OAO.

Recommendation 11. Experiments utilizing focusing x-ray optics now at hand should be flown on pointed rockets and satellites currently available, or being developed.

Recommendation 12. Plans should be begun for orbiting an x-ray telescope of greater focal length than can be accommodated in present satellites. Lengths in the range of 30 ft to 100 ft are considered necessary to achieve the desired sensitivity and resolution. Unfolding of an extensible system in space may be possible.

SUPPORTING TECHNOLOGY FOR X-RAY INVESTIGATIONS

X-ray astronomy involves techniques of a very specialized nature which have been developed rather slowly by a handful of physicists, working in only a few laboratories, largely for the purposes of solid-state spectroscopy and crystallography. An urgent need exists for the development of standards of absolute photometry, including monochromatic and continuous sources, detectors, crystal and ruled grating analyzers, reflectors and filters, and adaptation of light converters and image amplifiers to x-ray imaging.

Recommendation 13. It is recommended that a strong program in the technology of soft x-rays be implemented under the management of one of the NASA research centers.

GAMMA-RAY ASTRONOMY

Energies in the Range 0.1 - 30 MeV

Experiments in the next few years over the energy range 0.1 - 30 MeV will continue to be of an exploratory nature, designed to search for point sources and gamma-ray lines, and to study the diffuse component. Important observational attempts, particularly on known x-ray or radio sources, will soon be made from balloons. Experiments designed to study the diffuse component or to accomplish a sky survey must be conducted from a vehicle with minimum capabilities at least equal to those provided by Explorer-class satellites. With the foregoing considerations in mind, the Working Group makes the following recommendations:

Recommendation 14. Continued support should be given to the development of detection devices using actively shielded collimators or Compton scattering telescopes, as well as to the development of promising new techniques.

Recommendation 15. Space flight assignment should be given only to instruments which indicate clear improvement in background reduction, energy resolution, or angular definition, and which have been proven on balloon investigations.

Recommendation 16. Greater support should be given to balloon programs as well as Explorer-class satellites, to implement observations in this energy range.

Recommendation 17. Support should be provided for the continued study of solar x-ray and gamma-ray spectra during both quiet and active phases of the solar cycle.

Energies Greater than 100 MeV

No flux of photons with energy greater than 100 MeV has been definitely detected. Because of the low photon intensity and large background radiation, only upper limits to the flux have been determined. In addition, the ratio of the radiation from point sources to the diffuse radiation may be very small, making the detection of point sources all the more difficult.

With respect to the experimental aspects of high-energy gamma-ray astronomy, the Working Group makes the following recommendations:

Recommendation 18. Instruments with large collection factors (area x exposure time) should be developed which provide angular resolutions of better than 1°, which can determine the energy spectrum, and which possess a proven ability to reject background radiation.

Recommendation 19. Experiments should be undertaken in balloons to search for point sources and to define the design requirements for satellites.

Recommendation 20. Since measurement of the diffuse radiation flux would be a significant experiment and could be performed in the immediate future with small Explorer-type satellites, it should be attempted.

Recommendation 21. Since experiments to study spectral composition and directional structure of the diffuse flux and to measure weak fluxes from and spectra of discrete sources will ultimately need a large orbiting vehicle, one of the forthcoming large orbiting vehicles should be scheduled for this purpose.

2. CURRENT STATE OF THE OBSERVATIONS

X-RAY ASTRONOMY

About a dozen discrete x-ray sources (0.5-15 Å) have been detected thus far with rocketborne instruments. Half of these appear to be relatively isolated and fully resolved; the rest are clustered more densely and may be further resolved with slightly improved observations. The most remarkable feature of the distribution is a concentration toward the galactic plane, which implies that the sources are galactic objects. Six of the observed sources lie within only ±20° of longitude from the galactic center and have a range of only a factor of 2 in brightness. One possible conclusion is that they are at roughly similar distances from the Sun and possibly in the general vicinity of the galactic center. The brightest source lies in Scorpius, and is farthest from the galactic equator, at about +25° of galactic latitude. It is 10 to 20 times as bright as those clustered toward the galactic center. Figure 1 summarizes the observational information obtained to date.

Only one source, the Crab Nebula, has been positively identified with an optical or radio source. By observation of a lunar occultation, the

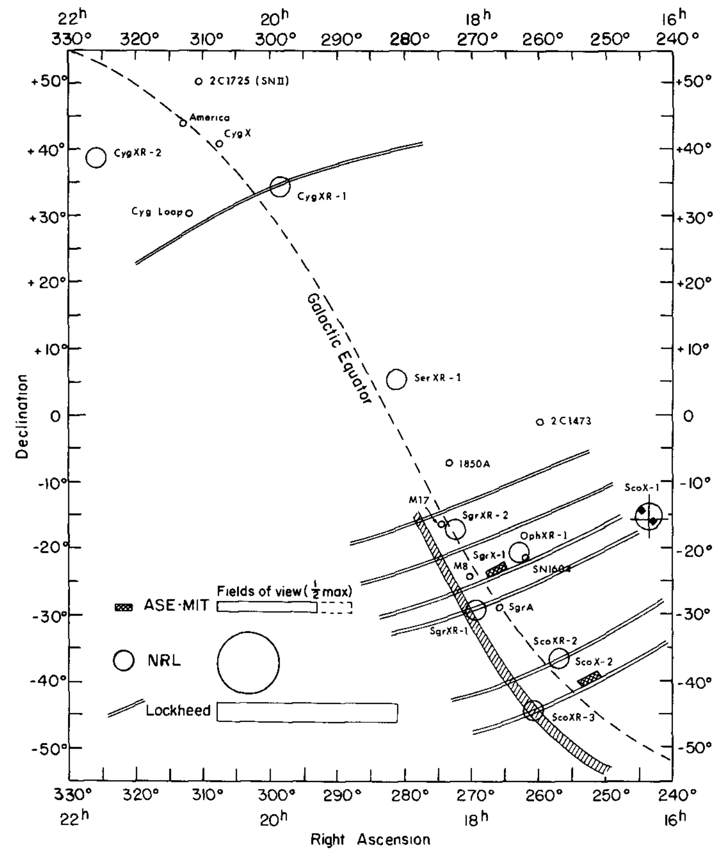

Figure 1. Summary of observational x-ray data to date. Source definition is indicated by the size of the circle or rectangle. The scan paths represent source observations in one dimension observed by the Lockheed group from a pointed and slowly rotating rocket. The proximity of the resolved sources to the galactic plane is clearly evident. (Courtesy of American Science & Engineering, Inc.)

x-ray emission was found to be associated with a region about one light year in diameter, approximately centered in the optical nebula. Other source positions are known within limits varying from ±15' to about 1.5°. Within the estimated position errors and sensitivity limits of the measurements, it appears that the radio center of the Galaxy is not an x-ray source. Kepler's supernova lies close to an observed source but at an angle somewhat larger than the estimated uncertainty in the position of the source. The strongest source, in Scorpius, has been located to within ±15'.

There appear to be at least two classes of sources: those accompanied by radio and optical emission, and those which remain as yet unidentified with any radio or optical object. The positive identification of an x-ray source with the Crab Nebula has suggested that some x-ray sources are supernova remnants. Nevertheless, Cassopeia A, the strongest radio source, believed to be a supernova of Type II, has been scanned with high sensitivity, but no x rays have been detected. The Tycho Brahe 1572 supernova of Type I has also been well scanned with only negative results. Conversely comparison of the observed x-ray positions with ancient records of visible supernova events of the past 2000 years reveals no correlations other than that with the Crab Nebula.

With the exception of the Crab Nebula, whose x-ray emission region has been defined by the occultation observation, information has been gathered only on the upper limit of the size of the x-ray sources. The observations thus far have been made with large area detectors and mechanical collimation devices, rather than with reflecting and focusing x-ray optics. The smallest field of view in the scan direction has been approximately 2°. With each improvement in resolution, the observed sources have been found still to lie inside the instrumental resolution. A modulation collimator has been successfully employed to observe the brightest Scorpius source and has shown its angular extent to be smaller than 7 min of arc in at least one dimension.

Spectral information is still very meager, but there are substantial differences in the spectral distributions of the sources thus far observed. Observations have been made with Geiger counters, proportional counters, and scintillation counters; and filter photometry has been applied in combination with choices of gas fillings to control the broadband spectral efficiencies of gaseous detectors. From such rough samplings of spectral bands in the 0.5 to 15 Å range, it seems evident that the emission of most of the individual sources cannot be fitted to any simple spectrum. It is likely that nonthermal and thermal processes occur in the same object and that nonuniformities exist in physical structure, composition, and temperature through its volume. In the case of the Crab Nebula, however, a fairly coherent explanation of the entire spectrum from radio observations to the shortest wavelength balloon observation (about 0.2 Å), can be constructed on the basis of magnetic bremsstrahlung. One is, nevertheless, left with the problem of explaining the persistence of x-ray emission for 1000 years when the lifetime of the radiating electrons is of the order of only years or even months.

Observations by means of detectors with different fields of view indicate a diffuse x-ray flux amounting to about 1 photon cm^{-2} sec^{-1} $ster^{-1}$ in the 1.5 - 8 Å range. The estimate of the flux is based on the difference between responses of the detectors when looking in the upward direction and when looking downward. It is not absolutely clear that this flux arrives from outside the Van Allen belt regions, or that it is extragalactic rather than the integrated effect of numerous sources within the Galaxy too weak for individual detection. Certain bits of evidence, however, favor the extragalactic hypothesis. No indication of horizon brightening, as would be expected from a nearby atmospheric source has been found in flights from the White Sands Missile Range. If the diffuse flux were galactic, it would be stronger toward the Milky Way, but no such concentration was found. It has also been proposed that the diffuse flux is the net contribution of all galaxies, each radiating from collections of discrete sources similar to those in the local Galaxy. The intensity of the observed diffuse flux is roughly what would be expected in this hypothesis.

X-RAY ASTRONOMY AND GROUND-BASED ASTRONOMY

An optical search to 23rd magnitude has been made in an area 40 min of arc by 50 min of arc around the position of the brightest x-ray source in Scorpius. There is nothing obviously identifiable with the x-ray source unless it looks like a faint galaxy or a star. The presence of about 140 galaxies per square degree to 23rd magnitude suggests that the source is not likely to be completely obscured by interstellar dust, and that the average extinction may be about two magnitudes. The possibility that a star or galaxy in the field is indeed the x-ray source cannot be excluded until an excellent x-ray position (to better than 1 min of arc) has been determined, or until the object is shown to be extended by more than 2 sec of arc. On the other hand, a coincidence of x-ray and optical positions within 1 min of arc, together with a 2 sec of arc upper limit on the diameter, would be convincing. When positions for x-ray objects are accurate to a few minutes, a search should be attempted with the aim of making such optical and radio identification.

The problem of the diffuse x-ray flux is also related to optical and radio astronomy, especially if the attenuation of this flux by the interstellar gas of our Galaxy and of other galaxies can be observed. The radio data concerning neutral hydrogen in our Galaxy already offer a detailed basis for comparison.

GAMMA-RAY ASTRONOMY

The presently available data on the total incoming radiation per unit solid angle, although purporting in some cases to represent actual intensity measurements, can safely be interpreted above 8 keV only as upper limits. These data, from x-ray energies up to 100 MeV, are shown in Figure 2. The line in the figure represents an empirical inverse-square law for the differential number spectrum; the corresponding integral spectrum (the total flux above energy E) would be given by $J(>E) = 0.02/E$ (MeV) cm^{-2} sec^{-1} $ster^{-1}$.

At an energy near 10^9 eV other points (again an upper limit of the gamma-ray flux) have been provided by the frequency of cosmic-ray air showers which lack the usual muon component. The integral frequency at this energy is about 7×10^{-13} photons cm^{-2} sec^{-1} $ster^{-1}$, or one per

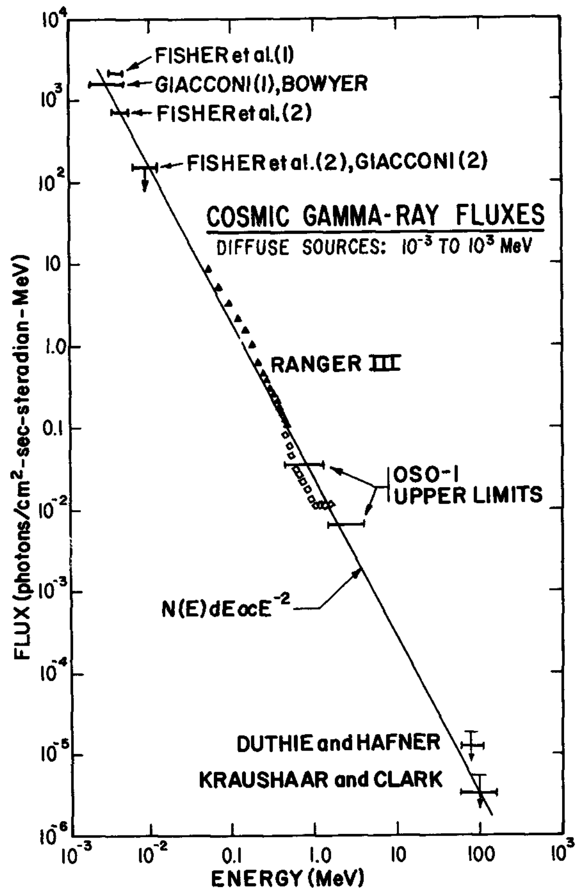

Figure 2. The present state of knowledge of the diffuse cosmic photon spectrum. Numbers in parentheses after names refer to first and second rocket observations. (Based on graph furnished by L.E. Peterson, University of California, San Diego.)

3500 charged cosmic rays of the same energy. If one extrapolates the line in Figure 2 following a curve generally parallel to the primary cosmic-ray spectrum, the curve should intersect this point.

No directional variation has yet been detected in any of the radiation described above; the curve should be taken as an indication of the possible total flux averaged over all directions, in line and continuous emissions from both point and diffuse sources.

Upper limits have also been obtained for the intensities in two particularly significant gamma-ray lines: 10^{-3} cm^{-2} sec^{-1} $ster^{-1}$ at 0.5 MeV (representing positron-electron annihilation) and 4 x 10^{-4} cm^{-2} sec^{-1} $ster^{-1}$ at 2.2 MeV (representing neutron-proton capture to form deuterium).

Directional detectors have been used to search for point sources in various energy bands. At 100 MeV, many suspected sources have been scanned with sensitivities in the range 10^{-3} to 10^{-4} cm^{-2} sec^{-1}, all with negative results. At 1000 MeV, a substantial portion of the northern sky (including Cygnus A, Cassiopeia A, 3C48, and 3C47) has been scanned at a sensitivity somewhat better than 10^{-4} cm^{-2} sec^{-1}. And at 10^{7} MeV, the four strongest radio emitters, including Cygnus A and the Crab Nebula, have been scanned at a sensitivity approaching 10^{-11} cm^{-2} sec^{-1}. All of these experiments have been negative and provide only upper limits of the intensity, although a possible flux has been reported by the Harwell-Dublin group. Further experiments now in preparation will at least refine the upper limits by one or two orders of magnitude, if they do not succeed in detecting definite amounts of radiation.

Even at present levels of sensitivity, the established upper limits of intensity have permitted significant cosmological and astrophysical conclusions. It has been noted that in the x-ray region, the diffuse flux is for X below that predicted to originate in the intergalactic medium by the hot universe model and hence permits the discarding of this model. Similarly, the weak intensity of the 0.5 MeV line and at 100 MeV rule out the hypothesis of a universe containing equal amounts of matter and antimatter. Indeed, the latter data also provide a significant limit to the product of gas density and ordinary cosmic-ray density in intergalactic space, and determine that only an extremely small fraction of the cosmic rays can be antiparticles. The failure to detect 10^7 -MeV photons from the Crab Nebula rules out the model in which the high-energy electrons are secondary to a hundredfold richer, nonradiating proton complement. Other examples can be given. The important thing to note, however, is that even slight refinements in the data, whether they lead to definite intensities or only reduce the upper limits, will strengthen conclusions like those mentioned above, and permit further inferences about the acceptability of various theories of nucleosynthesis, stellar collapse, supernova explosions, stellar and galactic evolution, and the nature of the Universe on the grandest possible scale.

A SAMPLE SPECTRUM

Figure 3 summarizes, by way of illustrating the foregoing statements, the present experimental information on the electromagnetic spectrum of the Crab Nebula; the higher frequency points in this figure all being upper limits. Several features can be noted in this figure. One is the tremendous range spanned both in frequency of the radiation and in counting rate. Another is the way in which many widely diverging techniques complement each other in providing the complete picture. A third is that the series of points which indicate the upper limit of the flux at high frequency connect smoothly with the definite measurements in the x-ray range and below, a fact suggesting that a rather modest increase in sensitivity may convert some of these upper limits to definite points on the spectrum.

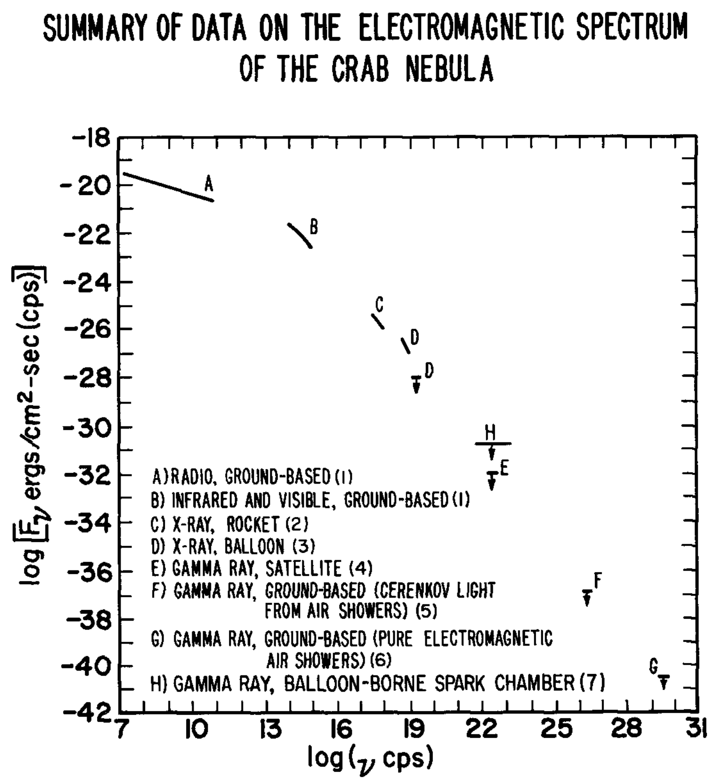

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Figure 3. Summary of data on the electromagnetic spectrum of the Crab Nebula.

3. THEORETICAL CONSIDERATIONS

THEORIES OF THE ORIGIN OF COSMIC X RADIATION AND GAMMA RADIATION

A discussion of theories for the production of cosmic x radiation and gamma radiation immediately provides insight into the cosmological significance of the field of x-ray and gamma-ray astronomy. Present ideas concerning possible production mechanisms play an important role in the planning of future experiments and in the design of instruments for observations. Consequently some of the more tenable ideas are presented here.

At the present level of knowledge, only five conclusions concerning sources of cosmic x rays can be drawn from the experimental observations:

(i) Discrete sources exist in the Galaxy.

(ii) At least some sources of small angular diameter exist in the Galaxy.

(iii) Different sources show major spectral differences.

(iv) The spectral distribution of at least one source (the brightest source in Scorpius) cannot be described by a single parameter.

(v) A diffuse and nearly isotropic flux of x rays exists.

In the discussion of possible x-ray production mechanisms in discrete sources, we must be aware that several different mechanisms may be simultaneously at work. Unique spectral properties of different source mechanisms are suggested below.

The picture is much less clear with regard to the production of cosmic gamma rays, for thus far no discrete sources have been observed, and only upper limits on the possible intensity of the diffuse flux of high-energy photons have been placed from experimental observations. It is nevertheless widely believed that both discrete sources and a diffuse flux of high-energy gamma radiation must exist. Theories for the production of these gammas consequently are also presented.

MECHANISMS OF X-RAY PRODUCTION

There are a number of physical processes by which photons of x-ray energies can be produced. The most likely mechanisms are:

(i) Magnetic bremsstrahlung (also commonly known as synchrotron radiation);

(ii) Collisional bremsstrahlung, recombination radiation and line emission;

(iii) Compton scattering; and

(iv) Blackbody emission.

Magnetic Bremsstrahlung

The form of radiation known as magnetic bremsstrahlung results from the deflection of electrons and positrons in the local magnetic field. Radio and visible synchrotron spectra can usually be approximated by a power law. The radiation emitted by relativistic electrons by this mechanism is linearly polarized.

In the event that optical and radio synchrotron emission is observed from a discrete x-ray source (e.g., the Crab Nebula), then the near constancy or slow change of the spectral index would be an argument in favor of the synchrotron emission mechanism in the source. But the lack of such a constancy would not necessarily be an argument against such a source mechanism, since a depletion of high-energy electrons in the source is to be expected. Interstellar absorption may also affect the observed spectrum. Observation of the linear polarization of the x rays would provide support for their synchrotron origin.

Perhaps the greatest difficulty in interpreting the x radiation from the Crab Nebula as synchrotron emission is the short lifetime (order of 30 years) expected for the 10^{14} eV electrons needed to produce 10 keV x rays in a magnetic field assumed to average 10^{-4} gauss over the dimensions of the source. It is possible, however, that local magnetic field variations of two to three orders of magnitude exist within the nebula, and that the x rays are produced by lower-energy electrons wandering into these internal magnetic fields. The electrons could thus survive for very long times before radiating. Suggestions have also been made concerning possible mechanisms for the continuous acceleration of electrons to high energies in the vicinity of a hypothetical neutron star remnant of a supernova. Thermal bremsstrahlung and blackbody radiation, in the x-ray region, would both be expected under these circumstances. Spasmodic or periodic sources of such high-energy electrons would lead to time variations of the x-ray intensity on a time scale of perhaps less than ten years, possibly even down to milliseconds. Detection devices with higher time resolution may make possible the observation of such variations.

Collisional Bremsstrahlung

Collisional bremsstrahlung is produced in the Coulomb scattering of electrons by nuclei in local gaseous matter. Thermal electrons in a hot, optically thin gas can produce photons in the x-ray region. If the electrons initially possess a Maxwellian distribution, then the resultant photon spectrum would be an exponent of the form exp $(-E/kT)$. Superthermal electrons (i.e., with energies of tens of keV) can also produce x rays of the observed energies through bremsstrahlung in a cold plasma. While the photon spectrum would depend on the initial electron spectrum, in neither case should the photons exhibit polarization.

Indications are that if the x-ray emission is due to collisional bremsstrahlung, a plasma temperature of about 10^7 °K is required. The observed flux of higher-energy x rays from the Crab Nebula would nevertheless seem to require a temperature of the order of 10^8 °K. In the case of the Crab Nebula, therefore, one must attempt to explain the existence of such hot local areas through a continuous internal source of energy. Heating from the radioactive decay of heavy nuclei produced in a supernova explosion has been mentioned as one possible source. Transferral of possible vibrational or rotational energy stored in a neutron star to the surrounding environment through magnetic interaction, particle acceleration, or progressive waves which steepen into shock waves above the surface, have also been mentioned as possible sources of the heating.

In addition to the thermal bremsstrahlung emitted, there would be recombination radiation and line emission from the gas. Assuming normal cosmic abundance of the elements, one can compute the collisional excitation of emission lines from such a hot gas. The results of these calculations indicate that some of the characteristic emission lines and radiative recombination edges should be detectable above the bremsstrahlung continuum. The Ne X line at 1.02 keV, for example, would be the strongest, and an energy resolution of only about 20% is required to separate out comparable counting rates from the line and the continuum at that energy. About ten other lines are generally not more than a tenth as intense as the Ne X line relative to the continuum. Recombination edges due to captures into the ground state of 0 VIII at 0.87 keV and Ne X at 1.36 keV should show discontinuities of factors of 1.5 in intensity across the recombination edge. Observation of line emission and recombination edges, therefore, would be important evidence that at least part of the x-ray emission from discrete sources is thermal bremsstrahlung. Measurement of the relative intensities of lines would provide information on the abundance of the elements within the sources, also on the temperature of the gas. To determine other physical characteristics of x-ray emitting regions, line shape measurements might be considered as spectral resolution techniques are improved.

Finally, simple considerations of the bremsstrahlung from a hot plasma cloud show that the particle densities in the cloud can be deduced from the observed photon flux and the angular diameter of the emitting region, if a total mass for the source is assumed. A distance for the source is thereby also deduced. Alternatively, the particle density and mass of the source can be deduced from measurements of the flux and angular diameter and knowledge of the distance of the source. Optical identifications should aid in determining the distances of sources.

Compton Scattering

Compton scattering radiation results from the scattering of high-energy electrons by photons of the local radiation field. It appears to have been ruled out as an important mechanism for x-ray production in discrete galactic sources on both experimental and theoretical grounds. From the theoretical standpoint, the probability that a synchrotron photon undergoes such a Compton scattering before escaping from the gaseous region is small, even for the conditions in quasi-stellar radio sources. From the experimental standpoint, a search for the corresponding 20 MeV gammas that would be produced in bremsstrahlung of the energetic electrons in Scorpius gave negative results, placing an upper limit on the proton density in the region of the source of less than 3×10^{-3} protons per cm^3. This is smaller than the average density of protons in interstellar space by a factor of 300.

Here the spectrum would be a Planck blackbody spectrum with the emitted energy proportional to T^4. The spectrum would be easily distinguishable from power law and exponential spectra. Surface temperatures of the order of 10^7 to 10^6 °K are predicted for such objects, depending on age. This is not entirely inconsistent with temperatures deduced from spectral observations of the two major x-ray source regions, namely, Scorpius and the galactic center. A small object, say, 10 km in diameter, at this temperature might be observable in the x-ray region at a distance of about 1,000 parsecs but not observable in the optical or radio portion of the spectrum.

Theoretical objections have been raised to the neutron star concept, the principal objection being that the cooling of a star by neutrino emission might be far too rapid to account for the long lifetime (order of 1,000 years) of the x-ray sources. The results of the calculations have depended on a possibly significant overestimation of the number of pions in the interior of a neutron star. Therefore, there is still some reason to believe that thermal x-ray emission from neutron star remnants of supernova explosions may be observable tens of thousands of years after the explosion, at least at low energies.

In addition to the expected blackbody radiation, there may be distinct spectral features (both in emission and absorption) in the spectrum of neutron stars from 10 to 100 keV, corresponding to the K and L levels of ionized elements in the range from Z = 50 up to 100. These features may not correspond to those ordinarily measured in the laboratory, owing to high Zeeman splitting of lines in the possibly intense surface magnetic fields, to gravitational red shifts, and to thermal broadening. Observation of such lines could nevertheless help in understanding the nature of the x-ray sources.

MECHANISMS FOR GAMMA-RAY PRODUCTION

Possible radiation mechanisms leading to the production of energetic cosmic gamma rays are the following:

(1) Decay of neutral pions ($\pi 0 \rightarrow \gamma + \gamma$), which may be produced in

(i) high-energy cosmic-ray proton collisions with the interstellar and intergalactic gas, or

(ii) proton-antiproton annihilation;

(2) Bremmstrahlung of relativistic electrons and positrons;
(3) Compton scattering of relativistic electrons by photons;
(4) Synchrotron radiation by high-energy electrons;

(5) Annihilation of positrons in the interstellar and intergalactic medium ($e^+ - e^+ \rightarrow 2\gamma$); and

(6) Nuclear de-excitations from excited states of heavy nuclei, following thermonuclear reactions and following neutron capture.

The first four processes produce a general cosmic photon continuum which is certain to exist, although the magnitude of the resulting flux is uncertain. Processes (5) and (6) will produce line emission.

The basic data needed to compute the nature of the photon spectrum are: the flux and spectra of high-energy protons and electrons, the mean density of gaseous matter, the low-energy radiation flux, and the magnetic field in galactic and intergalactic space as well as in discrete sources. Other astronomical data such as galactic and cosmic distances and the rate of expansion of the Universe are also important. It has already become apparent that interpretation of the observations on cosmic gamma rays can, in fact, be used to determine or at least to set limits on these important astronomical parameters. It is significant that these parameters would be extremely difficult to determine from the more conventional radio and optical observations alone. The limits already established from very preliminary observations of cosmic gamma rays are extremely meaningful when applied to cosmological theories. As with cosmic x rays, more refined observations of the spectrum of cosmic gamma rays will provide very restrictive tests of models of the Universe.

A large amount of information about the nature of discrete sources, such as the Crab Nebula, can be gained from gamma-ray astronomical studies. For example, it has been suggested that in a supernova outburst considerable nucleosynthesis takes place. Estimates have been made of the number of radioactive elements produced in such a supernova outburst and the complex line spectrum has been calculated. Detection of these gamma-ray lines would provide definite proof of some details of the theories of nucleosynthesis and the origin of the elements.

Test of the synchrotron radiation hypothesis in the Crab Nebula would be the detection of ultrahigh-energy (5×10^{11}eV) gamma rays which would be produced by Compton scattering of high-energy synchrotron electrons by the lower-energy synchrotron photon spectrum.

Gamma radiation propagates in the interstellar and intergalactic space with almost no absorption, permitting observations from regions hidden to other forms of electromagnetic radiation. Thus, gamma rays may become a new means of probing the galactic center and of the Universe.

THE DIFFUSE X-RAY FLUX

There are several possible source mechanisms for the diffuse flux. This flux may originate from within the Van Allen belt region; it may represent the integrated effect of numerous unresolved sources within the Galaxy; it may be due to the emission from x-ray sources in all external galaxies; or, it may arise from bremsstrahlung in a hot intergalactic medium or from Compton scattering with relativistic intergalactic electrons. The validity of these hypotheses can be investigated by studying the spectral and spatial distribution of the diffuse flux.

On the assumption that the average x-ray luminosity per galaxy is roughly the same as that for our own Galaxy, the extragalactic origin of the diffuse flux appears to be reasonable. This has an important consequence if the x-ray emission spectra of galaxies include strong lines. Due to the differential red shift from expansion, the combined line emission from all extragalactic combined sources would be smeared into a continuum with an edge at the proper emission line energy. The shape of this continuum depends on the large-scale structure of the Universe, that is, on the cosmological model. Thus, the determination of the spectral distribution of the diffuse flux offers the possibility of yielding significant information on the fundamental cosmological problem.

To estimate the average directional intensity due to the contributions from galaxies along a line of sight, we may assume an average source density throughout the Universe equal to the product of the Galaxy's x-ray luminosity multiplied by the density of galaxies. This is then integrated out to the Hubble radius. Taking 10^{-75} cm^{-3} for the density of galaxies, and 10^{28} cm for the Hubble radius, one finds 10^{-1} cm^{-2} sec^{-1} $ster^{-1}$ for the expected average intensity of extragalactic x rays.

One cosmological conclusion has already been drawn from the intensity of the diffuse flux. By assuming that it has its origin entirely from thermal bremsstrahlung in a hot intergalactic medium (the "hot universe" model), the magnitude of the observed diffuse flux appears to establish an upper limit of 10^7 °K to the temperature of the intergalactic medium, which is two orders of magnitude below the temperature required by the hot universe theory. It is expected that future more refined observational data on the diffuse flux of cosmic x rays will provide more restrictive tests of cosmological theories.

ESTIMATE OF X-RAY FLUXES FROM EXTRAGALACTIC OBJECTS

The available data on x-ray sources in the Galaxy can be used to arrive at plausible estimates for the x-ray fluxes that might be anticipated from external galaxies. For example, the 10 cm^{-2} sec^{-1} flux from the general direction of the galactic center may arise from sources near the galactic center. Assuming this to be the case, and that the Galaxy is typical, the flux from the Andromeda Nebula would be expected to be smaller than that from our own by the square of the distance ratio. This gives a flux of about 2 x 10^{-3} cm^{-2} sec^{-1}. In the case of the Virgo cluster, the greater distance is more than compensated by the large number (over 2,000) of galaxies which could be within the field of view. Assuming that the average x-ray luminosity of these galaxies is the same as ours, the estimated total flux from the cluster is 2 x 10^{-2} cm^{-2} sec^{-1}. These numerical values may well be lower limits, since no allowance was made for interstellar absorption effects which may be substantial in the direction in which the local sources near the center of our own Galaxy were observed. Furthermore, the average luminosity of galaxies may be substantially higher than that of our own, as could be the case if some types of galaxies were unusually powerful emitters of x rays.

The prospects of extragalactic x-ray astronomy might also be gauged by rough extrapolation from radio and optical fluxes of synchrotron radiation. Almost all radio galaxies emit a fairly smooth spectrum over the full observable range of radio frequencies from about 50 Mc/sec to 10,000 Mc/sec and the range of observed spectral indices runs from about 0.2 to 1.5 if fitted to a power law. Although in many cases the index is nearly constant over the entire radio-frequency range, more often the index increases at higher frequencies, indicating a deficiency of higher-energy electrons. The most likely sources to provide detectable x-rays would be the relatively young sources which have both the highest radio brightness and the smallest spectral index. Some specific examples are offered below to illustrate the possibilities of extrapolation from radio data.

The strongest extragalactic radio source is Cygnus A. Its flux at 100 Mc/sec is 7.5 times that from the Crab. At 6 meters the spectral index is 0.5, compared to 0.3 for the Crab Nebula, but at wavelengths shorter than 50 cm, the index increases to 1.2. Extrapolated to the x-ray range, this would indicate a flux ~2 orders of magnitude weaker than that from the Crab. The distance to Cygnus A is 2 x 10^8 parsec. If the intergalactic hydrogen content averages 10^{-28} g/cm^{-3} unit optical depth would occur at 10 Å and, at shorter wavelengths, the attenuation would rapidly decrease. Accordingly, Cygnus A should be detectable with an order of magnitude increase in detector sensitivity over that used in past observations, unless the spectral index increases very rapidly toward shorter wavelengths.

The galaxy M 82 provides visible evidence of a colossal galactic explosion. The Palomar photographs show matter being spewed out in all directions. In the plane of the galaxy the explosion is slowed down by the relatively dense gas, but above and below the plane the particles escape relatively freely at relativistic energies. M 82 is located at high galactic latitude and its distance is 10^7 parsec. Intergalactic absorption should be negligible and galactic absorption minimal. Its radio flux is about 100 times weaker than that from the Crab and its spectral index is 0.23 from radio to visible frequencies. It is, therefore, a likely x-ray emitter at an intensity perhaps 100 times weaker than the Crab.

3C273 is the brightest quasar. It has been resolved into two components, A and B, with spectral indices of 0.9 and 0.0, respectively, near 400 Mc/sec. Eighty percent of the radio flux originates in the B component, with the flat spectral characteristic. At 400 Mc/sec, 3C273 B is 50 times weaker than Taurus A. Recent optical evidence indicates that the spectral index increases rapidly in the optical range of wavelengths, and its x-ray flux may be considerably less than 1/50 that from the Crab. Because of its great distance (5 x 10^8 parsec) intergalactic extinction will be strongly wavelength dependent and unit optical thickness will occur at about 8 Å if the density of intergalactic space is 10^{-28} g/cm^{-3}.

4. METHODS OF OBSERVATION

NONFOCUSING X-RAY OPTICS

All of the x-ray instruments flown thus far have employed simple mechanical collimation to define the field of view. Sensitivity has been achieved through the use of large area detectors—Geiger counters, proportional counters, and scintillators. The practical limitation on the further development of such devices is the space available on rocket and satellite carriers and the load lifting capability of balloons. In the Aerobee 150 rocket, the instrument compartment is a 15-in. diameter section, between 2 and 3 ft long. In x-ray surveys already carried out, this space has been almost completely utilized. The one successful x-ray observation thus far accomplished from a balloon used about 100 cm^2 of crystal scintillator, but apparatus now under construction will employ a 5000 cm^2 proportional counter.

There are opportunities for order of magnitude increases in the sizes of large-area detectors, if larger rockets can be obtained. Existing military rockets, such as the Sergeant and Nike Hercules, offer roughly 10 times the instrument compartment cross section of the Aerobee and about 5 times the payload, with substantial gain in altitude performance and flight time. It is important that such systems be made available to scientific users. Apollo vehicles, at various stages in the series of unmanned flights, can accommodate 5 to 10 square meters of x-ray detectors without resorting to unfolding techniques.

The Crab Nebula has been observed with a Geiger counter sensitive to the 1-15 Å spectral region, combined with an anticoincidence counter. The anticoincidence rejection efficiency was about 75%. The observed signal-to-background ratio was 7 and the flux above background about 3 counts cm^{-2} sec^{-1}. In practice 95% of the background counts produced by the interactions of cosmic rays in the detector and collimator can be rejected through the use of active anticoincidence shielding. The remaining cosmic-ray induced background, which amounts to an equivalent of 0.1 count $cm^{-2} sec^{-1}$, is still generally large compared to that due to the diffuse component of the cosmic x rays, except for detectors with very large fields of view.

From these numbers we can estimate the detection sensitivity of detectors with larger areas using the Crab as a standard source intensity. It is only necessary to consider the statistics of accumulated signal count relative to background. Let us suppose the total time of observation is 1 sec. Faster scanning will produce poorer statistics; slower scanning will improve the detection sensitivity. With unstabilized rockets the 1-sec integration is difficult to achieve; with stabilized rockets it can be well exceeded. A 1-m^2 counter with 95% efficient anticoincidence background rejection will have a residual background of about 1000 counts sec^{-1}. In 1 sec the 3-sigma background fluctuation will therefore be about 100 counts. The Crab would produce 30,000 counts in 1 sec, and a source only 1/300 of the brightness of the Crab would thus produce a signal equal to 3 sigma (background). Increasing the area still further will improve the sensitivity of detection as the square root of the area. In the Apollo Extension Systems (AES) program, it is feasible to consider a counter area of 100 square meters, a field of view of about one square degree and a scan rate of one degree per second. Between the largest detectors that can be erected from AES, with the aid of man, and the area limit of the Aerobee rocket, there is a wide range of possibilities in intermediate rocket and satellite sizes with appropriate compromises in scan rates. For example, even with the Aerobee rocket, it is possible, with stabilization, to utilize the full flight time for the observation of a single source, and thus gain 2 to 3 orders of magnitude in integration time over the unstabilized random scan mode.

Various collimators have been used with large area detectors. Hexagonal honeycomb gives a nearly circular field of view and has been used effectively for locating sources in a broad expanse of sky. Rectangular cellular collimators have been employed to increase the resolution in the scan direction at the expense of expanding the field of view in the direction perpendicular to the scan. The modulation collimator, consisting of parallel grids, has provided upper limits on source dimensions as small as 7 min of arc. With a well-stabilized Aerobee rocket and a smoothly controlled scan it is feasible to extend modulation measurements to yield a resolution of about 10 or 20 sec of arc.

Position measurements, except for the Crab occultation observation, have thus far been made from unstabilized or rather poorly stabilized rockets and have necessarily been relatively uncertain.

Spectral information from large area detectors has been derived by use of pulse height discrimination and by filter photometry. Large area proportional counters can achieve 20% energy resolution in practice and are expected to demonstrate such performance in flight in the immediate future. Much improvement in filter photometry is also possible within the Aerobee class of instrumentation. In the case of the Scorpius source it is even feasible to attempt x-ray crystal spectrometry with 0.1 Å resolution, from an Aerobee stabilized to one or two degrees.

Polarization measurements can provide important clues to the nature of the x-ray emission process. Studies of the performance of a liquid hydrogen target for Thomson scattering indicate that as little as 3% polarization from the Scorpius source could be detected in the 300 sec of observing time available during an Aerobee flight.

Large-area detectors are especially well suited to occultation measurements. The only positive identification of an x-ray source thus far has been obtained from the observation of the occultation of the Crab Nebula by the Moon. A remotely controlled mechanical occulter of large size, separated by considerable distance from a manned laboratory, is, in principle, feasible. A lunar orbiter can be employed to much greater advantage.

The most advantageous base for occultation measurements is the surface of the Moon itself. For example, a large-area detector may be placed on the Moon near the center of a lowland region surrounded by a high rim or crater wall. The rise and set of x-ray sources at the rim can be recorded continuously. If the rim is about 50 km distant from a detector whose vertical dimension is 10 cm, the subtended angle is about 0.5 sec of arc. Because the Moon rotates at the slow rate of about 0.6 sec of arc per sec of time, the rate of star-rise and star-set is relatively slow compared to the situation on Earth. The occultation angle of 0.5 sec of arc will be covered in about 1 sec of time, which will permit the accumulation of a large number of counts by a detector a few square feet in area. The counter would be collimated to restrict the field of view in the vertical direction, but would accept a wide field in the horizontal direction in order to scan a broad band of the sky.

FOCUSING X-RAY OPTICS

Introduction

Focusing x-ray optics have already been used successfully for solar observations. Such telescopes can now be utilized for the observation of cosmic x-ray sources. In principle, they can produce images of cosmic x-ray sources with angular resolutions comparable to what can be achieved in visible light. In addition, they can focus a parallel beam of x rays impinging on a large collecting area on to a small target area, where it can then be detected with a much improved signal-to-noise ratio. The latter application permits the use of dispersive techniques for high-resolution spectroscopic measurements, and affords new possibilities for polarization measurements. The x-ray reflecting telescope should prove to be a powerful tool for the study of galactic and extragalactic sources at wavelengths greater than 1 Å.

The Working Group therefore recommends the following:

(i) experiments utilizing focusing x-ray optics now at hand should be flown on pointed rockets and satellites currently available, or being developed;

(ii) efforts should be made to improve the present rocket and satellite pointing controls to reduce the limit on pointing jitter to 5 sec of arc or less;

(iii) plans should be initiated for orbiting an x-ray telescope of greater focal length than can be accommodated in present satellites. Lengths in the range of 30 ft to 100 ft are considered necessary to achieve the desired sensitivity and resolution. Unfolding of an extensible system in space may be possible.

Nature of X-Ray Optics

The optical devices for focusing x rays consist of reflectors on which x rays impinge at small angles and are totally externally reflected. Total reflection optics are essentially achromatic at wavelengths longer than a certain cutoff value that depends on the atomic number of the reflecting material. The lowest cutoff wavelength that can be achieved at present is at 3 or 4 Å. This limit can probably be extended to about 1 Å. Reflection optics do not substantially alter the spectral shape and the state of polarization of the impinging radiation.

Imaging Systems. Two or more reflections are necessary to remove first-order aberrations of a focusing system. The reflecting surfaces of the objective were a paraboloid and a hyperboloid. A ray diagram for these two surfaces is shown in Figure 4.

For most cases in which imaging systems are used in practice, the lower limit on the intensity of a discrete source that can be detected by an x-ray telescope is set, not by the signal-to-noise ratio, but rather by the requirement that a significant number of photons be detected within an image resolution element. This can be understood if one considers that the area of a resolution element in the focal plane is many orders of magnitude smaller than the area of collection. The part of the background that depends on the area of the detector element, such as that produced by cosmic rays, will therefore be reduced by a corresponding factor when compared with detectors with only mechanical collimation, and can be further reduced by increasing the angular resolution of the device. The diffuse cosmic x-ray flux is much weaker than the background from other

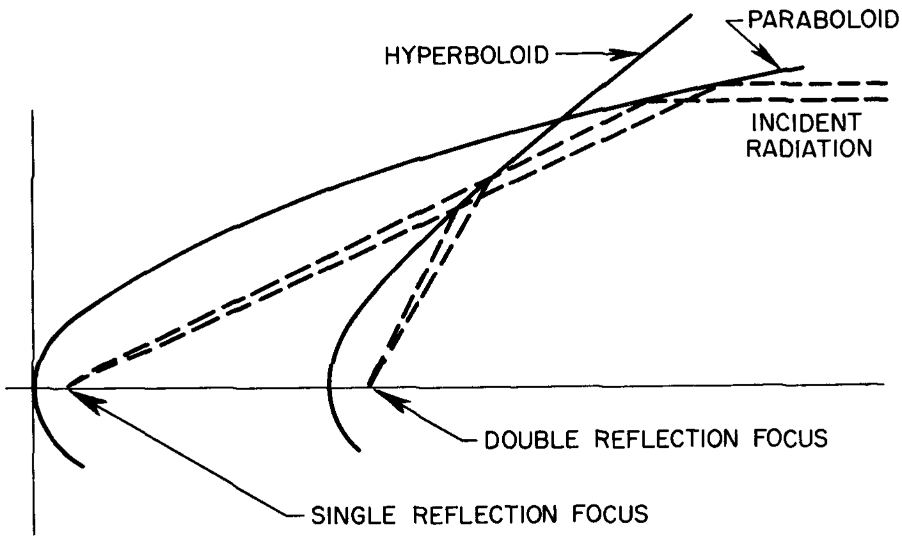

Figure 4. Schematic representation of the optical principles utilized in the grazing incidence x-ray telescope. (Courtesy of American Science and Engineering, Inc.)

factors. In view of these considerations, the effective sensitivity of a focusing telescope is proportional to At, the product of the area of collection and the time available for observation. The sensitivity of mechanically collimated detectors, which is determined by the signal-to-noise ratio, is, on the other hand, proportional to (At) 1/2.

Telescopes of moderate dimensions (2 meters focal length) could permit in one hour of observation the imaging of sources 10^{-2} or 10^{-3} times the intensity of the Crab, with 5 sec of arc resolution. Telescopes with focal lengths of 30 meters or more should permit an increase in sensitivity to the point where sources with intensities about 10^{-6} of the Crab may be detected.

One-Dimensional Focusing System. To conduct surveys for either discrete or extended x-ray sources in the low-energy (<12 keV) range, one may use focusing systems with line-shaped fields of view. A large field of view in one dimension can be obtained with a nest of curved mirrors, arranged somewhat like a simple Venetian blind. Both high sensitivity for detecting discrete sources and precise positional information in one coordinate may be obtained with such a system. Its signal-to-noise ratio lies between that of the large-area detectors with cellular collimators and that of the imaging telescopes discussed above. For medium-sized instruments and with moderate observing times, the sensitivity may be comparable to that obtained in a full imaging system.

Detection Techniques. Although conventional x-ray detectors can be used with focusing x-ray optics in certain applications, the highest resolution will be realized with photographic recording or electronic imaging systems. Of these two, photographic recording, which allows longer integration times and extremely high resolutions, may well prove necessary to realize the full capabilities of a very large high-resolution system. This may require the use of a man to retrieve the photographs. Electronic image detectors, which are used at present in other branches of astronomy, are more sensitive than photographic emulsions and may ultimately be capable of comparable resolutions in the x-ray region. Their principal limitation may be the large rates of data transmission that they require.

Investigations Using Focusing X-Ray Optics

In this section, different kinds of problems are matched with vehicles and other conditions needed to solve them, with special reference to x-ray optics.

In general, one can say that for most problems satellites will prove superior to sounding rockets, except in flexibility and cost. In particular, the long integration times possible with a satellite will allow the detection and spectral investigation of sources orders of magnitude fainter than can be done from rockets.

Several significant experiments can be carried out immediately with existing focusing x-ray telescopes mounted on pointed rockets and satellites, even though pointing accuracy and stability are still limited. Pointing accuracy and stabilization much better than are now available and longer focal lengths will, however, be needed to make possible detection of much fainter sources and detailed observations of their structure. Initially, pointing and stabilization should match the 5 sec or better angular resolution, now in view for shorter-focus telescopes, but design goals of 1 sec of arc pointing accuracy and stability and a 30-meter focal length may be achievable in the next decade. The NASA Orbiting Astronomical Observatory (OAO) satellites and the Apollo Extension Systems (AES) program (perhaps with a gimballed or tethered telescope) would lend themselves to achieving these goals.

Angular Size and Location of Sources; Surveys for Weak Sources. Focusing optics with focal-plane modulation grids can be used on rockets with modest pointing accuracy (0.25°) and moderate drift rate (0.25° sec^{-1}) to measure the size and location of all known sources, as well as somewhat weaker sources, with an accuracy of perhaps 5 sec of arc. Satellites with comparable accuracies and drift rates would reach two orders of magnitude fainter, and so would detect many weaker sources. Such measurements, however, would not define the structure of the sources. Surveys for weaker sources, or of the structure in the diffuse flux, could be carried out with the one-dimensional or fully focusing optics with a finer angular resolution than that attainable with cellular collimators.

Structure of Sources. With rockets, the internal structure of x-ray sources can be studied using photographic image recording, but the resolution would be comparable only with that of the rockets' pointing stability. Electronic image recording techniques might be able to compensate for jitter in pointing. With a jitter of 1 min, the structure of only the strongest known sources could be observed; with 5 sec, all known sources could be studied, and weaker ones as well. The angular resolution in images obtained from satellite instruments now seems to be limited only by the pointing jitter of satellites of, e.g., the OAO type. If jitter is reduced sufficiently, however, the limiting factor will be the resolution of the detector. Photographs, which are now superior from the standpoint of combining image resolution, integration of weak fluxes, and compactness of information storage, can of course be recovered from rockets; but their use in satellites to realize their full potentialities will require the development of a film recovery system, perhaps based on the use of man in space.

Spectral Investigations. The availability of focusing optics opens up new possibilities for the measurement of the spectra of discrete sources with high spectral resolution. The combination of large collecting areas and dispersive optics permits an analyzed beam to be detected by a very small and well-shielded detector, the background counting rate for which can in principle be made negligibly small. The characteristics of available spectrometer designs indicate that an energy resolution of 1% can be attained from satellites. The stronger sources should be accessible to measurements made from rockets with such techniques.

Polarization Studies. Experiments to measure polarization of x rays based on Thomson scattering in a target of liquid hydrogen or lithium hydride are being prepared for use on rockets. Equipment utilizing the Borrman effect is now under development and may prove capable of determining the amount and direction of polarization. The use of x-ray optics in conjunction with polarization analyzers would of course considerably improve the signal-to-noise ratio. Experiments that appear marginal with rockets could be performed with confidence from a satellite, which according to our best estimates should allow the detection of polarization as weak as 1% in all known sources.

GAMMA-RAY ASTRONOMY IN THE RANGE 0.1 - 30 MeV

Introduction

Experiments in the next few years over the energy range 0.1 - 30 MeV will continue to be of an exploratory nature, designed to search for point sources and gamma-ray lines, and to study the diffuse component. Important observational attempts, particularly on known x-ray or radio sources, will soon be made from balloons. Experiments designed to study the diffuse component or to accomplish a sky survey must be conducted from a vehicle with minimum capabilities at least equal to those provided by Explorer-class satellites.

The Working Group recommends that:

(1) Continued support be given to the development of detection devices using actively shielded collimators or Compton scattering telescopes, as well as for the development of promising new techniques;

(2) Space flight assignment be given only to instruments which indicate clear improvement in background reduction, energy resolution, or angular definition, and which have been proven on balloon investigations;

(3) Greater support be given to balloon programs as well as Explorer-class satellites, to implement observations in this energy range;

(4) Support be provided for the continued study of solar x-ray and gamma-ray spectra during both quiet and active phases of the solar cycle.

Gamma-Ray Detector Technology

In the 0.1 to 30 MeV energy range, the short wavelength of the photons precludes efficient broadband focusing or interference techniques. Therefore, one must use detectors which depend on scattering or absorption phenomena. At energies above a few MeV detection devices which utilize the pair production process become practical.

Currently, three combinations of Na I or Cs I scintillation counters are being used by workers in the field. Single omnidirectional counters were used on Ranger III and OSO-I. A combination of counters known as a Compton coincidence telescope was used on the OSO-I to form a directional counter with about 10° angular aperture. This failed to give significant results because a high flux of background gamma rays was produced by cosmic rays interacting with the local matter of the satellite. A directional counter, with an active collimating shield of Cs I, has been flown on the OSO-II, and is planned for use on the OAO-A and the OSO-C as well as for use on a number of balloon observations. Pair spectrometers, which may be designed to give good angular resolution as well as energy resolution and background rejection, have not yet been employed for gamma-ray astronomy observations.

Energy resolution gamma radiation is limited by present counter technique to about 5 or 10% for gamma energies near 1 MeV. Past experiments have not even taken advantage of this inherent resolution. However, the best prospects for a significant improvement in resolution, barring a major breakthrough in scintillation counter technique, lie in the development of solid-state counters with large volumes. Present devices have volumes up to 10 cm^3, and resolutions of better than 1% at 1 MeV. If volumes of 100 cm^3 were available in solid-state detectors without a corresponding loss in stability or linearity, these devices could replace the scintillator-phototube combination in many instruments. Utilization of the resolution inherent in semiconductor devices will also require the development of space-qualified multichannel analyzers of high precision.

Angular resolution of present and immediately foreseeable detectors is about 10° or a solid angle of 0.1 ster. Devices such as the Compton telescope or the shielded detector with an active collimator can be extended to an aperture of a few degrees, or about 10^{-2} ster. It is not clear at present what techniques may be employed to achieve resolutions of minutes of arc in gamma-ray detectors.

The reduction of the background counting rate in this energy range is closely associated with the angular resolution problem. The background has three principal sources: (1) secondaries produced by cosmic rays interacting with the matter in the instrument or nearby; (2) incomplete rejection of the gamma rays arriving in directions other than through the forward aperture; and (3) production in matter, other than the source under observation, within the forward aperture of the instrument. The last item may include instrumental matter, residual atmosphere above a balloon, or even production in the Galaxy or metagalaxy. Anticoincidence techniques must therefore not only reject direct particle effects, but also the effects of unwanted gamma rays outside the instrument aperture. The rejection efficiency must be particularly good in the case of devices with low efficiency for detection, such as Compton telescopes or pair spectrometers. It is important that as instruments of smaller angular aperture are constructed, the counting rate from background effects be reduced in proportion to the reduction in the solid angle. Of course, sources with angular sizes smaller than the effective instrument aperture may be studied by scanning the source until enough events have been obtained to perform a statistically significant background correction.

Future Developments. In the next three to five years, the development of present devices will probably be carried to its technical limit. Areas of single detectors appear to be limited to several hundred square centimeters because the background detected in coincidence devices tends to increase faster than the area, and the dead time due to active anticoincidence shields tends to offset the gain in area. It is therefore usually desirable to increase the observing time rather than the area.

One may visualize, for example, groups or clusters of shielded detectors. Arrays of scintillators or solid-state counters may be connected as a Compton coincidence or pair-spectrometer hodoscope to observe over many elemental solid angles simultaneously. Although technically difficult, spark chambers may eventually be applied to this energy range.

Furthermore, the search for new detector techniques will continue. Progress in this regime of the gamma-ray spectrum will most certainly follow improvements in detector technology.

Vehicles for Experiments

Rockets will generally not be useful in this energy range, chiefly because of the difficulty of mounting large-area detectors on small rockets. Therefore, exploratory observations must be made from balloons and satellites. The balloon will be a major vehicle for certain specific observations and detector studies. Extended observations and sky surveys must be made from satellites. Although present detectors have weighed 20 to 75 lb, counter arrays weighing several hundred pounds may be anticipated. Owing to the constraints imposed on satellite construction, in order to reduce the background, particularly with low-efficiency devices, satellites of the Explorer class may offer particular advantages.

Each new satellite mission should be a refinement over previous missions. In addition to providing new observations, each new mission should be designed to provide information required to instrument succeeding satellites.

An orbiting gamma-ray observatory must be equipped with booms capable of supporting relatively large detector masses distant from the satellite body. Unlike the situation in optical astronomy, it is not likely that the field will have developed so that large general purpose instruments can be reasonably planned within the next decade.

Use of Man and Manned Missions

During the next few years the important observations in the energy range 0.1 to 10 MeV will be made from balloons and Explorer satellites. The use of man and manned space vehicles may permit important supplemental observations and may allow certain instruments, possibly not feasible otherwise, to be launched. These missions should in no way be regarded as a substitute for the balloon and satellite work, but would in fact rely heavily on experience gained with such vehicles in order to provide for an efficient utilization of the Saturn capability.

The use of large manned observatories operating in this energy range cannot now be planned. Observations from the lunar surface have the disadvantage of operating in a high-background situation, and imposing considerable restraints on the data acquisition.

The Saturn capability may be used either in a manned or unmanned mode, or for the injection of special-purpose, rather self-contained payloads into orbit. In most cases it will be necessary to separate the instrument from the large vehicle mass, and to provide an orbit either below the trapped radiation zones, or beyond the magnetosphere.

The manned capability inherent in the Apollo Extension Systems may be used in conjunction with special devices, such as a Compton telescope array, for example. The astronaut might remove the large subassemblies of such a system from the instrument compartment, assemble them, remove the assembled instrument to a large distance from the spacecraft, and align it coarsely, so that an automatic tracking system may then acquire a celestial object of interest. The data could then be telemetered to the ground, where scientists could interpret the data in nearly real time. The astronaut could be instructed as to when calibrations, say, with a radioactive source, are required or when enough events have been accumulated to form a statistically significant measurement. It may be possible to carry out simple changes in the geometry or logic of the instrument. In this manner the astronaut would serve to provide flexible and intelligent control of the experiment.

GAMMA-RAY ASTRONOMY IN THE RANGE ABOVE 100 MeV

Introduction

No definite flux of photons in this energy range has been detected. Because of the low photon intensities and large background radiation, only upper limits to the flux have been determined. In addition, the ratio of the radiation from point sources to the diffuse radiation may be very small, making the detection of point sources all the more difficult.

With respect to the experimental aspects of high-energy gamma-ray astronomy, the Working Group makes the following recommendations:

(1) Instruments with a large collection factor (area x exposure time) should be developed which would provide angular resolutions of better than 1°, which could determine the energy spectrum, and which would possess a proven capability for background rejection;

(2) Experiments should be undertaken in balloons to search for point sources and to define the design requirements for satellites;

(3) Since measurement of the diffuse radiation flux would be a significant experiment and could be performed in the immediate future with small Explorer-type satellites, it should be attempted.

(4) Since experiments to study spectral composition and directional structure of the diffuse flux and to measure weak fluxes and spectra from discrete sources will ultimately need a large orbiting vehicle, one of the forthcoming large orbiting vehicles should be scheduled for this purpose.

Distinguishing Features of Gamma-Ray Measurement

Low Quantum Rate. In contrast to x rays, ultraviolet light, visible light, and infrared and radio waves, the total primary flux of high-energy gammas—including both the diffuse radiation and all point sources—is known to be no more than a few quanta per square meter per second and per steradian. A particularly important quantity to measure, namely, the diffuse flux from interstellar collisions of cosmic rays with gas atoms in the plane of the galaxy, is expected to be about 10^{-4} cm^{-2} sec^{-1} $ster^{-1}$ in the direction of the galactic center. A substantial portion of the sky has already been scanned for point sources, with none appearing at intensities as high as 10^{-3} cm^{-2} sec^{-1}. Optimistic theoretical estimates for the expected flux values yield only 10^{-5} cm^{-2} sec^{-1} for the most favorable source (the Crab Nebula), and realistic estimates for the various probable sources are in the range 10^{-7} to 10^{-8} or less. Surprises may appear, but these figures determine the sensitivity thresholds that are likely to be found necessary for detectors.

Potentially Large Background. The primary flux of charged cosmic rays is at least 1000 times that of the primary high-energy gamma rays. Furthermore, in the atmosphere and in the walls of a vehicle or the detector itself, these charged particles are efficient generators of secondary gamma rays which (except for the directionality of radiation from point sources) are indistinguishable from the primary gammas one hopes to detect. Associated with this secondary production is the likelihood of long-range penetration and large-angle scattering, which rules out simple collimating schemes. In order to detect sources as weak as 10^{-7} cm^{-2} sec^{-1}, one will need a rejection factor for such general background on the order of ten million; and to reach a sensitivity of 10^{-8} the rejection factor must be 100 times higher.

Diffuse Flux. In addition to the radiation from point sources, a diffuse radiation is expected. The measurement of the diffuse flux will be of profound significance, particularly in revealing the composition of intergalactic space, but also for seeing the structure of the Galaxy, especially its central part. The ratio of diffuse flux to point source strength will probably be substantially higher than it is for x rays and radio waves, which makes moderately good angular resolution a requisite for distinguishing point sources. In particular, the diffuse flux is expected to be between 10^{-5} and 10^{-4} cm^{-2} sec^{-1} $ster^{-1}$. For comparison, the current experimental upper limit is 3×10^{-4}. To observe a point source producing 10^{-7} cm^{-1} sec^{-1} against this diffuse radiation, angular resolution of about one degree is needed. Furthermore, in order to resolve the expected structure in the Galaxy, the angular resolution of the detectors should be no worse than 0.5°.

Discriminating Type of Detector. A high information content per recorded particle is necessary to attain the large (factor 10^7) background rejection which is required. Simple Geiger counter or scintillator arrangements have not yet proved satisfactory. Pictorial instruments (emulsions, spark chambers, etc.) suggest themselves. There is currently under development an instrumental system combining spark chambers and emulsion. The gamma rays convert to electron-position pairs in the emulsion, which are in turn detected below in the spark chamber, triggered by a Čerenkov counter. A suitable anticoincidence system eliminates charged particles. The advantages of the instrument are, 1) the large collecting areas possible, but at the same time, 2) an angular resolution of about 1°, because the point of the conversion of the gamma ray can be seen in the emulsion, and 3) the possibility of using a thick converting layer without loss of information. Background events may also trigger the spark chamber, but there can be no confusion once the tracks are identified in the emulsion. The need for rapid analysis of large numbers of events, mainly background on which the sought-for flux is superimposed, calls primarily for electronic ingenuity and built-in detector selectivity. Experience in the solution of similar but more serious problems in high-energy nuclear physics suggests that this one is probably easily solvable. Any system in which emulsions are involved will of course need to be recovered and will have a limited exposure time in orbit. This suggests that, in addition to balloons and rockets, the development and use of recoverable satellites should be encouraged.

Long Exposure Times. Precise values cannot yet be given for the required product $At\epsilon$, where A is detector area, t the time of observation in a particular direction, and ϵ the detector efficiency: it is the function of balloon experiments in the first phase of these studies to define this requirement more closely. However, indications are that in order to detect point sources, this product may have to exceed 10^9 cm^2 sec, and since ϵ cannot exceed 1/3, the short duration of sounding rocket flights would require areas so large (1000 square meters) that rockets are clearly ruled out as appropriate vehicles for this purpose. Balloons limit the observation of single directions to about 10^4 sec per flight, and on this account as well as the enhanced background effects in the atmosphere, the use of balloons will limit the sensitivity to about 10^{-6} cm^{-2} sec^{-1} for point sources, although even this value requires a rather large and heavy detector (a square meter of active area, probably weighing at least 1000 lb). After preliminary work with balloons, it will be important to take advantage of the combination of long observation time and low background possible in orbiting vehicles, from which sensitivity limits as low as 10^{-8} cm^{-2} sec^{-1} are possible, using detectors that can now be built.

Fairly Large Weight. It is essential that the detectors provide energy resolution. To do this, the apparatus for measurement of high-energy gammas must be dense, and the dimensions anticipated in meters. Experiments may be expected to grow gradually in size and weight from present levels of 500 lb weight and 1000 cm^2 area to future experiments with sensitive areas of at least 10 m^2 and a weight of 10,000 lb.

Angular Resolution and Orienting Ability. It follows from the foregoing conditions that angular resolution of about a degree is necessary, and for some purposes 0.5° would be highly desirable. Some types of apparatus currently visualized will require only a record of their orientation, instead of active pointing. Future equipment, however, will require orienting and stabilizing mechanisms. Precision better than 0.5° is not likely to be required in the near future, largely because known means of detection cannot define the angles of gamma rays more closely.

Prospective Program

Use of Balloons and Small Satellites. The upper limit to the strength of high-energy gamma point sources set by observations so far range from 10^{-4} to 10^{-2} photon cm^{-2} sec^{-1}, whereas balloon experiments are capable of achieving sensitivities as low as 10^{-6}. Hence, an extension of present exploration to several orders of magnitude fainter is possible by this means. It is quite possible that discrete sources with intensities in the range 10^{-6} to 10^{-4} photon cm^{-2} sec^{-1} exist. It certainly seems advisable to pursue this exploratory path, at least for the next few years— partly to discover any sources that may exist, but also partly to define the design requirements for eventual much more expensive experiments from outside the atmosphere. From within the atmosphere it is very difficult to determine the diffuse part of the primary high-energy gamma intensity, because the atmospheric source is also diffuse and overwhelmingly strong; but it would be at least possible to lower the upper limits on the diffuse cosmic component somewhat. On the other hand, measurement of the diffuse component is one of the significant things that is simple enough to put on a satellite of the Explorer type in the immediate future.

Ultimate Need of Large Orbiting Vehicle. With the lower background and longer exposure times possible in orbiting vehicles outside the atmosphere, two important things can be accomplished: (1) the diffuse gamma flux, its spectral composition, and its directional structure can be determined, thereby mapping the quantity which is the product of gas density and cosmic-ray density over the Galaxy and also its average value in intergalactic space; and (2), discrete sources to a level between 10^{-6} and 10^{-8} cm^{-2} sec^{-1} can be observed, in which range it is very likely that a number of sources will be found. Taken together with the radio, optical, and x-ray data from such discrete sources, the gamma-ray spectrum will unambiguously determine the radiation mechanisms, as well as many of the critical parameters specifying the physical conditions in those remarkable objects. The greatest sensitivity mentioned here as being possible may even be high enough to detect gamma rays from the strongest quasars.

It should be made clear, however, that the attainment of sensitivites as high as 10^{-8} requires a fairly large-scale effort: e.g., the product At must then exceed 10^{11} cm^2 sec, as could be obtained with an apparatus having a sensitive area of 10 m^2, directed at single sources for exposure times of about two weeks. This would be pointed only in directions where optical, radio, or x-ray data had indicated the presence of an interesting object to be investigated. A Saturn-boosted vehicle would be appropriate for these purposes, and the men on board could exercise intelligent control of the observations if that should prove helpful.

5. VEHICLE REQUIREMENTS

THE ROLE OF BALLOONS

Introduction

The Working Group recognizes that balloons will be essential vehicles for astronomical observations in the x-ray and gamma-ray region during the next several years. Because of the relative simplicity and comparatively low costs of balloon research and because of the comparatively short times required to complete an experiment, balloons will also play a vital role in enabling universities to participate in such research and in attracting graduate students to the field. The Working Group therefore recommends that balloon experiments and balloon vehicle development be made an integral part of the space astronomy program, and that the support of these efforts be expanded to meet the scientific needs which are now apparent.

Advantages of Balloons

The value of balloon observations in x-ray astronomy was demonstrated by the recent measurement of the spectrum of high-energy x rays from the Crab Nebula, carried out with simple apparatus in a balloon flight at 130,000 ft. Similar flights with more sophisticated equipment at this and higher altitudes can yield a complete sky survey of discrete sources of x rays with energy greater than 20 keV, and can determine the precise source locations, spectra, and angular sizes. Because of the long observation times which balloons provide at a relatively low cost per flight, balloon experiments are the best means for attaining these objectives during the next few years. They have the advantage of short lead times, amounting to only a few months, so that they can be planned and executed in quick response to new discoveries and technical developments.

Moreover, balloons are invaluable for two other major reasons: as vehicles for testing ideas and equipment for later use in satellites, and as a means of enabling universities to participate in the program. Indeed, the latter point is a crucial one: if any but a very small number of universities are to have a direct part in the NASA program, balloons are the solution. The time-scale of experimental development can be reasonably short compared to the average time of residence of a graduate student, so that he can see a piece of research work through from beginning to end, and most of the instrumentation can be handled by a well-equipped university machine shop. But it is more than simply a question of whether universities can participate or not; there is the problem of attracting students into the field. Indeed, many of the most competent investigators now engaged in satellite and rocket experimentation gained their early experience in balloon work.

Need for Further Fundamental Balloon Design Studies

The minimum altitude for useful x-ray observations is approximately 130,000 ft. At the present time flights to this altitude with scientific payloads of up to 200 lb are routine and reliable. Flights above 140,000 and even 150,000 ft have been achieved, but only at much greater cost and with much less reliability. On the other hand, several current lines of technical development, both in balloon materials and in structural design, give promise of major increases in the performance and reductions in the cost of balloons for these extreme altitudes. A relatively small development effort, properly guided by qualified balloon engineers and scientists, should be able to achieve these improvements within a short time and thereby greatly enhance the already substantial advantages of balloon observations in x-ray astronomy.

Balloon Instrumentation

Several of the x-ray balloon observations for which a scientific need can be seen at the present time will require certain instrument items in common. These should be engineered and produced so that they are available to the various interested investigators. One such item is a star-guided orientation control system with 1 min of arc accuracy or better, together with an appropriate telemetry and command system.

As with x-ray astronomy, in the field of gamma-ray astronomy balloon observations have been and will continue to be important in the search for discrete sources at increasingly lower thresholds of detectable intensity, as well as in the development and testing of instruments to be used in satellite experiments. New techniques that have been developed recently for suppressing interfering background counts in the energy range from 0.1 to 10 MeV should be fully exploited in balloon experiments, for example, those aimed at the detection of radioactively produced gamma rays from supernova remnants. As for observations of gamma rays in the energy range above 10 MeV, there is general agreement that the next significant exploratory step beyond those taken with the present generation of gamma-ray telescopes will require the construction of instruments with about 1° angular resolution and sensitive areas of over 1 m^2. These can and should be used first with balloons at the highest attainable altitudes to search for discrete sources. Since the intensity of background high-energy gamma radiation generated in the overlying atmosphere varies in direct proportion to the pressure altitude, every increase in altitude will lower the threshold of detectable source intensity. These measurements will provide essential data needed in the design of the large area instruments that must ultimately be flown in satellites. Instruments with the necessary area, resolution, and discrimination will almost certainly weigh over 1000 lb. Thus a need exists for the development of balloons capable of lifting such massive experiments to altitudes above 130,000 ft.

Need for Expanded Launching Facilities

During the past year the rate of scientific balloon flights has overtaxed the capacity of the National Center for Atmospheric Research facilities at Palestine, Texas, where most of the x-ray and gamma-ray flights are now launched. The prospect for the next three years is an increasing demand for flights not only for x-ray and gamma-ray astronomy, but also for other fields of research. These demands cannot be met without a substantial increase in the physical plant at Palestine and in the personnel involved in launch operations.

Summary of Recommendations

In view of the anticipated needs of x-ray and gamma-ray astronomy for flights with currently available balloons, and for flights with balloons of higher performance characteristics that lie within the immediate range of technical development, the Working Group recommends the following:

(1) Increased support for balloon experiments;

(2) An increase in the number of authorized flights to accommodate an estimated 40 experiments in x-ray and gamma-ray astronomy per year;

(3) Funding of a three-year program for the development of two types of reliable balloons with the following performance characteristics:

(a) 250 lb of scientific payload to be carried to 145,000 ft;

(b) 2000 lb of scientific payload to be carried to 130,000 ft.

Attainment of these objectives will require fundamental advances in balloon technology.

The developmental program should be the responsibility of an experienced group of balloon scientists and engineers, preferably in the National Center for Atmospheric Research, and is expected to cost a total of about $2.0 million for the three-year period.

(4) Funding of engineering development and prototype construction of a commanded star-guided orientation control system capable of better than 1 min of arc accuracy.

THE ROLE OF ROCKETS

The discovery of the first discrete x-ray sources was accomplished with a rocketborne instrument and it is expected that a very substantial fraction of x-ray astronomical research will continue to use sounding rockets. Rockets, balloons, and satellites supplement each other. Each has an important role that may change. At present, the Working Group notes the following important points concerned with the role of rockets in x-ray research:

(1) Like balloonborne instruments, but in distinction to satelliteborne instruments, rocketborne instruments can be conceived, built, and flown on a relatively short time scale. New developments can be investigated promptly. This has the important function, among others, of ensuring that the more elaborate and more expensive satelliteborne experiment will be as technically advanced as possible.

(2) The relatively short flight time in rocket experiments is partially offset by the fact that the instruments are recoverable for further flights.

(3) X-ray technology will certainly continue in a state of rapid development and innovation. Rockets provide an ideal tool for the trial of new instruments before assignment to a satellite.

(4) Competent experimental groups entering the field of space research can gain valuable practical experience through carrying out rocket investigations before attempting satellite investigations.

(5) Rocketborne experiments are much more adaptable to graduate student training than are satellite-borne experiments. A graduate student has a reasonable chance to participate in the conception, construction, flight, and data analysis of a rocket-borne experiment. This degree of participation is fast becoming practically impossible in the case of satellite research, possibly excepting the simplest kind.

The Working Group, therefore, makes the following recommendations:

(1) Over the next several years rockets will continue to be a primary x-ray exploration vehicle. More groups may be expected to participate in this important work and should be encouraged to do so. The number of rockets currently available—about six per year—is barely adequate for even the present active groups. At least a dozen rockets each year is considered essential for maintaining an appropriate pace of scientific progress and as necessary preparation for more ambitious satellite missions.

(2) As the work progresses, increased capabilities of rocket vehicles are required. An increased volume and weight-lifting capacity beyond the Aerobee class of rockets appears desirable for survey experiments. Existing military rockets should be exploited for this purpose.

(3) Greater accuracy of pointing is required to exploit the fine angular resolutions which can be achieved with currently available detectors. Pointing accuracies finer than 0.25° and small jitter rates are essential. Pointing accuracies of 1 min of arc have been achieved in developmental systems. Such systems should be made available for use in x-ray astronomy and their characteristics improved, particularly with respect to jitter or drift rates.

THE ROLE OF SATELLITES

Explorers

In comparison with larger more complicated satellites, small relatively inexpensive satellites of the Explorer class, which are often designed to carry out a single experiment, will continue to have an important role in x-ray and gamma-ray astronomy. These satellites offer flexibility in that an initial important discovery made from balloons or rockets can be followed relatively quickly with more refined observations from an Explorer. In addition, since an Explorer satellite often contains a single experiment or a small group of compatible experiments, greater design flexibility can be maintained during the program and a greater degree of control on the mission can be exercised by the experimenters, than in the more elaborate scientific satellite programs (though not greater than in rocket and balloon flights). These same characteristics—relative flexibility, shorter lead times, greater scientific control—allow more opportunity for graduate student participation in this exciting field of research. For all these reasons, these satellites allow the greatest advantage to university groups.

Weight and space limitations probably exclude programmed pointing equipment from these vehicles. It would seem therefore, that this type of satellite will be most useful for surveys in which the modest detector areas can be compensated for by long exposure times. It is impossible to foresee the ingenuity that the Explorer type of satellite opportunity will stimulate. The Working Group, therefore, strongly endorses the continued encouragement of groups who wish to conduct their research in this more independent manner.

Pickaback Experiments on the Saturn Instrument Unit

Experiments carried aloft while attached to the instrument unit of the Saturn rocket offer advantages intermediate between those of rockets and Explorer-type satellites. The experiment module can have a very simple interface with the spacecraft electronics. The experiment orbits the Earth in operation for about five hours and remains under the orientation control of the Saturn guidance system. Important advantages offered by this procedure are: (1) the small incremental launch effort; (2) the relatively large number of anticipated launches, spaced by a few months; and (3) the possibility of pointing at specified objects.

Orbiting Astronomical Observatory (OAO)

Only two x-ray experiments and two gamma-ray experiments are scheduled for flight on the series of OAO spacecraft authorized to date. All of these experiments were conceived as pickaback or back-up devices and are in no sense really matched to the OAO characteristics. The Working Group therefore recommends that:

(1) Proposals for x-ray experiments suitable for inclusion as primary OAO experiments be considered as possible substitutes for already accepted OAO experiments in the more conventional fields of astronomy;

(2) The first unassigned OAO be set aside for x-ray observations, to be supplemented if possible by much lower priority gamma-ray observations;

(3) The OAO pickaback opportunities be recalled to the attention of the scientific community and suitable experiments in x-ray or gamma-ray astronomy be accommodated. If at all possible, a minor rearrangement of spacecraft components should be performed in order to permit use of optical focusing devices with focal lengths comparable to the length of the OAO.

Supernovae are believed to occur in our Galaxy at a rate of about 1 per 100 years. They are, according to present theories of nucleogenesis, the source of all heavy elements. But observational support for this important conjecture is lacking. The Working Group believes it is important that x-ray and gamma-ray instruments be available for supernova detection, should such a phenomenon occur in our galactic neighborhood. Exisiting programs (the NRL-NASA solar x-ray monitoring satellite, x-ray and gamma-ray detectors on the OSO, OAO, and OGO series of satellites) offer incomplete spectral coverage with uncertain probability of supernova data recovery. The Working Group feels that a promising practical program for passive supernova x-ray and gamma-ray data recovery would involve a small "universal" package containing an x-ray detector sensitive in the 5-8 Å region and a gamma-ray detector capable of energy resolution in the 50 keV - 3 MeV region. Both detectors should be sensitive over a wide angle, and small and simple enough to permit their inclusion on enough spacecraft so that all parts of the sky are covered at least 80% of the time. The Vela Hotel Project, for example, could perhaps be requested to add instruments of these kinds to their standard orbital packages.

The Working Group therefore recommends that support be made available for the design and construction of a prototype universal supernova monitor, and that serious consideration be given for the inclusion of this monitoring device on many future spacecraft. Two levels of sensitivity would be desirable in such a package: one level, sensitive to only very high flux values, would be appropriate to any supernova that occurs in the local Galaxy; the other more sensitive channel would be appropriate for the more numerous supernovae occurring in other nearby galaxies.

6. OBSERVATIONS MAKING USE OF MAN IN SPACE

The Working Group has considered many aspects of the possible use of man in making x-ray and gamma-ray observations in space. The following conclusions emerged from our deliberations:

(1) There are certainly observations in which man will be helpful and even necessary. Two concerns, however, were expressed.

(a) The effort and expenditures for experiments of the type contemplated are great. An appreciable fraction of an investigator's useful life may be invested. It was felt that if an experiment is accepted for a manned mission, some assurance that it would be regarded as an essential objective of the mission is necessary to establish an atmosphere in which scientific and manned space flight interests can be successfully joined.

(b) The cost of manned ventures is very great and can only be charged to the broad national interest in the conquest of space. But it is inevitable that scientific missions will have to bear some of the responsibility for justifying the cost and it is therefore essential that only experiments that are clearly excellent be accepted for Gemini and Apollo Extended Systems.

(2) The Working Group recognizes that it cannot and should not attempt to design and present specific experiments. Nevertheless, we further recognize that only through discussing specific experiments can the general nature of our task emerge. There are clearly many technical operations for man to perform, such as assembly of components and erection of structures, as well as the carrying out of observing routines according to prescribed programs. Such tasks require a trained technician or engineer. Initial observational programs are expected, however, to require responses to be programmed in detail in advance. These require scientific judgment and initiative on the part of the astronaut.

(a) Large area (100 square meters) x-ray detectors equipped with mechanical collimators appear to be the optimum system for certain types of surveys and for occultation measurements. Such a detector could be carried aloft in pieces, assembled, and placed into orbit with its own power supply, guidance, and telemetry system.

(b) Focusing x-ray telescopes appear to be the most promising detectors for detailed studies of source structure and for high-resolution surveys. Large instruments of this type would best be assembled in orbit by a man. Photographic recording has many advantages over electronic methods when large amounts of data must be accumulated, and here man has the potential role of rendezvousing with the telescope to retrieve and replace film, as well as to repair and adjust the telescope.

(c) Gamma-ray detectors for photons with energies of 100 MeV and greater will require very large instrumental components such as spark chambers and Čerenkov counters. The need to assemble them in space is not so clear as it is for x-ray instruments, but their complexity and high initial cost may possibly warrant repair and adjustment by rendezvous with man in orbit.

(d) In the 1 MeV or nuclear gamma-ray region, detectors of modest proportions must be located far from massive objects and pointed in a particular direction (e.g., the Crab Nebula). Complex arrays of these detectors could be assembled, coarsely oriented, and occasionally calibrated by an astronaut.

(e) An x-ray astronomy observatory based on the Moon would benefit from all the obvious advantages of a stable base and the possibility of long-term tracking of sources. The demands on man are similar to those analyzed in great detail in such comprehensive reports as the LESA study ("A Study of Scientific Mission Support of a Lunar Exploration System for Apollo", final report, North American Aviation, Inc., 1965). During the X-Ray and Gamma-Ray Working Group study, Moon-based observations were not considered in detail except for the possibility of the occultation studies described under Nonfocusing X-Ray Optics in Section 4. In that type of observation, man would have several tasks, e.g., to calibrate the x-ray observations by timing the occultations of visible stars with an optical telescope; to determine with auxiliary x-ray detectors the azimuthal position of the occulted source; and to exercise judgment in altering the geometry of the occultation apparatus to gain detection sensitivity at the expense of resolution, or vice versa.

Not only can lunar occultations on the Moon be used to considerable advantage for x-ray observations; such techniques will evidently also be extremely effective in locating discrete sources of high-energy gamma radiation.

APPENDIX: LIST OF PARTICIPANTS

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VI
Physics and Geophysics

1. INTRODUCTION

TASK OF THE WORKING GROUP

The task of the Working Group on Physical Sciences was to review those physical experiments of a basic nature that could be advantageously undertaken in space. These were to be assessed as to their importance, while both short- and long-term technological requirements and practicability were to be borne in mind. The Working Group, by and large, excluded other fields covered in the Summer Study; consultants (see Appendix) were invited to discuss certain specific topics. Thus, the Working Group was almost wholly concerned with the question: "What would physics be like if it had developed in a space environment?"

Note was taken of the main characteristics of the space environment: (1) low temperature, (2) low pressure, (3) weak magnetic field, and (4) zero-gravity field. Some fourteen typical areas or subjects were surveyed: surface physics, plasma physics, low-temperature physics, high-energy physics, magnetohydrodynamics, fluid dynamics, experiments in a weak magnetic field (a few gammas), experiments in a zero-gravity field, experiments in an atmosphere-free regime, relativity experiments, gravitational experiments, fundamental physical constants, antimatter, and planetary investigations dependent upon the establishment of an equipotential reference datum in space. The Group did not consider experiments to map the distribution of magnetic fields and particles in interplanetary space and the more immediate surroundings of the Earth, since these appeared to have been adequately covered in the report of the 1962 Summer Study at the State University of Iowa ("A Review of Space Research," NAS-NRC Publ. 1079, 1962) and subsequent reports, and since NASA has a vigorous program in support of such investigations.

SUMMARY OF DISCUSSIONS

The types of experiments useful to conduct in space should capitalize upon the ambient conditions—zero gravity, low temperature, low pressure, and weak magnetic background. Although many types of experiments can be proposed, most can be coped with at or near the Earth. Thus, for example, one might immediately think of surface-physics investigations, including catalysis, in the ultrahigh vacuum beyond the atmosphere. However, ambient pressures of 10^{-10} torr, at which the formation of a monomolecular layer on a clean surface takes about half an hour, are readily obtained by pumping; and partial pressures of 10^{-12} torr in specific containers are achieved by gettering. Similarly, balloon altitudes are adequate to satisfy the requirements of high-energy physics in the study of nuclear reactions of cosmic rays.

Such considerations lead one to look to combinations of features of the environment. Two that appear of interest are (1) optical experiments requiring a very long path in vacuo and (2) plasma-physics experiments requiring a very large volume in vacuo.

On the whole, four types or sets of experiments appeared valuable and feasible: relativity and gravitation, physical constants, plasmas, and geophysical height sensing.

Although many significant fields of investigation were recognized and discussed by the Group, the actual number of experiments regarded as feasible over the next decade or two was relatively small. These experiments were classified on the basis of their value to fundamental physics, as having significant value for application, or as having interest but no recognized basic value or application at this time. They were also classified in terms of their specific need for a space environment, their utilization of man in space, if any, and the type of support required.

Relativity and Gravitation

The principal experiments having basic importance in physics lie in the fields of relativity and gravitation. Although most of these experiments have been considered previously by other study groups, the following six were selected for discussion on the basis of their differing modes of approach:

1. The use of the asteroid Icarus or a special satellite for determining the relativistic rotation of the line of apsides (perihelion rotation) of a planet or asteroid; its use to determine the contribution of solar oblateness to the motion of the line of nodes of the orbit.
2. Gravitational deflection of light.
3. The precession of a spinning body in space.

These three experiments represent the complete body of relativistic effects realizable in the near future and capable in principle of distinguishing between (a) Einstein's rather specific Theory of General Relativity and (b) the more general theory of relativity, for which part of the gravitational effects are due to a scalar field. The determination of the Sun's oblateness is essential for the unambiguous interpretation of the excess perihelion rotation.

4. The secular change in gravitational constant through laser tracking of corner reflectors on the Moon to determine lunar deceleration. The experiment would also have geodetic value and geophysical applications to the study of the Moon's interior through studies of its libration. It is also conceivable that this experiment might permit the detection of gravity waves.

5. The use of a time-keeping satellite for studying changes in gravitational interaction.

6. An Eötvös experiment in space for studying the relation of inertial and gravitational mass.

Physical Constants

Experiments leading to the more precise definition of physical constants were discussed. The following were considered to be of interest at this time:

1. The determination of a better value of the gravitational constant through a Cavendish experiment in space. Although this is a dimensional constant, it is important for establishing the mass and gravitational attraction of the planets.
2. The determination of a better value for the velocity of light. Although the uncertainty in this value is of small consequence on the Earth, the degree of uncertainty is of sufficient magnitude to be important in measuring astronomical distances. No specific approaches are noted, and, indeed, the question as to whether space affords prospects better than the Earth remains open.

Plasmas

Space provides a natural environment for studies of plasma physics that are precluded in laboratories on the Earth. In terrestrial laboratories, the probes employed for measuring the properties of the plasma introduce disturbing effects; these effects are obviated by the much greater scale size of the phenomena produced by nature in space. The turbulent structure in the fluctuating transitional region between the uniform interplanetary magnetic field and the magnetosphere affords unique opportunities in the study of collisionless shock-wave phenomena.

Coordinated measurements of the characteristics of solar plasma beyond the magnetosphere, or of the properties of the ionosphere, could be obtained with manned spacecraft that send out maneuverable subcapsules. The distance between the units could be varied at will, and would be governed by the observations. The detailed topography of the ionospheric structure and airglow phenomena could be investigated with this technique. Transient effects, such as sporadic E, would be susceptible to study by laboratory techniques. The propagation of artificially produced plasma could be observed, and it might be possible to affect naturally occurring plasma with waves produced by nonnuclear explosions, by sparks, or by other means.

The behavior of charged particles in magnetic fields, including particle interactions, is far from understood. Much of the work in the magnetosphere is concerned with this subject. Aside from observation and measurements, actual experimentation is possible, as demostrated by the artificial injection of charged particles; radioactive tracers may also afford a useful technique in this work.

Geophysics

If it were possible to develop a precision microwave altimeter and gravity gradiometer, it would be possible to establish an equipotential reference datum in space that would permit a significant breakthrough in many geophysical studies of the Earth. Moreover, the technique would have a direct bearing on the understanding of similar later studies of the Moon and other planets.

Other Experiments

In the course of considering fundamental questions and major problems or techniques, a variety of other experiments entered the discussion. These generally were of a rather more specialized nature. A few are noted here: the interrelations of solid and liquid and gas phases of materials in a space environment, the collection of cosmic dust and micrometeoroids, and the exposure of emulsion plates for cosmic-ray detection. As these experiments have been reviewed in connection with the manned space program (see "Summary of Biomedical Experiments Presently Planned for the Manned Space Flight Program" in Chapter 10), they were not considered further.

The Lagrangian points were discussed, in part because man might be able to observe whether there is, in fact, an accumulation of planetary material at these equilibrium positions. The general topic, examined three years ago at the Iowa study ("A Review of Space Research," NAS-NRC Publ. 1079, 1962, pp. 4-34—4-45), was reviewed. Further theoretical work since that time has yielded additional information on the nature of the stability; no experimental verification of dust accumulation has been obtained. Photography of the triangular Lagrangian points from Gemini and Apollo flights appears feasible.

Such features of space as high vacuum, gravity-free condition, and low temperature prompted speculation on various possible experiments. Two are noted here: (1) an intermolecular scattering experiment, in which a beam of particles is scattered by a gas sample at very low energies (~1 eV); (2) Rayleigh instabilities, using the classical Bénard cell experiment in the gravity-free environment, where an artificial gravitational field would be applied so as to permit a study of instability from onset.

FINDINGS AND RECOMMENDATIONS

Insofar as space studies of fundamental significance in physics are concerned, the Working Group found that relativity and gravitation are the critical subjects. Some work is under way at present; this merits continued attention and support, and, accordingly, the Working Group adopted the following recommendation:

Recommendation 1. The Working Group considers experiments in relativity and gravitation as the most important fundamental ones in physics and recommends their support and conduct as soon as practicable.

Because a precise height sensor and gradiometer afford prospects for measuring a host of significant physical parameters of the dynamic Earth, which might then be applied effectively to the study of other planets, the group concluded that the development of instrumentation was important. To this end, and in the hope that available equipment will be used to begin the program, the following recommendation was adopted:

Recommendation 2. The Working Group recommends (a) that the development of a suitable, high-precision radar altimeter be tackled vigorously, (b) that existing equipment be used on presently available space vehicles to determine the nature of operational problems inherent in the conduct of measurements with a precision of 1 to 2 cm, (c) that the problem of a suitable gravity gradiometer be examined, and (d) that related studies, now under way, for the more precise determination of orbital constants be continued.

Because the possibility of placing corner reflectors on the Moon opens up prospects for a variety of experiments, a third resolution was adopted by the Working Group:

Recommendation 3. The Working Group recommends that, since a set of corner reflectors on the Moon would facilitate a number of significant scientific investigations, NASA should incorporate the installation of such reflectors on the Moon's surface as part of the lunar landing program.

Moreover, some consideration might well be given to the use of reflectors on long-lived satellites.

In common with other working groups in the Summer Study, the Working Group on Physical Sciences reviewed the findings of the ad hoc Committee on NASA-University Relations (the Simpson Committee). The Group concurred in the findings of this committee and urged favorable consideration of the recommendations by the government. In its own study of the general subject, as stimulated by the Simpson Committee report, the Working Group adopted the following resolution:

Recommendation 4. The Working Group recommends that the support of ground-based experiments, and of balloon and rocket programs, be expanded to provide information which is essential to supplement observations carried out with spacecraft or which cannot be obtained by spacecraft. Proper support of this kind is totally justified by scientific and economic considerations. Moreover, an adequate effort would yield significant by-products: it would, in general, permit continuity of university participation in space research and, in particular, encourage the training of graduate students by affording means for following through a space-related research problem from conception to completion.

Again in common with other working groups, the Working Group on Physical Sciences considered the uses of man in space for scientific purposes. Most of the experiments noted by the Working Group lie in the future, do not depend on a man as a manipulator in space, but do depend on further technological developments. Because of this, no formal recommendation is needed, although the general discussion led to some findings.

First, in general, research in space can be characterized in three ways: (a) most of it can be done by remote-controlled or automatic devices; (b) there are, however, some problems that will eventually require man, although science by itself could not justify the costs of manned effort; but (c) if the national interest continues to call for the development of manned capabilities in space, then the most rewarding by-product of the enterprise probably lies in the pursuit of problems as characterized above in, and relating to, (b). Typical of the kinds of major problems to which a man can contribute, because human discrimination, judgment, and guidance appear crucial, are planetary investigations, emplacement and adjustment of large telescopic systems, and some plasma studies.

But, second, although the large cost of space activity rules out the conduct of what we might call secondary or minor problems, these can merit pursuit as incidental to already planned and otherwise justified manned missions, for they can represent valid effort (a) as experiments of interest and (b) as part of the general acquisition of manned space capability. Thus, there are useful experiments under consideration by NASA for manned flights in Earth orbit: most of these relate to medical and physiological subjects of obvious operational value; some relate to engineering problems useful in space technology; and a few take up limited scientific topics, feasible within the framework of the Earth orbit program.

Third, and last, in space as on Earth, scientific investigations will call upon man and device as necessary to the research goal. Accordingly, the distinction or separation of space research into a manned category and an unmanned one is artificial. This suggests that the unity of the scientific effort be maintained, allowing the nature of the problems and economics to dictate how a given task can best be carried out.

2. POSSIBLE EXPERIMENTS IN SPACE

RELATIVITY AND GRAVITATION

The techniques of space are uniquely suitable for research on gravitation and relativity. The very great weakness of gravitation requires large bodies (astronomical in size) as sources of gravitation. Rapidly moving detectors are also needed to measure it. Space-curvature effects are important only over vast distances, and real research in this area can be produced by the physicist only if he is willing to stop viewing himself as a laboratory-bound creature. Perihelion rotation, light deflection, and precession of a spinning top constitute the complete body of relativistic effects realizable in the near future and capable in principle of distinguishing between (a) Einstein's rather specific Theory of General Relativity and (b) the more general theory of relativity, in which part of the gravitational effects are due to a scalar field.

Perihelion Rotation

One of the two most important experiments is concerned with the relativistic rotation of the line of apsides (perihelion rotation) of a planet or planetoid. It has been pointed out (Dicke, 1964) that, because of an unknown contribution from a possibly oblate Sun, this relativistic effect has an uncertainty in excess of 20 per cent. Inasmuch as the uncertainty in the gravitational deflection of light is just as great, there is not yet a single test of the Theory of General Relativity per se that is more accurate than 20 per cent. This statement should not be misunderstood.

It refers to Einstein's specific theory of gravitation, "General Relativity," and not broadly to all generally covariant relativistic theories.

A slightly flattened Sun having an oblateness of $\Delta R/R = 5 \times 10^{-5}$, would rotate the perihelion of Mercury at a rate that is about ten per cent of Einstein's value. This effect falls off with a higher power of r than does the relativistic effect, and the two contributions would be separable in principle by combining data from two planets.

1. The asteroid Icarus will pass relatively close to the Earth twice a year for the next three to four years. It is an almost ideal object for investigating both the relativistic effect and the solar oblateness. Its eccentricity and inclination are large, making the motion of the node and the perihelion large, and observation easy. The motion of the node provides a good test for the oblate Sun, and the motion of the perihelion is sensitive to both the oblateness and relativistic effect. If the asteroid were tagged with a radar transponder, a precise measure of its orbit could be obtained.

2. An artificial planet moving in an elliptical orbit could also be used to observe the relativistic perihelion rotation. There are two necessary conditions: (a) the accuracy of the range measurements must be sufficiently high and the eccentricity of the orbit sufficiently great that the necessary precision can be obtained in the lifetime of the vehicle; (b) either the average density must be sufficiently great that uncertainties in gas drag and light pressure are of negligible importance, or the vehicle must be equipped with a system of accelerometers and gas jets to cancel out these extraneous forces.

Concerning precision: standard radar-transponder techniques should be adequate. For a planet with a period of about one year and an eccentricity of about 0.2 - 0.5, the line of apsides rotates $\sim 2 \times 10^{-7}$ radian/year because of the relativistic effect. This represents a motion of the aphelion of about 30 km/year, well within the capabilities of present techniques for range measurements. It is evident that a useful life of two to three years might be sufficient for the observation.

The uncertainty in the radiation pressure presents something of a problem but not an extremely difficult one. For a reflecting spherical planetoid of radius r(cm) and density ρ(g/cm^3), the ratio of light pressure to gravitational pull by the Sun is $1.2 \times 10^{-4}\ 1/\rho r$. This force has essentially zero effect on the motion of the line of apsides as long as the heliocentric aspect of the planetoid and its albedo are not correlated with solar distance. This radiation-induced force should be inverse square to an accuracy of a part in 10^6 for $\rho r \sim 1$. For a large, high-density planetoid ($\rho r \sim 200$) the required accuracy is two parts in 10^4.

The pressure induced by the solar wind on the planetoid is much smaller than light pressure, but unfortunately it is not an inverse-square force and it is variable. This force is less than 10^{-3} of the radiation-induced force, but it may be more difficult to deal with.

It is concluded that a dense, sufficiently large planetoid would yield a measure of the relativistic rotation of the line of apsides. However, a better approach might be to add an accelerometer and set of gas jets to servo-balance these extraneous forces to zero. Such a device, called a "zero-g satellite," is now being designed by R. Cannon and collaborators at Stanford University.

Light Deflection

The accuracy of the well-known light-deflection test of General Relativity is at present very poor because of the necessity, up to the present, of an eclipse of the Sun to make the observations. The uniqueness of the event at a given site precludes the necessary careful study of systematic errors while the eclipse is taking place; thus, little reliance can be placed on these observations.

There appear to be at least two ways in which the gravitational deflection of light (or its equivalent) might be determined using space techniques. First, the Sun, and its surrounding star field might be photographed above the atmosphere without the intervention of a solar eclipse. Second, an artificial planetoid might carry a radar transponder or a precision oscillator behind the Sun to permit an absolute determination of the retardation of electromagnetic waves in passing the Sun. The latter is similar to the suggestion of Shapiro, (1964), to use ground-based radar to monitor an interplanetary distance as Mercury or Venus passes behind the Sun.

1. Photographic or photoelectric means of determining the gravitational deflection of light would consist of a camera with an aperture of 5-10 cm diameter, a photographic plate or film (or preferably an image-storing iconoscope), and a quartz flat that could be interposed in front of the objective to photograph a comparison star field on the same film. Photographs would be made every four hours for two or three days. Each photograph would comprise two separate exposures, with the same comparison star field being used on all photographs. It would be necessary to use a Lyot optical system or some system designed to reduce internal scattering. It would also be necessary to stabilize the camera carefully during the exposures.

2. Light retardation in passing the Sun. The radar method suggested by Shapiro is so eminently reasonable that there may not be a need for a competitive approach. However, in an alternative approach, one would fly a precision atomic clock on an artificial planetoid and follow its apparent frequency, radiated to the Earth, as it moves behind the Sun and reappears. Alternatively, a radar transponder could be interrogated from the Earth. To avoid confusion of the interpretation by the retardation caused by the solar wind, it is desirable to work at short wavelengths (under 6 cm), and there may be advantages of the foregoing approaches over straight radar at these wavelengths.

A Spinning Top

It has been suggested by Schiff (1960), that the relativistic precession of the axis of a spinning top, discussed a half-century ago by de Sitter and others, might be detected by using a spinning artificial Earth satellite and monitoring the motion of the spin axis. The expected precession of about 7 sec of arc per year is composed of two parts: (i) the de Sitter precession due to space curvature, and (ii) the precession, arising from the tendency of a rotating mass (the Earth) to tug inertial coordinate frames around with it. This latter effect is particularly interesting because of its close relation to Mach's principle.

Based on a suggestion of Schiff's, Fairbank and Everitt (Stanford) have been designing an experiment that makes use of a cryogenic gyroscope and readout system, and have been engaging in extensive experimentation. The experiment might also be performed using a brute force approach: flying a large dense artificial satellite and monitoring the orientation of its spin axis from the ground. This approach, first suggested by Knoebel (Illinois), also provides some interesting possibilities, and is being studied at the University of Illinois.

Corner Reflectors on the Moon

By landing one to three optical corner reflectors on the Moon, a large number of observers in many countries would have a tool for a variety of interesting and fundamental investigations of the Moon. These include relativistic factors affecting its motion; factors involving the Moon's interior, its rigidity, and its figure; and the excitation and damping of the Moon's physical libration.

Distance to the Moon. A particularly exciting prospect is the establishment of optical bench marks on the Moon—bench marks of long life whose value would increase with age. It is certain that increasingly more advanced lasers will be developed. Unless we start now, there will be no well-defined fixed points on the Moon to look at with these greatly improved lasers. There appears to be no fundamental reason why a measure of the distance to the Moon accurate to one part in 10^{10} could not ultimately be obtained. However, such precision would be meaningless without at least one well-defined fixed reference point.

Secular and Tidal Accelerations. The long life possible for such bench marks is particularly important when problems involving the lunar orbit are considered. The accuracy with which the secular acceleration of the Moon's longitude can be determined is proportional to the 2.5 power of the observation period. Points on the Moon are so ill-defined that at present 50 years of observation are required to determine its secular acceleration with any precision. The greatly improved accuracy that corner reflectors provide would permit the reduction of this observation period to a few years. Thus, it would become possible to determine the tidal acceleration in a comparatively short interval of time.

Gravitational Constant. A fundamental relativistic question concerns the possible existence of a zero-mass scalar field as part of gravitation. If such a field exists, gravitation should be steadily weakening as a direct result of a secular increase in the magnitude of the scalar field, a cosmological effect of the expanding Universe. This, in turn, would imply a secular slowing of the motion of the planets and the Moon (as measured by an atomic time scale). The classical astronomical determination of the lunar acceleration is based on an ephemeris time scale. Thus, a new determination would permit a comparison of the atomic and ephemeris time scales and hence would expose a secular change of the gravitational constant, if it should exist. The expected change (if it exists at all) is an increase of the lunar period of 2-6 parts in 10^{11} per year.

Geodetic Application. Geodesy provides another important application. The distance of any observation station on the surface of the Earth from the axis of rotation can be determined from the observations.

Libration. The physical libration of the Moon could be deduced from the greatly improved measure provided by the distance to three corners. The physical libration, in turn, would provide information about the principal moments of inertia, rigidity, and internal damping of the Moon.

A better assessment of the value of tracking corner reflectors to obtain improved estimates of the physical librations (and hence the principal moments of inertia) should be possible after improved determination of the Moon's gravitational field is obtained from the Lunar Orbiter Satellite, now planned for 1966. Such a satellite may answer the question of whether the present unreasonably large moment of inertia results from an error in $(C-A)/MR^2$ obtained in part from the Moon's orbit.

Gravitational Waves. One interesting relativistic phenomenon of great importance that might show up in sufficiently accurate range measurements to points on the Moon would be the presence of gravitational waves. They might be of the conventional tensor type, or they could appear in the above-mentioned scalar field. These waves might be detected if they were occasionally substantially stronger than suggested by elementary considerations of average energy density.

The existence of tensor-type gravitational waves and radiation are directly predictable from the General Relativity Theory (Maller, 1952; Forward, 1961) and are currently being studied by Weber at the University of Maryland. It has also been postulated recently that gravitational radiation is quantized (the graviton) and possesses the following characteristics: (i) the radiation is of quadrupole type; (ii) velocity of propagation is the velocity of light; and (iii) gravitational waves result from the acceleration of masses.

Unfortunately these effects are exceedingly small and are quite difficult to generate or detect. For example (Maller, 1952), a one-ton rotating mass quadrupole with a rotational frequency of 10,000 rpm would radiate energy at a total rate of only 4×10^{-33} watt. In addition to the problems of constructing such large radiators, spurious vibrations and background noise must be considered.

Numerous experiments have been conducted recently by Weber in an attempt to detect and measure gravitational radiation. Thus far, there has been no empirical verification. The major problem is mechanical background noise introduced through the supports for the detector. The environmental conditions available in a zero-G laboratory aboard a space vehicle or satellite would greatly eliminate this source of noise and would allow the construction of detectors with a much lower operating frequency than those operated on the Earth.

Because the gravitational radiation expected from astronomical sources is expected to be peaked in the very low-frequency region, it is desirable to build detectors with as low an operating frequency as possible. On Earth, however, the noise spectrum due to ground motion also is highly peaked at low frequencies, and it is almost impossible to eliminate these from the supports by filter techniques now being used by Weber at the higher frequencies (1600 cps).

Corner-Reflector Size. How big should the corner be? An accurate (diffraction-limited) corner cube has an effective area proportional to the fourth power of the length, and for a corner 25 cm on a side its effective area would be enormous ($\sim$400 km^2). The light returned would be much greater than the diffuse reflection from the Moon.

Moon Quakes, Libration. An interesting application of the technique would require two or three somewhat smaller corners placed on the Moon's surface a few meters apart. Interference between the two corners would depend upon tilt. Thus, the return signal would be sensitive to tilt of the Moon and would be responsive to Moon quakes as well as to libration.

Clock Rates in Widely Different Gravitational Potentials

According to the general theory of relativity the interval between two successive beats of a clock located in a gravitational potential $V = \int \rho \, d(vol)/r$ is increased by the factor $1 + V/c^2$ relative to the interval for a similar clock in a local inertial coordinate system for which the gravitational field vanishes. As applied to the frequency or wavelength of lines in the spectrum of a star as observed from well outside the star, this effect is the well-known "gravitational red shift." Experiments to measure this effect by comparing the rates of two accurate atomic clocks, one on the Earth and one in space, were proposed during the first years of the space program. But before the proposals could be carried out, Pound and Rebka (1960) performed their well-known experiment, using a gamma-ray emitter and absorber for which the width of the line was sharpened to about 10^{-12} by the Mössbauer effect. The difference in height between the two elements was about 22 m, corresponding to a frequency shift $V/c^2 = 2.5 \times 10^{-15}$. Although they had to apply corrections for other effects much larger than the shift they were looking for, they obtained a result with a 1-σ uncertainty of only ten per cent which differed from the theoretical value by only five per cent. The precision of their technique was comparable to that estimated for the proposed orbiting clock experiments, and their experiment was of course considerably less expensive. Their result is still the most accurate existing confirmation of this particular prediction of general relativity.

Meanwhile, however, the development of atomic clocks, such as those based on the gases of rubidium, cesium, and thallium, has proceeded so far that a considerably more precise experimental check of the gravitational shift can now be performed, taking advantage of a potential difference much larger than that available to Pound and Rebka. Unlike Mössbauer clocks, atomic clocks can be compared by scaling their frequencies and telemetering the accumulated readings over long distances. Present-day rubidium-vapor clocks are stable to one part in 10^{11} over a period of weeks. The frequency shift of a clock far removed from the Earth compared with one on the ground is given by $V/c^2 = 7 \cdot 10^{-10}$, which is seventy times greater than the relative drift of the clock rates; this fact implies that a greatly improved red-shift measurement could be made with an appropriate satellite as soon as suitable flight equipment can be developed.

Timekeeping Satellite

To look for a gradual weakening of the gravitational interaction (Brans and Dicke, 1961), it would be desirable to have a reasonably short-period, high-density satellite with 12 to 24 precision quartz corner reflectors (say, 5 cm on a side). The satellite might be a 500-kg sintered tungsten ball, and it should fly above the Earth's atmosphere at a gas density of about 20 particles/cm^3. It should be accompanied by another satellite also carrying corner reflectors and moving in almost the same orbit. The companion satellite would be constructed to present the same external appearance but would be, say, only one-third as dense as the principal satellite. Thus, the differences in motion could be used to evaluate the effect of gas drag and light pressure on the principal satellite. The two satellites would be launched together and later pushed apart very gently by a light spring. Alternatively, a single timekeeping satellite could be constructed as a hollow spherical shell of high density, containing a small untethered gold ball at the center, with a capacitance bridge and electrostatic accelerometer to monitor the gas drag and light pressure. Telemetry would be used to transmit the information to the ground.

Eötvös Experiment

The concepts of inertial mass and gravitational mass are very differently defined, but their identity, which is postulated by the general theory of relativity as one aspect of the "principal of equivalence," has been demonstrated by terrestrial Eötvös experiments to within three parts in 10^{10}. The equivalence of gravitational and inertial mass is not subject to test with natural astronomical bodies but could be tested with artificial bodies of known mass.

Imagine two bodies of different materials placed in orbit with identical initial conditions. If the ratio of gravitational mass to inertial mass for body A is identical with the same ratio for body B, difference in their trajectories would be detected; if, however, the ratio differed by $1{:}10^9$ for the two bodies (still with the same initial conditions), differences of this same order of magnitude would develop between the semimajor axis and periods of the orbits of A and B. On the scale of typical orbits of artificial Earth satellites, there would develop a difference of the order of a centimeter or so between the semimajor axes, and a difference of a few microseconds between the periods.

Although present-day techniques may be inadequate to detect effects as small as these, that situation will not always prevail; furthermore, the idea behind such an experiment is so simple that the possibility of performing it should be continuously re-examined as techniques improve. Even if not detectable absolutely, differences in the orbital constants of the order of magnitude given in the example above might be detected differentially, by measuring the vectorial displacement of one body from the other as a function of the time. In particular, the difference in period would have a cumulative effect, so that one body would move progressively ahead of the other a few centimeters per revolution. One can imagine observations being made on the pair of bodies from nearby orbiting spacecraft, or inside a space laboratory. The most serious difficulties would presumably be those arising from very slight differences in the initial conditions, or from a failure to correct completely for other effects, such as those produced by the gravity-gradient forces caused by presence of the spacecraft and experimenter, electrostatic or other forces, those due to the minute but nonnegligible mutual gravitational attraction of the two test bodies, and those due to electrostatic and other forces of geophysical origin.

Our ability to carry out such an experiment will probably be marginal for some time; compared to it, the following alternative version of the Eötvös experiment was endorsed by the Group as being more nearly feasible.

Eötvös Experiment using an Eötvös Balance in Space. (Roll et al., 1964). This is an experiment in which there is a peculiar advantage in having available the untethered free-fall state. Let a body be constructed such that four spheres of equal mass are placed at the corners of a square, with two of the bodies made of gold or a similar high-Z material and two of light material (say, aluminum), and with diagonally opposite spheres being of different materials. In such a body, placed in a servo-controlled wind shield orbiting about the Earth, any small tendency for the heavy material to have a weight in relation to its inertial mass that is anomalous compared with the light material would cause the balance to rotate so as to move the heavy spheres up (or down) relative to the light spheres.

The advantage of four weights as compared with two is that this arrangement can be made free of a gravitational quadrupole moment. Two weights 20 cm apart would suffer a torque, caused by the gravitational gradient, that would be 3000 times as great as the limit, $1:10^{11}$, that has already been set.

The space experiment should be capable of an improvement by three orders of magnitude, to an accuracy of $1:10^{14}$, as a result of replacing the gravitational acceleration toward the Sun ($0.6\ cm/sec^2$) by that toward the Earth.

Cavendish Experiment

The currently accepted experimental determination of the gravitational constant is $G = (6.670 \pm 0.015) \times 10^{-8}$ dyne cm^2/g^2. Numerous speculations have been made regarding the universality and constancy of this number. The implications of possible long-term secular changes in this number, or of variations with local gravitational potential, are of extreme importance in astrophysics and cosmology. As a preliminary step to answering questions of this type, it would seem that a more accurate determination of the constant of gravitation should be made in the near-Earth environment. It is quite conceivable that performing a Cavendish experiment in a laboratory approximating zero-gravity conditions would allow a more precise determination.

As an example of the type of conceptual study that must be carried out to evaluate the possibilities for such an experiment, let us consider an experiment in a zero-gravity laboratory with two solid tungsten spheres of 4-cm radius set into near-circular orbits about their common center of mass under the influence of their mutual gravitation. If the mean separation of this artificial binary system is adjusted to approximately a = 10 cm, an appreciable departure from zero eccentricity can be tolerated without having the spheres touch. Since the density of tungsten is 19.3 g/cm^3, the mass of each sphere will be 5.18 kg. The period T will be given by

$$(2\pi/T)^2 a^3 = MG, \quad (1)$$

where a is the semimajor axis and M is the sum of the masses. Taking $a = 10$ cm, we obtain $T = 7.56 \times 10^3$ sec = 2.10 hours. Such a period does not appear inconvenient for making precise measurements of the relative positions of the spheres. As, for the first time, one would have the possibility of measuring the period of a binary gravitating system for which the masses could be accurately determined by comparing with laboratory mass standards, one could use the measured period and semimajor axis for a precise determination of G. This would reverse the procedure that is customarily applied to deduce the total mass of a binary stellar system from the known value of G.

The method of observing the relative positions of the spheres might depend on their size. For spheres of the size range discussed here, periodic photographs taken with precision optics could be used. The positions on photographic plates of certain fiducial marks provided on the spheres could be measured with an optical comparator if a manned laboratory were available nearby. Simultaneous photography of a star-field background could give a precise measure of the instantaneous orientation of the camera axis. Three cameras with approximately orthogonal axes would probably be used. Optical interferometric measurements of relative position and Mössbauer measurements of relative velocities might be considered if a means to overcome problems associated with rotation of the masses can be suggested. It should be easily possible to measure the direction of the line of separation relative to the fixed stars to an accuracy of 1° after $10{,}000^\circ$ of rotation (60 hours), and to measure the separation of the spheres to an accuracy of 0.01 mm. This would give T to one part in 10^4. According to Eq. (1) this would in turn give G to five parts in 10^4, which would represent a substantial improvement in the accuracy with which G is now known. A considerable improvement on the measurement accuracy for angular position and separation should be easily possible using optical systems of long focal length.

GEOPHYSICAL APPLICATIONS BASED ON AN ORBITING MICROWAVE ALTIMETER AND GRADIOMETER

The geocentric radius to the surface of the land and sea is a changing quantity at each point on the Earth. Tidal forces, wind stress, and barometric pressure constantly remold the sea surface. Erosion, tectonic events, glacial accumulations, and internal adjustments remold the surface of the land. These dimensional changes can be progressive, cyclic, or intermittent, but each has an explanation and significance in furthering understanding of physical processes at work within the solid Earth, oceans, and atmosphere.

World-wide surveillance of these effects might be provided by an orbiting microwave altimeter and gradiometer.* Given sufficient experience in the analysis and interpretation of terrestrial events, the technique should also prove valuable in the examination of other planets. With an orbiting, precision height sensor and gradiometer we might hope to measure:

(1) ocean surface waves, including tides and tsunamis;
(2) the patterns and transports of primary ocean currents;
(3) the atmospheric pressure (and winds) at sea level;
(4) redistributions of mass within the lithosphere;
(5) eustatic changes of sea level or land level;
(6) volumes of snow and ice accumulation on land;
(7) density of foliation in forests and grasslands; and
(8) the figure of the Earth and its gravity field.

The dimensions and frequencies of relief events to be expected in each of these physical categories are:

	amplitude (cm)	scale (cm)	frequency(sec^{-1})
(1)	0 to 2×10^2	0 to 10^8	10^2 to 10^{-5}
(2)	0 to 10^2	0 to 10^7	10^{-5} to 3×10^{-7}
(3)	0 to 10^2	10^5 to 10^8	10^{-3} to 10^{-5}
(4)	0 to 10^5	10^7 to 10^9	10^{-5} to 10^{-11}
(5)	0 to 10^4	10^8 to 10^{10}	10^{-7} to 10^{-9}
(6)	0 to 10^5	10^7 to 10^9	10^{-5} to 10^{-7}
(7)	0 to 10^4	10^7 to 10^9	10^{-5} to 10^{-6}
(8)	0 to 10^3	10^7 to 10^9	10^{-4} to 10^{-8}

For dynamical interpretation of items 1 through 5, measurements of relief would be most valuable if made with reference to an equipotential surface at or close to the geoid. In this case, we must redefine the geoid as that equipotential defined by the surface of a motionless ocean, under uniform atmospheric pressure, in which density is either uniform or a function of pressure alone.

To be effective, observations of relief for oceanographic and meteorological purposes would have to be responsive to changes of 1 cm. For example, the hydrostatic response of the sea surface to a change of atmospheric pressure is 1 cm/mb. Correspondingly, the rise of sea level across the width of the Gulf Stream is about 10^2 cm in middle latitudes. Other categories would be easily accommodated if this amplitude sensitivity were to be realized.

* See note, p. 6-19.

Proposals of this type for a satelliteborne altimeter have sometimes been rejected on the grounds that the satellite orbit is not known with sufficient accuracy. While it is true that the best orbit determinations are currently no better than ±80 m along the track and ±40 m across it, most of this error has a much longer wavelength than the wavelength of the geophysically interesting variations to be measured by the altimeter. For example, the predominant error in height would have an amplitude of about ±40 m and a period equal to the satellite orbital period, with a variation from one revolution to the next of not more than 5m. With a continuous measurement of height and a reasonably careful analysis of the measurements, variations in height with a period shorter than one-third the orbital period (corresponding to wavelengths shorter than 13,000 km) should be distorted by orbital errors by no more than a few centimeters.

Frequency discrimination is most sensitively limited by tsunamis and tides, where the effects of aliasing of time series of surface elevations can be very serious. An orbital period of 90 min would be suitable for tides except that cyclic repetition of measurements at any given place on the Earth would lead to time series at 12- or 24-hour intervals owing to Earth rotation. Sun-stationary orbits would emphasize the lunar tide, while a lunar-stationary orbit would emphasize the solar tide. These simplifications might be useful.

Aliasing through choice of orbit is less of a problem in all other observational categories, for which daily sampling would delineate the progress of change of all processes except step functions such as faulting. Measurements of surface-height changes would be useful to a degree, but would be much more meaningful if they could be referred to some reference geopotential surface.

From the measured tangential velocity of the orbiting altimeter one may find its potential within an arbitrary constant of integration. Having the instantaneous potential and the height of the Earth's surface beneath that place, the potential of the Earth's surface may be found to a linear approximation. Whether a linear approximation is sufficient or not depends on the ratio of the height of the orbit to the local geocentric radius of the Earth and the local deviations of the Earth's gravity field from spherical symmetry. This question needs careful examination in terms of potential theory and of the possible use of a gravity-gradient sensor in addition to the orbiting altimeter. Presumably, measurements of the gravity gradient together with height would lead, after transformation from a fixed inertial to a rotating terrestrial coordinate system, to a good estimate of surface gravity over the oceans. This, with Stokes' theorem, could yield a best fit of the geoid in continental regions that would, in turn, allow proper terrain corrections to be made in reducing gravity on dry land to the geoid. Then, again, Stokes' theorem might be reapplied to a closer approximation of the Earth's gravity field and a better definition of the Earth's figure. This sort of "geodetic bootstrapping" would not only employ detailed ocean surface gravity for the first time but reveal the extent to which isostatic compensation is realized in the crust, and its strength in the face of uncompensated loads.

In the several phases of this orbiting sensor concept, it is to be expected that as much additional information as necessary be incorporated into the interpretation and analysis of height measurements. For example, one would certainly wish to interpret the barometric load on the ocean with reference to synoptic weather maps, Tiros and Nimbus photographs of cloud forms and radiation, and, not least of all, the steric relief of the sea surface due to regional differences of sea-water density and steady-state currents. Similarly, where sea-level barometric pressure systems are clearly established, there should be accompanying geostrophic wind fields and well-developed sea states. It is through this background of related phenomena that the presence and progress of tsunamis might be established, and, again, if tsunamis are present, one would look for sudden changes in terrain owing to faulting or volcanism in the directions from which the tsunamis appear to originate. Given careful study of the orbital perturbations over tectonically active regions, the variations of gravity and relief might possibly serve to anticipate these cataclysmic events.

In the longer term, eustatic changes of sea level have an unknown origin. Sea level is known to rise and fall slowly with respect to the land, but which actually moves? Does the land rise or the sea fall? Simple analysis, assuming that the mass and crustal density of the Earth is conserved, suggests that the mean sea level maintains more nearly constant geopotential than the continental masses. But all this remains an open question.

The virtue of an orbiting altimeter (and gradiometer*) is that it could provide totally new kinds of information about the shape and size of the

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* Note added October 1965: An orbiting gravity gradiometer may be even more useful in application to the Moon than to the Earth, because of the lack of surface gravimetry, the much larger variations anticipated, and the ability to sustain a much lower orbital altitude.

The gradiometer will be quite complementary to the determinations of the gravitational field from orbital perturbations, which are highly smoothed and hence reflect mainly the harmonics of low degree. Since it will be a new device, however, it is highly desirable that the accuracy, drift characteristics, and mode of operation of the gradiometer be such as to make possible a comparison of gradiometer results with those made by orbital perturbation. The mode of operation required would be nearly continuous operation over at least one complete revolution. The accuracy plus drift per revolution would have to be about $\pm 10^{-11}$ gal/cm for such a comparison for an Earth satellite at 1000 km altitude, about $\pm 5 \times 10^{-10}$ gal/cm for a lunar satellite at 100 km altitude. The accuracy currently estimated as attainable for the system proposed by ARMA Division is about $\pm 3 \times 10^{-11}$ gal/cm.

The most active effort toward a satellite gradiometer appears to be that of the NASA/MSS manned orbiters. The argument made in favor of placing the gradio-

Earth, its changes of dimension with time, and the physical events in the air, sea, and solid Earth that are necessarily involved. Moreover, with this new tool at our disposal, and sufficient time to learn how to use it on this planet, a means may well be provided for the physical exploration of other planets as well.

COLLISIONLESS SHOCK-WAVE EXPERIMENT

The possible existence of a collisionless shock wave in interplanetary space was first suggested by T. Gold in 1955 to explain the sudden-commencement phase of a geomagnetic storm. Subsequently, many theoretical studies of the collisionless shock wave have been made, but the physical processes of its formation and many details of its structure are still not understood.

The first successful detection of a collisionless shock wave was made by the first Interplanetary Monitoring Probe, IMP-1, launched on November 27, 1963. The existence of the shock front is evidenced by data showing a transition from a uniform interplanetary magnetic field to the turbulent and rapidly fluctuating magnetic field characterizing the transition region between the shock front and the magnetosphere. Evidence for the presence of a collisionless shock wave propagating in the interplanetary space had also been obtained by C.P. Sonett from Mariner II measurements.

In addition to the magnetic field, measurements of the physical properties of the plasma in the transition region have been made (H.S. Bridge of MIT; I. Strong of Los Alamos; and J.H. Wolf of NASA's Ames Research Center). However, the results are far from complete, and some of them are conflicting. Little is known about the density, temperature, and velocity profiles in the shock front. Whether there are fluctuations in the physical properties of the plasma—in particular in the velocity in the transition region—is not certain at present.

With a properly instrumented manned orbiting laboratory, many interesting experiments can be carried out to measure the turbulent structure in the transition region. As the magnetosphere extends to about 10 Earth radii on the sunlit side of the Earth and as the transition region on this side is 3 or 4 Earth radii thick the orbit of the laboratory should be properly chosen.

Measurements should be carried out for physical quantities such as magnetic-field intensity and velocity and number density of the plasma

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meter in a manned satellite is that it requires frequent calibration and adjustments that are difficult to automate. It should be emphasized, however, that continuous measurement over an entire revolution is desirable, which may place severe restrictions on the astronaut's freedom of motion and other sources of "noise" in the gravity gradient generated within the satellite.

particles in order to obtain both their mean values and fluctuations. The determination of the energy spectrum of the plasma, especially for the electrons, will be highly desirable. A unique advantage of a manned orbiting laboratory is that more accurate determination of these quantities within the response time of the instrument is possible. The reading time and the sampling time for IMP-1 were 4.8 sec and 20.4 sec, respectively, and both are thought to be too high. In addition, the thickness of the shock front obtained by IMP-1 was of the order of 40 km or more. It would be interesting to measure the precise shock-front thickness and to obtain the profiles of the physical quantities throughout the shock front. Sonett has found that the classical Rankine-Hugoniot relations should be modified to have $\gamma = 1.2$ for the collisionless shock wave in interplanetary space. It will be extremely valuable to determine the shock relations for the collisionless shock wave in front of the magnetosphere.

Any experiment on the collisionless shock wave can probably be carried out only in space, since, thus far, all attempts to produce such a shock wave in the laboratory have failed. The collisionless shock wave in front of the magnetosphere is the only readily accessible one in space, and it is logical that every attempt should be made to explore it.

The purpose of this experiment is not only to verify previous results obtained by IMP-1 and by other space probes and satellites. Much-needed and important information about the turbulent nature of the magnetic field and plasma in the transition region behind the collisionless shock wave is to be expected. Useful clues would be obtained for the development of a theoretical model and analysis, and eventually for a better understanding of the phenomena of the collisionless shock wave.

DEEP INFRARED BACKGROUND RADIATION

It has been suggested that a measurement above the Earth's atmosphere of the spectral distribution of general background radiation in the deep infrared (10-100 μ) could provide information of fundamental physical importance. Much of this radiation would have had its origin in the starlight from galaxies when the Universe was young. Radiation originally in the visible and ultraviolet spectral regions from the remotest and hence youngest stars would be strongly shifted to the red by the expansion of the Universe. Because of this extreme red shift, the spectral distribution is strongly affected by the cosmological model, whether open, closed, or flat. It is even more strongly affected by the early revolutionary history of the galaxies. Peebles has estimated the various contributions to this general background radiation. The results are plotted in Figure 1.

The solid curves of Figure 1 give the integrated radiation from the background of remote galaxies for three possible models for the evolution of

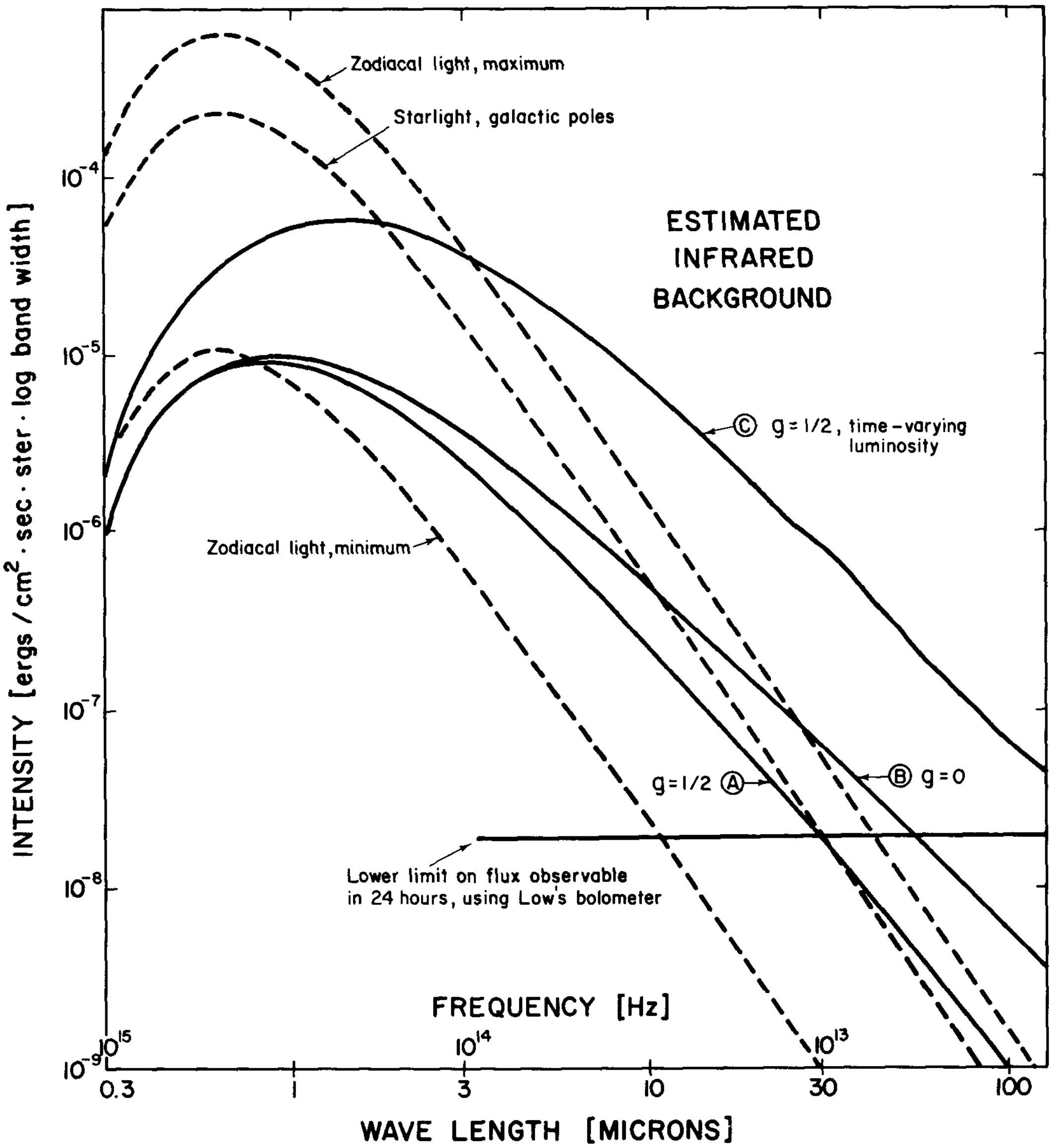

Figure 1. Estimated infrared background

the Universe; the middle dotted curve gives the estimated background from stars in our own Galaxy near the galactic poles (actually foreground when compared to the extragalactic background); and the upper and lower dotted curves give the possible range of the zodiacal light looking normal to the plane of the ecliptic. The vertical scale is the energy flux per unit interval in the logarithm of the bandwidth. For the cosmological models, the reciprocal Hubble constant has been set at $H^{-1} = 1 \times 10^{10}$y. In cosmological models A and B the luminosity of each galaxy is constant, and each galaxy radiates like a blackbody at 6000°K. In model A the acceleration parameter is $g = 1/2$, corresponding to a matter-filled Universe with a mean mass density of 2×10^{-29} g/cm^3. In model B the mass density is substantially less than 10^{-29} g/cm^3. In model C the spectrum of each galaxy remains characteristic of a blackbody at 6000°K, but the luminosity of each galaxy varies with time as the function $L(t) = L(t_f) \exp 4(1-t/t_f)$, where t_f is the present age of the Universe. The factor 4 in the exponent was chosen so that our Galaxy would have converted 30 per cent of its mass to helium with a present mass-luminosity ratio equal to 3 in the solar units. The curves imply that from observations of the spectrum of the infrared background one can hope to gain information on the evolution of galaxies, the effect of the choice of cosmological model being relatively minor.

The curve for the maximum intensity of the zodiacal light background normal to the ecliptic was extrapolated from the visible spectral region, using the solar spectrum. However, from the polarization there is reason to believe that the zodiacal light has been scattered from particles of micron size, so that in the infrared the background may approach that due to scattering from the solar wind only. The curve for the minimum intensity of the zodiacal light is that calculated for a direction toward the ecliptic poles resulting only from scattering by the solar wind with a density assumed to be 15 electron/cm^3 at the orbit of the Earth. The intensity of the starlight is in the direction of the galactic poles. The curve was based on a simple blackbody spectrum at 6000°K. The infrared flux may be larger than that shown if there are appreciable numbers of cooler stars. The horizontal line is the intensity just detectable (unit signal to noise) with the bolometer described by Low (1961), where the bolometer accepts radiation over a bandwidth $\delta\nu/\nu = 1$ and one steradian solid angle, the bolometer temperature is 1°K, and the observing time is 24 hours.

An investigation of infrared background radiation could be carried out either with an automated instrumented spacecraft or with assistance of man in space. The necessity for cooling the infrared radiation detectors with liquid helium would make the participation of a trained space technician useful.

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APPENDIX: LIST OF PARTICIPANTS

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PART THREE

VII
Rocket-Satellite Research

1. GENERAL SUMMARY

In response to a request from the Director of the Office of Science and Technology, Executive Office of the President, the National Academy of Sciences undertook an examination of the present state of research by rocket and satellite experiments. The Academy's Space Science Board created an ad hoc committee composed of active university research workers using rocket and satellite techniques to pursue their work, aided by scientists and technical representatives from the several federal agencies involved. Appendix 1 lists the participants in this study.

This report of the Committee consists of (i) an introduction distilled from the general and detailed discussion of the group and from information supplied from the federal agencies, (ii) recommendations concerning the national rocket research program, and (iii) scientific discussions of the present state of research using rockets, including mention of opportunities for further research with sounding rockets and satellites. In the scientific sections are treated the present state of knowledge, in a highly encapsulated form; examples of problems of high current interest that could be attacked with rocket and satellite experiments; discussions of relations between ground, rocket, and satellite experiments and observations; and some specific recommendations.

The list of problems of high current interest is not intended to be exhaustive or indicative of any kind of priority. It is, in effect, the type of list most often produced by studies of this kind. Therefore, the main purpose of such lists is to attest to the lively scientific interest in fostering a healthy rocket research program, in which a large number of scientists of reputation have a committed research interest. The contents of the lists merely serve to illustrate the breadth of problems that this portion of the scientific community would like to attack, problems that have strong and direct relations to the far larger space flight effort and that have as well very strong relations to the training of new scientists in the universities and federal laboratories.

Rocket research became a practical reality shortly after World War II. The number of scientific groups engaged in exploring the Earth's atmosphere and beyond by means of rocketborne instrumentation has grown from a few in 1946 to well over 80 today. More than 1,000 rockets with scientific payloads have been launched in the past two decades, in addition to the more than 4,000 smaller meteorological rockets, and a large number of important discoveries have been made. To mention just a few: the deep penetration into the atmosphere of solar x rays emitted from flares; the details of the solar extreme ultraviolet and the x-ray spectrum; the unexpected temperature variations in the upper atmosphere; the sampling of dust layers in the upper atmosphere; the discovery of x-ray stars. Furthermore, rocket research has contributed to our store of practical knowledge, with a promise of valuable applications among which are a better understanding of the upper atmosphere and solar-terrestrial effects as they relate to the lower atmosphere and the weather, of the ionosphere as it concerns long-distance radio propagation, and of the possible effects of modifying the upper atmosphere, and, additionally, the various national defense problems in which the upper atmosphere plays a major role.

This work has had its difficulties as well as its rewards. The latter, touched on above, stem from the ability of rockets to provide in situ measurements in a region of interest and to look beyond even the solar system from above the masking layers of the atmosphere. The difficulties stem from the fact that rocket research is a complex technology. Rockets do not provide an easy environment for scientific instruments. Separation of vehicle effects from the observations is often difficult. Technical problems, however, have not prevented progress. In fact, this period of exploration and instrument development has brought the rocket scientist to the point where he knows better what questions to ask and what measurements are needed to test hypotheses. Many intriguing and important scientific problems remain, and there are many technical problems of challenge to the engineering community as well.

Rocket research today is only a modest enterprise by the standards of space research as a whole, and small even in comparison with several other fields, such as high-energy physics. The national rocket program, which is supported by NASA (almost 50%), NSF (very modest), DOD (perhaps 15%), and AEC (the remaining portion), costs some $50 million per year. This figure includes only scientific research programs and some pro rata share of costs of operating launch facilities.

The national scientific satellite and deep-space program, excluding military operations and the manned space flight program, amounts to approximately $500 million per year. It is difficult to argue here the absolute balance or lack thereof between the rocket program (supported by several agencies) and NASA's over-all space research program (largely that of the Office of Space Science and Applications), although an implicit imbalance is noted in Recommendation 16 (p. 13). It is, however, in order to argue that if the nation can support a space research program of the magnitude of $500 million per year, it most certainly can afford, as an important part of it, a rocket research program of at least $50 million per year. The purpose of this report is to make that argument, and to recommend that the program be allowed growth. In making that argument, it is hoped that this document will also point out the inextricable relationships between a healthy research program using sounding rockets and a healthy spaceflight program—and its importance to the advancement of science generally, and of its applications.

THE COMMITTEE'S TASK

The Committee was asked to address itself to the examination of the relative difficulties and benefits associated with the employment of Earth satellites and of appropriately designed sounding rockets for various classes of near-Earth space research. This charge was taken to include considerations of sounding rocket technology and supporting systems for the future program.

At the outset, it is important to stress that comparison of rocket and satellite techniques—their relative advantages and disadvantages—is invariably a complex matter that is best done in terms of a specific scientific problem and the experiment proposed to elucidate it. Nevertheless, there are generalizations that can be made, and are made here, qualified as appropriate and illustrated with examples. As a kind of road map in the atmosphere, Figures 1-8 illustrate some of the vehicles and scientific questions relevant to a vertical cross section of the atmosphere and to astronomical observations from near space. The remainder of this section of the report contains not only general conclusions of the Committee but some specific recommendations pertinent to stated scientific problems.

Scientific problems were the main concern of this study. It was felt that justification of a research program in terms of practical benefits is probably superfluous as far as the scientific community is concerned. The Committee believes, however, that the nation as a whole deserves to be informed as to the applications that this research may have to human welfare. Moreover, a number of areas of research with promise of potential benefits can be readily mentioned in general terms; some of them are already beginning to be realized. For example, a major goal of meteorology is to forecast the weather, and in order to do this better it will eventually be necessary to incorporate data on upper-atmosphere conditions into the forecasting scheme. As a corollary to this, it will be necessary to find the links between changes in the Sun (and other extraterrestrial influences, such as lunar tides, cosmic rays, and meteor showers, that primarily affect the upper atmosphere) and changes in the lower atmosphere. There is good evidence that such links do exist, but we still cannot describe them owing to our lack of knowledge of the dynamical interactions between levels in the atmosphere. Another area of practical importance is the variable ionosphere and the part it plays in long-range radio communication, a subject of central interest in the rocket-sounding programs. Related to this are the effects of rockets on the upper atmosphere, the atmospheric effects of nuclear explosions, and the phenomena associated with the re-entry of space vehicles and rockets into the upper atmosphere. Still another category of benefits to be derived from the rocket program includes the major contributions it can make to the much larger space program, both in terms of contributions to knowledge and in the training of space scientists. These practical considerations will not be stressed here, but they (and other applications) will be implicit in much that follows.

Factors Affecting Altitude

The portion of the atmosphere between the highest practical balloon ceiling (about 47 km) and the lowest practical perigee of a satellite orbit (about

200-250 km), is directly accessible only to sounding rockets. Historically, ground-based observations have been used to study this portion of the atmosphere through indirect techniques such as radio-wave propagation and studies of meteor trails. Attempts have also been made in recent years to employ indirect methods from satellites above this region that look down on or through the atmosphere. A notable achievement of the satellite program, for example, is the radio-wave sounding of this portion of the atmosphere from above it. The topside sounder satellite has vastly increased our knowledge of the temporal and spatial characteristics of the upper portion of the ionosphere. Questions have been raised, however, by this kind of mapping that can be answered only by direct probing through the region to study in detail its vertical behavior.

Satellites can, of course, provide a kind of vertical sampling through much of the atmosphere above 200 km through the use of elliptical orbits. The limitation involved in this technique, however, is that it typically takes months to get a complete vertical section through a particular latitude and longitude. Consequently, the main contribution of satellites in elliptical orbits is to the study of the climatology of various attributes of the atmosphere, rather than to the understanding of specific altitude-sensitive physical processes. It also takes months to obtain complete coverage through the volume of the atmospheric shell between perigee and apogee, especially when day-night effects are desired. Seasonal effects can be obtained but, depending on the orbit configuration, may take longer than one progression of seasons to supply information on the whole volume of the atmosphere at all times of the day.

A small number of rocket flights, on the other hand, can give a sample of day-night changes, geographical variations, and seasonal comparisons, with the advantage that there is flexibility of launching times and places, within the limits of available rocket ranges. Figure 7 shows the worldwide distribution of rocket-launch facilities. U.S. scientists can usually make arrangements for observations, with some exceptions, at these ranges. Ship expeditions have been used, as a specific class of experiments warrants, to fill in gaps or to provide launchings at special oceanic locations. It is recommended that these expeditions be continued at the rate of about one every year or two.

In summary, rockets have a clear advantage in vertical soundings at times and places of interest, in order to perform experiments aimed at elucidation of physical processes. Satellites appear to have the advantage when used to map large volumes of the atmosphere above 200 km and to obtain climatological data. In addition, there are many kinds of experiments, discussed in later sections, for which the optimum experiment plan is to employ rocket soundings coordinated with satellite passes over launching ranges.

Timing

Satellite experiments, at best, have lead times of one to two years. The time is foreseen when many boosters will be made available to place into orbit small satellites on shorter time schedules, but the current program does not encourage such endeavors. The small classes of space flight vehicles Explorer satellites, for example, and the Interplanetary Monitoring Platform (IMP) have time schedules that are acceptably short for many experiments. Satellites in the observatory class, on the other hand, require very long lead times (3-5 years), which will grow longer in the foreseeable future, and they present basic difficulties with respect to continuity of research in universities and the participation of graduate students in that research. Furthermore, there is the scientific problem of designing and developing an instrument for use perhaps 3-5 years hence. In some instances, scientific advances have passed the satellite experiment by. Once an experiment is rendered obsolete, a difficult decision is faced. If the experiment is to be modified or replaced, further delays to the rest of the integrated payload will most likely occur as the new experiment is adapted into the payload configuration. The solving of the interface problems is a tedious and time-consuming effort for a complex satellite observatory—not that it is absent from even a single-experiment rocket payload—but the difficulties seem to increase at a greater rate than as a linear function of the number of components.

Rocket experiments are thus generally more suited to the carrying out of valid scientific experiments by small university groups, and for the doctoral research of a graduate student. This is true both because of the shorter lead times and the costs. (See discussion below on Training of New Scientists.)

A healthy rocket program also enables the nation's research scientists to respond quickly to new discoveries. For example, x-ray astronomy now has grown into a significant program from the first rocket discoveries only a few years ago. Ultimately, satellite x-ray detectors will map the entire sky, but rockets will continue to be needed, for example, to calibrate the satellite detectors by coordinated rocket flights at times of satellite passages. In the meantime, before the satellite program can absorb the new experiments being proposed and developed, a great deal of exploratory work can be accomplished at modest cost by rockets, work that will greatly increase the specificity and value of the coming satellite observations.

TECHNOLOGY

Recovery

While it is true that some satellite vehicles have been recovered, this very difficult activity is incorporated only in the NASA program for certain unmanned biological experiments. Rockets, on the other hand, are much more easily recovered within the limitations of the range and at some expense of available payload weight. There are some experiments, notably those using cosmic-ray emulsions, spectrographs that record on film, and atmospheric samplers, for which recovery is mandatory. Even though increasingly more experimenters are turning to telemetry, there are some other experiments for which recovery offers a significant advantage. If, for example, ultraviolet and x-ray photon counters can be recovered and recalibrated, the error limits of the instruments can be set with great certainty. In addition, the units can be reused, often with little repair. Recovery of instruments and their reuse have greater importance for more complex experiments, for example, optical systems with pointing controls. There are examples of optical systems being flown twelve times. Some of these systems are costly and require valuable manpower for their manufacture; therefore, a reliable recovery capability would be an important economic factor. It is estimated that recovery of systems from the Aerobee class of rocket costs some $25,000 per flight. Even allowing for refurbishing instruments, this cost could be more than regained if recovery systems could be made more reliable.

The techniques of air-snatch recovery have been demonstrated, and it may well turn out to be operationally feasible and economically worthwhile to develop this capability further at White Sands, Wallops Island, and a few other major launch facilities. We recommend that this possibility be investigated.

Instrument and Technique Development

Although rockets often offer a hostile environment to instrumentation (dynamic heating, wide range of pressures, high acceleration, plasma effects, and telemetry-signal rectification, for example) the rocket, and in some cases the large balloon, offers a convenient and relatively inexpensive means of developing, testing, calibrating, and perfecting many kinds of instruments to be used on complex costly space missions. There is influence in the reverse direction also. The superminiaturization required for spacecraft promises to improve balloon and rocket instruments to the point where payloads can be shrunk and lightened.

There are, however, great observational needs still to be met in the rocket region of the atmosphere: new techniques must be invented to measure ambient temperature above 60 km, ion composition in the region of short mean free paths, and winds in the daytime above 60 km.

Payload Integration

As rocket experiments become more sophisticated, particularly as it becomes increasingly desirable to measure a number of parameters during a single flight, the problems of rocket payload integration inevitably increase. The experimenter, nonetheless, has the advantage of being able to consider the whole flight package as his own.

This is but another argument to support the role of rocket research in the face of ever-increasing complexity of the satellite program; the complete control over his payload, and sometimes even of the vehicle, permits the rocket experimenter and his engineering colleagues to cope with problems on a personal basis and permits exposure of scientific and engineering problems to the interested graduate students.

Costs

The matter of cost is likely to enter into discussions of rockets versus satellites and is, in fact, a difficult matter on which to generalize. The meteorological rocket, which is now in use more or less routinely at some ten stations on a schedule of three to five launches a week, costs some $1,000-$1,500 each. A simple rocket experiment, such as the photographic observation of luminous vapor trails for the determination of wind, may cost only about $10,000 per launch (not considering amortization of the ground photographic network). A typical ionospheric experiment, with only one or two parameters being observed, falls into the $20,000-$40,000 price class. A galactic x-ray mapping experiment on an Aerobee rocket may cost over $150,000. An optical experiment using an Aerobee-Hi type of rocket and a biaxial pointing system will probably cost over $200,000. It is estimated that an advanced optical experiment, with satellite-type pointing control, on the newly developed Aerobee-350 type of vehicle, may approach $500,000 in cost. These estimates include cost of vehicle, payload costs, contractor services, and data reduction. They do not include a reliability factor. Taking into account successful flights only, and the total rocket budget over the past five years, the average cost for the NASA rocket program has been about $130,000 per successful flight.

The advocates of the idea that satellite experiments are more economical than rocket experiments point out that while a satellite experiment may cost more (the average is probably in the range of $1 million to $3 million—the exact amount is unimportant here), it provides from 2,000 to 5,000 times more observing time (it should be noted, on the other hand, that this refers to total observing time, not necessarily to useful observing time), and the cost per bit of satellite data is, therefore, far lower than for rocket data. But such comparisons are not usually valid. The primary criterion is the experiment: first, is it worthwhile performing? If the answer is yes, is it better to use a rocket or a satellite? Not many experiments can be performed equally well using either vehicle. Usually the dictates of the experiment (vertical versus horizontal variation; study of a specific event such as an aurora, radio absorption, the need to map large volumes of space, a solar cosmic-ray event) will clearly point to either the rocket or the satellite is the preferred mode of transportation of the instrument.

TRAINING OF NEW SCIENTISTS

Universities are the traditional source of scientific talent. Both satellite and rocket work are supported in the universities. In the earlier small satellite period, there were a few groups that included graduate students (State University of Iowa, University of Chicago, Johns Hopkins, MIT, University of Minnesota). These young scientists published papers on their work and in many cases accomplished their doctoral research via satellite experiments. They had the opportunity then of helping to design the experiment, manage its construction, receive their own data with project telemetry receivers—in short, the graduate student could participate in almost all phases of the experiment along with his senior colleagues.

This kind of intimate participation has always been and still is an essential part of research using small sounding rockets. The satellite and deep-space-probe work, however, has evolved so fast and so far toward systems complexity that even senior workers in the universities have lost the possibility of intimate contact with most phases of their experiments beyond the development of the prototype. The time is coming, the university workers hope, when small satellites will again be available to the university community, sometimes on a block payload allocation, so that they and their students can conceive, construct, and manage most phases of a group of experiments. Once again, then, will graduate students be able to participate fully enough, and on a short enough time scale, to make possible doctoral research with satellite experiments. Until then, and of course even after such a desirable state of satellite work is attained, rocket research offers an ideal mode of introducing to the graduate student the realities, problems, and challenges of geophysical research in situ in the upper atmosphere.

Both this committee and the Space Science Board's Committee on NASA-University Relations recommend that the support of rocket programs in the universities be expanded; and this committee furthermore endorses the concept of the NASA-University Explorer Satellite program with its short lead times and relative lack of complexity.

GENERAL CONCLUSIONS OF THE STUDY

1. Research in the upper atmosphere and near space using small sounding rockets is in a healthy state from the point of view of demonstrated results and potential values: there are a multitude of problems of widely conceded general interest that can be attacked only, or best, using rockets.

2. There is a large number of active scientific groups currently engaged in the use of rockets, some ten new groups are known to be planning rocket programs, and there are additional groups that have good promise of developing rocket activity in the next few years. There appears to be promise of a sound scientific demand for a rocket program that will approximately double or triple in the next five years, with a sufficient number of scientists and individual groups to use productively such numbers of rockets.

3. The rocket research program, however, does not stand alone. Efforts must continue to coordinate it with some satellite experiments and with ground-based observations. Rocket research is an integral part of geophysical research that will continue to pay high dividends; it should be exploited to its fullest, in terms of the unique advantages of the rocket, maintaining, as appropriate, a close association with ground-based and satellite programs.

4. University participation in the rocket research program should continue to be encouraged and supported. The NASA program should be allowed to grow.

The NSF has supported some research using rockets, but the contribution here has not been a significant portion of the program to date. It would be healthy for the over-all program if NSF could support some more rocket work in the universities, and it would be most appropriate and useful for the NSF to continue to fund supporting research that complements rocket flight experiments. NSF support of scientific ballooning is considerable, and this program also constitutes a significant contribution to the national space program.

In the past, the Air Force, through the Air Force Cambridge Research Laboratories, has supported a wide variety of rocket programs. Emphasis in AFCRL has shifted to internal programs, but does not involve university groups in independent research. It is considered by the study that it would be desirable if the AF Office of Aerospace Research could resume more support of independent rocket research programs by university scientists. The participation by university scientists and graduate students in programs of AFCRL, NRL (via the Hulburt Center), and NASA's Goddard Space Flight Center (via one-year fellowships) has been most useful and has resulted in the emergence of new rocket-oriented groups and in the training of space scientists. Such programs should be continued and strengthened.

5. The growth of rocket research requires capital re-investment in bringing some existing rocket systems (notably the Aerobee-150) up to date and increasing their reliability. Some enforced technological changes are known to be coming (e.g., change in telemetry from 200 to 1400 Mc). Funds required to meet such changes that are imposed by other groups or by considerations outside the realm of rocket research should be provided for outside the regular rocket budgets so that the scientific programs do not have to be cut back. New rocket developments (or concerted efforts to perfect systems consisting of already existing components) are needed in terms of increasing reliability, increasing by a small factor the peak altitudes attainable by small rockets, increasing control over vehicle flight attitude and of peak altitude, and improvement of recovery systems. In addition, the national rocket program needs expansion of launch facilities at major bases, to take care of increasing demand; provision for capability for night operation, to take care of the rapidly increasing astronomical programs; expansion of launch locations to the Alaska region; and provision for ship expeditions every other year or so, particularly through the next solar maximum, to provide for observations in critical geographic regions where it is not feasible or possible to construct permanent launch facilities.

RECOMMENDATIONS

The following list presents in full the recommendations made by the ad hoc Committee on Upper Atmosphere, Near Space, and Astronomical Research with Sounding Rockets and Probes, of the Space Science Summer Study, Woods Hole, Massachusetts, 1965. The discussions leading to the formulation of these recommendations are summarized in the body of this report.

Recommendation 1. Provision be made, both in direct budgetary support and in facility support, to accommodate an expansion of the nation's rocket research program by a factor between 2 and 3 over the coming 5-year period.

Recommendation 2. The nation's near-space satellite program for scientific research be maintained over the next 5 years, substantially at the level currently planned (including modest expansion).

Recommendation 3. The indicated expansion of the rocket program be undertaken, even if this can be done only at the cost of a stretch-out of the satellite program.

Recommendation 4. The government agencies that support research with rockets, specifically NASA, explicitly fund for technological developments in common-use rockets and supporting systems.

Recommendation 5. The scientific rocket research program be expanded preferentially in the area of university research support, from which the majority of the required new talent can be expected to evolve; and

The facilities of those national and government supported laboratories that are well equipped for conducting space research be made more available than at present for thesis work at the PhD level and at the postdoctoral level for training purposes, and that the supporting agencies of these laboratories become actively engaged in the funding of the training program.

Recommendation 6. The United States continue cooperation with other countries in scientific research with rockets, and work toward an expanded international rocket-research program during the next period of solar maximum.

Recommendation 7. (a) Special effort be made to support increased national rocket and satellite programs during the coming solar maximum 1967-1970; and

(b) The National Academy of Sciences take appropriate initiative within COSPAR to develop an imaginative international research effort emphasizing rocket research during this period.

Recommendation 8. (a) A suitable shipboard launching facility be made available for providing access to scientifically important but remote oceanic locations;

(b) The rudimentary rocket facilities near Fairbanks, Alaska, be enlarged to permit increased operations in this region of great geophysical interest, where a unique and well-developed network of geophysical observing stations already exists; and

(c) The launch facilities and range support at Ft. Churchill and at White Sands be improved and augmented as required to meet the growing needs of the scientific rocket program.

Recommendation 9. The Space Science Board Committee on Research with Rockets be reconstituted, with broad representation of all the disciplines which use sounding rockets; that it regularly review informally the scientific progress in each discipline; that it provide for the study of the rocket-research program and a mechanism for coordinating experiments; and that it make general or policy recommendations concerning the conduct of the national research program with rockets. The membership of this committee should include leaders in research with rockets.

Recommendation 10. The Aerobee-150 be improved by providing a modern booster of increased specific thrust, strengthening the tail section, and making other engineering changes to raise the present reliability.

Recommendation 11. Full support be given to the perfecting of various families of pointing or attitude-control systems as a high-priority item, particularly for astronomical observations.

Recommendation 12. A detailed study, including a cost analysis, should be made of all factors involved in air recovery in the belief that such an analysis will prove that air recovery can be suitably employed in many rocket experiments.

Moreover, it is recommended that a special effort be made to improve the reliability of parachute recovery techniques and to decrease the weight penalty of the existing system.

Recommendation 13. (a) The number of sounding rockets to be launched for probing the neutral atmosphere in the period 1966 to 1970 should be about 950, which is approximately double that launched in the period 1960-1965. Since these will in many cases carry more-sophisticated payloads than earlier rockets, the anticipated cost will be three times that of the 1960-1965 period. (See Tables 5 and 6 for a further breakdown of rocket requirements.)

(b) A total of about seven scientific satellites devoted to study of the neutral properties of the atmosphere should be launched between 1966 and 1970. This number is judged to be adequate for the requirements of this discipline and is in accord with current plans.

(c) In order to increase the scientific productivity of the total program it is recommended that coordination and comparison of experiments be arranged among the investigators.

Recommendation 14. The number of rockets provided in support of research concerned with the ionized components of the atmosphere, including airglow observations, should be increased from the present level of about 36 per year to over 100 per year in the next 5 years, reaching about 130 per year during the forthcoming period of solar maximum (about 1970).

2. SUMMARY OF DISCUSSION OF RESEARCH NEEDS, AND RESULTING RECOMMENDATIONS

LEVEL OF SUPPORT FOR ROCKET RESEARCH

We have reviewed, under four broad categories of scientific discipline,* the challenges and opportunities that can best be met by the exploitation of sounding rockets, either alone or in combination with other means of measurement, and we have reviewed the limitations that would be imposed by the availability of appropriate manpower. We have found in each scientific category a strong requirement for an expansion of the sounding-rocket program. The projected expansions vary from one category to another, both in their absolute numbers and in their precise relation to the present level of effort, in a manner discussed in detail in the individual discipline reports. It is possible to generalize to this extent, however: that in all categories, over the next 5 years, there should occur an expansion by a factor of 2 to 3 in terms of the number of vehicles to be launched, and by a factor nearer to 3 than 2 in terms of the funds to be allocated. The Committee therefore makes the following recommendation:

Recommendation 1. Provision be made, both in direct budgetary support and in facility support, to accommodate an expansion of the nation's rocket research program by a factor between 2 and 3 over the coming 5-year period.

LEVEL OF SUPPORT FOR SATELLITE RESEARCH

We have reviewed, under the same four categories* but in somewhat lesser detail, the corresponding factors that apply in the exploitation of satellites. Insofar as we can judge, the planned level of effort, which includes a modest expansion over the next few years, is adequate to the demands of each discipline; certainly we found no compelling arguments for either an increase or a decrease in that planned level, nor are we aware of any widespread advocacy of an increase or a decrease. Accordingly, the Committee advances the following recommendation:

* The four scientific discipline categories chosen for this study were: the neutral atmosphere, the ionized atmosphere, the magnetosphere, and astronomy.

Recommendation 2. The nation's near-space satellite program* for scientific research be maintained over the next 5 years, substantially at the level currently planned (including modest expansion).

SUPPORT OF ROCKET RESEARCH RELATIVE TO SATELLITE RESEARCH

It is implicit in the two foregoing recommendations that we find the present balance of effort between the sounding rocket and satellite programs to be incorrect, when projected into the next 5 years. In accordance with our recommendations, the imbalance should be redressed simply by an expansion of the rocket program, without detriment to the satellite program. But, in the event that some trade-off is obligatory, it is clear from the relative funding that the major advance called for in the rocket program could be achieved without impairing the prescribed satellite program. The following recommendation is therefore submitted by the Committee:

Recommendation 3. The indicated expansion of the rocket program should be undertaken, even if this can be done only at the cost of a stretch-out of the satellite program.

TECHNOLOGICAL IMPROVEMENTS IN ROCKETRY

Research with rockets, as well as with satellites, involves a substantial investment in the development of the vehicles and supporting systems. In the past, a considerable share of the vehicle development has been borne by the military agencies, and the research community uses many types of rockets that were originally designed for military applications. However, there is a growing demand for special rockets and supporting systems tailored to meet research needs, and the engineering development of these systems has frequently been paid for out of research money, in the absence of funds earmarked for this purpose. The Committee identified several new developments in rocket technology, some of them dictated by the research programs, such as the solar-pointed Aerobee, the fine-attitude-control system for stellar studies, the improvement of the Aerobee-150 rocket, and refinement of rocket payload recovery techniques; the Committee also identified some developments dictated by nonscientific factors, such as the

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* The four scientific discipline categories chosen for this study were: the neutral atmosphere, the ionized atmosphere, the magnetosphere, and astronomy.

enforced change to a higher telemetry frequency at all ranges. The cost of these common-use developments cannot, and should not, usually be borne by a single research group. The Committee thus makes the following recommendation:

Recommendation 4. The government agencies that support research with rockets, specifically NASA, should explicitly fund for technological developments in common-use rockets and supporting systems.

UNIVERSITY PARTICIPATION IN RESEARCH WITH ROCKETS AND SATELLITES

The Committee notes and approves the Report of the Space Science Board ad hoc Panel on NASA-University Relations (the Simpson Report of April 21, 1965) and the Report of the Woods Hole Working Group on NASA-University Relations (July 8, 1965). While the considerations in these reports are broader than those of this committee, we wish specifically to endorse Recommendation 6 of the second report cited above, which is hereby quoted verbatim:

> The Woods Hole Summer Study recommends that suitably space-oriented ground-based balloon and rocket programs be expanded as promptly as possible. The present rocket program should attain a level between two and three times that now in being as soon as possible.

The present Committee concluded, in addition:

The Committee endorses the concept of the short lead-time NASA University Explorer Satellite Program, which promises the possibility of useful participation by university graduate education programs in satellite research.

SCIENTIFIC TRAINING REQUIREMENTS

It is the opinion of the Committee that the planned expansion of the over-all space science program requires an intensified effort at the graduate and postgraduate level of training to provide an orderly growth and well-founded future for one of our most important national efforts. Accordingly, the Committee makes the following recommendation:

Recommendation 5. (a) The scientific rocket research program should be expanded preferentially in the area of university research support, from which the majority of the required new talent can be expected to evolve; and

(b) The facilities of those national and government supported laboratories that are well equipped for conducting space research should be made more available than at present for thesis work at the PhD level and at the postdoctoral level for training purposes, and that the supporting agencies of these laboratories become actively engaged in the funding of the training program.

PARTICIPATION IN INTERNATIONAL ROCKET PROGRAMS

This committee, while at Woods Hole, met jointly with the Space Science Board's Committee on International Programs to discuss U.S. participation in international rocket efforts. There are a number of reasons for believing that the United States should be active in international rocket programs, among them are:

(a) The scientific value of certain rocket experiments is greatly enhanced by synoptic observations at many points on the globe, and this can be achieved by coordinating our launchings with those of other countries or launching our rockets at ranges in other countries.

(b) Ideas for new experiments are generated by international scientific discussions, particularly in COSPAR.

In view of the considerations discussed above, the Committee makes the following recommendation:

Recommendation 6. The United States should continue cooperation with other countries, in scientific research with rockets and work toward an expanded international rocket-research program during the next period of solar maximum.

EMPHASIS ON SOLAR MAXIMUM

Solar maximum is a time when auroral phenomena, solar cosmic-ray events, geomagnetic storm activity, etc., are unusually intense and frequent. The coming solar maximum (about 1967-1970) will be the first opportunity to use advanced rocket and satellite technology to carry out many specific experiments and globally coordinated observations, as well as intensive patrol and exploratory observation in the interplanetary medium and magnetosphere. These observations are needed to advance the understanding of the solar terrestrial coupling and to test current hypotheses. There are many research groups in this country and abroad with active programs and interest in pursuing these problems. The Committee therefore puts forward the following recommendation:

Recommendation 7. (a) Special effort be made to support increased national rocket and satellite programs during the coming solar maximum 1967-1970, and

(b) The National Academy of Sciences take appropriate initiative within COSPAR to develop an imaginative international research effort emphasizing rocket research during this period.

IMPROVED AND NEW LAUNCH FACILITIES

The growth of the rocket program in the last two decades has, in part, resulted in the establishment of several excellent major launch facilities (White Sands, Ft. Churchill, Wallops Island) and has in part been further stimulated by the availability of these facilities. These locations will continue to be prime rocket-launch facilities, supplemented by a number of other locations, including ranges in other countries that U.S. experimenters can employ, as appropriate. Nevertheless, there are a few important areas not represented in the present network, areas crucial to certain types of experiments. Many of these locations can be reached by ocean-going vessels, and access to such areas for rocket launches has been provided on several occasions by ships. Examples of such scientifically interesting areas are the Davis Strait for launches near the geomagnetic pole, the South Atlantic to study the great magnetic anomaly, equatorial regions to study ionospheric current systems, and Southern Hemisphere ocean areas that are geomagnetically conjugate to established networks or launch sites in the Northern Hemisphere. There is also a class of experiments for which good geographic coverage or good spatial resolution requires some kind of mobile launch facility.

Furthermore, there is one particular U.S. land location at which a rocket launching site is needed for ionospheric and magnetospheric research. The Fairbanks, Alaska, area is uniquely suited for such observations, since it is surrounded by an existing network of geomagnetic, ionospheric, and auroral observing stations, and from it rockets can be flown into that part of the magnetosphere where, there is reason to believe, the magnetic lines of force are closed on the day side and open on the night side. This location is thus uniquely situated to complement the Ft. Churchill range, which is at a higher magnetic latitude.

The Committee therefore makes the following recommendation:

Recommendation 8. (a) A suitable shipboard launching facility should be made available for providing access to scientifically important but remote oceanic locations,

(b) The rudimentary rocket facilities near Fairbanks, Alaska, should be enlarged to permit increased operations in this region of great geophysical interest, where a unique and well-developed network of geophysical observing stations already exists, and

(c) The launch facilities and range support at Ft. Churchill and at White Sands should be improved and augmented as required to meet the growing needs of the scientific rocket program.

RECONSTITUTE A ROCKET RESEARCH COMMITTEE

In the early period of upper-atmosphere and astronomical research with rockets the scientists active in the program met regularly to discuss results and plan future experiments. They met under the name of Upper Atmosphere Rocket Research Panel (UARRP), but the UARRP never had official status.

After more than 10 years of activity, during which time it set up a Special Committee for the IGY (SCIGY), this group also became concerned with satellite research, and its name was changed to Rocket and Satellite Research Panel (RSRP). Since the creation of NASA and of the Space Science Board this group has been inactive. Some of its various functions have been performed by the Space Science Board, by an interagency coordinating group that communicates information on programs, and by the scientific and technical societies, such as the American Geophysical Union (AGU), the American Institute of Aeronautics and Astronautics (AIAA), the Union Radio Scientifique Internationale (URSI), etc. However, none of these has quite played the same role, and there has been no forum where detailed discussions of rocket research can take place, and no group that can provide strong scientific leadership and direction to the nation's rocket research effort. It is felt that this is a serious deficiency. The Committee thus makes the following recommendation:

Recommendation 9. The Space Science Board Committee on Research with Rockets should be reconstituted, with broad representation of all the disciplines that use sounding rockets; that it regularly review informally the scientific progress in each discipline; that it provide for the study of the rocket-research program and a mechanism for coordinating experiments; and that it make general or policy recommendations concerning the conduct of the national research program with rockets. The membership of this committee should include leaders in research with rockets.

3. INTRODUCTION TO AN ASSESSMENT OF ROCKET AND SATELLITE RESEARCH

BACKGROUND OF SOUNDING-ROCKET RESEARCH

While the historical base for space research certainly goes back to the ground-based observations that have given us a fund of knowledge of the upper atmosphere, the solar system, and the cosmos beyond, a major step toward space was taken in the United States in 1945 with the launching of the first successful sounding rocket. From this small beginning, the sounding-rocket program slowly developed, and during the IGY alone more than 200 U.S. rockets of various kinds carried scientific instruments into the upper atmosphere.

Other countries also undertook to fly rockets during the IGY, and in 1959 the International Committee on Space Research (COSPAR) was created to serve as a scientific forum to exchange results from rocket and satellite research and to plan international programs, the majority of which involved rockets. There are now at least 15 countries with rocket launching sites used for research purposes, and more than 30 rocket ranges are maintained around the world (see Figure 7). (In contrast, there are still only two countries that launch satellites; four others have built satellites to be launched by the United States.)

Periodically, the rocket program of the United States has been reviewed by the scientists involved. In October 1959, shortly after the National Academy of Sciences' Space Science Board was formed, an ad hoc Working Group on Upper Atmosphere Rocket Research met and prepared a report giving a sort of blueprint for the future in this field.* The Space Science Board's 1962 Summer Study at Iowa City included a number of discussions and recommendations concerning upper-atmosphere and astronomical research with rockets and satellites (with emphasis on satellites).**

We have focused attention primarily on sounding rockets rather than satellites. This is because, in making the comparison between the present and future programs of rocket research and satellite research, we noted what appeared to be an imbalance that deserved to be remedied—an imbalance that suggested that the rocket program deserved more attention by the Committee.

COMPARISONS BETWEEN ROCKETS AND SATELLITES

A country that can fly both rockets and satellites must decide on the relative emphasis to place on these two types of vehicles. In the body of this report, the many factors involved in this decision are treated in detail, but there are certain generalizations that can be made at the outset regarding the relative advantages of each kind of vehicle for space research. Here are some of the kinds of tasks for which sounding rockets are preferable to satellites.

(a) For aeronomy and ionospheric research:

Direct measurements of atmospheric conditions below the level of satellites and above the level of balloons, i.e., from about 30 to 200 km.

Measurements at a given time and place, or simultaneous measurements at a number of places (synoptic observations). The time can be selected to coincide with some unusual event, such as a solar eclipse, lunar occultation of celestial objects, solar flare, magnetic storm, polar blackout, etc.

Direct measurements, in any altitude range, of distributions in the vertical.

Measurements that require very large bandwidths and correspondingly large powers for a short time.

(b) For astronomical and cosmic-ray research:

Exploratory experiments, in which it is often advantageous scientifically and most economical to recover the instrumentation for recalibration and modification, and to fly it again as dictated by the results of earlier flights.

* Kellogg, W.W., "Rocket Research and the Upper Atmosphere," Chap. 7 in Science in Space, L.V. Berkner and H. Odishaw, eds. McGraw-Hill, New York (1961).

** A Review of Space Research, National Academy of Sciences—National Research Council Publ. 1079 (1962).

Experiments involving photography, which also require recovery. Very large quantities of precise data of certain kinds can be captured on film in a short time.

Observations of energetic particles with stacked emulsions, where recovery is required and where too long an exposure to the upper atmosphere environment will be detrimental.

(c) General applications:

Tests of prototype equipment to be used on space vehicles, where exposure to the launching and the space environment prior to investing in a full-scale satellite or space-probe flight may result in great savings and a better experiment.

Although satellites stay aloft for considerably greater periods than rockets, and thus provide relatively long periods for observing the atmosphere, their use does not simply multiply the observations made possible by a rocket. The selection of a satellite orbit determines whether the satellite will, with respect to atmospheric and magnetospheric research, be useful primarily for latitudinal variation studies, local-time-variation studies, global surveys, or the observation of temporal changes due to solar activity or other variations. Moreover, because of their great velocity, they do not readily permit study of the altitude variation of many parameters, an attribute of satellite vehicles that is frequently not recognized. Thus, while rockets make possible the observation of a particular and important aspect of the atmosphere, namely the vertical distribution of parameters, satellites permit different and complementary studies. For this reason, in addition to the several others discussed above, satellites can supplement the unique and singular role in atmospheric research that is played by rockets. There is, of course, a special class of satellite experiments— meteorological— in which the satellite offers a worldwide view of the lower atmosphere. Here, the satellite has no competitor.

For astronomical research, satellites are required for all work of a monitoring nature that depends on observations above the atmosphere. This is particularly true for solar studies. Satellites are also required for placing large astronomical telescopes above the atmosphere, for studies whose goal is unlimited spectral coverage, maximum resolution of detail, or the detection of the faintest objects—all of which are fairly long-time observations.

Rockets are not without their inherent problems, of course. They pass through regions rapidly, violently disturbing the medium in the lower portion of their useful trajectory. At rocket apogee, almost a minute of observations is obtainable, but for some phenomena that occur in altitude ranges as narrow as 5 km, control of rocket apogee is a delicate matter, and attainable in practice at present only with the liquid-fueled Aerobee. For some kinds of observations, a controlled rocket motion or attitude is required in order to obtain the desired scanning geometry or in order to point the detectors in the direction of interest. Rocket technology has not yet reached the point where such control is fully reliable and accurate enough to be depended on by the experimenter. It is recommended that such techniques be perfected.

These comparative advantages and limitations of rockets and satellites are generally well recognized. The fundamental point is that the scientific aim of a given experiment must dictate the choice of vehicle and its flight path or orbit. Some of the generalizations that have been made in the past must be questioned, such as the frequent claim that satellites produce "more bits per buck." "What value the bits?" one must ask. It has turned out that even on an absolute scale, where the cost of a total rocket experiment is compared with the cost of a satellite experiment (neglecting vehicle costs), it is not evident that one will cost more than the other, although satellite experiments do tend to be more expensive on the average. There are also other factors, such as the lead times involved, and these will be discussed below. The main guidance when the choice of vehicle is being made must, however, come from the requirements of the experiment.

SPACE SCIENCE IN THE UNIVERSITIES

The Committee felt that a discussion of future rocket and satellite programs would be far from complete without considering the people involved: the number of interested groups, the training of new talent, the way in which the programs would be managed as in-house efforts or independently by non-government groups, etc. It became clear that a central issue is the way in which university research is supported, since to a large extent the nation will have to depend on the universities for the training of the next generations of space scientists and engineers. (We are aware of the conclusions of a separate Space Science Board Committee on NASA-University Relations, which stressed this same point.) In this connection, the experience of several university groups that have worked with both rockets and satellites was tapped extensively.

A factor that is well recognized by the university scientists involved in space research, but often overlooked by the program planners, is the impact that the choice of vehicle has on a graduate student. An experiment on a large satellite, such as the NASA observatories, or a space probe, usually means several years' work before any data are obtained. Only two or three U.S. universities have the control of their own satellites and the freedom from "interface problems" that permit them short lead times approaching those for rocket experiments—often less than a year. The result is that few graduate students can afford to delay their theses long enough to see a satellite experiment through to completion. Rocket experiments are preferable for them, and there are other reasons besides the time involved. Rockets are cheaper and require less negotiation and paperwork with the sponsoring agency; therefore, a student can assume more responsibility for an experiment from start to finish, recognizing the chance for errors. Thus, there is a better opportunity for a continuing space-research program at a university when graduate education can be supported with rockets.

This is why a university group starting a program of space research is more likely to be willing to become involved in ground-based or rocket experiments than in satellites. The new groups begun each year have created increased demand for rockets on the part of the university community, as will be documented in this report.

Thus, an important general conclusion developed in this report is that, aside from the fact that rockets have certain attributes that make them unique for many research purposes, they will apparently continue, as in the past, to be an essential ingredient in the training of new generations of space scientists. This factor must be included in any consideration of the relative emphasis to be placed on upper atmosphere and space research with rockets and satellites.

SCIENCE IN SPACE WITH ROCKETS AND SATELLITES

Scientific research using rockets and satellites has become very broad, and in order to assess it intelligently it is necessary to divide it into several categories. The Committee has chosen to discuss separately the neutral atmosphere, by which we mean motions, temperatures, densities, composition, etc., with emphasis on the atmosphere below 200 km; the ionized and chemically active atmosphere, including studies of radio propagation, electron-density distributions, photochemical reactions involving dissociation of molecules, the airglow and aurora, etc.; the magnetosphere, in which energetic particles and magnetic fields occupy the center of the stage; meteoric dust and upper-atmosphere aerosols; and astronomy. Actually, the astronomy discipline is a very large one indeed, and has been further subdivided in terms of solar astronomy, stellar and galactic optical astronomy, planetary astronomy, x-ray and ultraviolet astronomy, and radio astronomy. A final category is that of fundamental physics, in which there are a few interesting experiments that can be done best with rockets. Examples of such experiments are plasma studies with no walls, measurements of photoionization cross sections, and coefficient of drag measurements in hypersonic flight. (The Committee did not discuss fundamental physics in any detail.)

The Committee's approach to the very complex assessment of scientific research with rockets and satellites was to form a number of working groups to consider, within each of the disciplines mentioned above, the level of effort that could be justified on scientific grounds, based on a reasonable extension of the present effort. (The Working Group reports are contained in Section 6.) Consideration was then given to the growth of interest in a given discipline, in terms of the number of new research groups that have become involved in the pertinent research area in the past few years. In every case, there has been some increase in the number of interested groups, and the assumption was made that these groups would have to be supported by a certain number of rocket or satellite vehicles, the choice of vehicle depending on the requirements of the experiment.

It is interesting that in some cases it is predicted that more exploratory experiments will be made in the future than are being made now, as a result of new problems that are being opened up; this is typical of almost all branches of astronomical space research. In other areas, emphasis must be placed on obtaining more information under a variety of conditions, or on obtaining multiple observations by a number of different techniques in order to get an answer to the interactions in the atmosphere. Two good examples of this last requirement concern the effects that motions in the E-region have on the formation of sporadic-E ionization, due to the combined effects of electric and magnetic fields generated by the winds there, and the complex phenomena associated with ionospheric current systems, in which magnetic fields, free electrons, and ions, and motions in the ambient atmosphere all interact to produce the phenomenon. Meteorological rockets fall definitely into the category of methods for documenting changes in the stratosphere on a regular basis.

ADMINISTRATION OF THE PROGRAM

Another factor considered by the Committee was the way in which space research with rockets and satellites is supported by the various government agencies. We were particularly concerned with how the university community was involved, for the reasons given in the discussion of space science in the universities, above.

The Committee noted that less than half of the U.S. rocket-research effort in the past 5 years has been supported by NASA, whereas NASA supports the major portion of the scientific satellite program. The National Science Foundation (NSF) has funded some rocket research, largely through the Hulburt Center of the Naval Research Laboratory (NRL).* The Atomic Energy Commission (AEC) and the Department of Defense (DOD) have supported more than half of the research rocket flights. Recently, the Central Radio Propagation Laboratory (CRPL) of the Department of Commerce has embarked on a limited program of ionospheric research with rockets, complementing its long-standing activity with ionospheric satellites.

The reasons these various federal agencies give for their research programs range from the NSF's dedication to basic research to the AEC's and DOD's dedication to their missions of insuring the national security. While NASA has a mission—the exploration of space—its policy places it much closer in the above framework to the NSF than to the AEC and DOD.

Perhaps the chief distinction between the way the clearly mission-oriented agencies (DOD, AEC, and, to a lesser degree, the CRPL) and the way NASA and NSF (including the Hulburt Center of NRL) conduct their programs lies in the role the universities play. The policy of NASA and NSF is to allow both in-house and university groups considerable freedom in the conduct of the research, the choice of problems, the time scale, etc., and explicitly to encourage the training of students. The other agencies, however, have an important job to do, and any outside group, be it university or industry, is generally expected to contribute to the specific problem at hand. The technical and scientific control remains in the hands of the in-house scientists. Within this framework, there may or may not be room for graduate education, since the support of education is not the prime purpose of the sponsoring agency. The Committee took this distinction between the administrative policies of the various federal agencies into account in formulating its recommendations for the future.

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* The Hulburt Center receives its major support for rocket and satellite research from the Office of Naval Research (ONR), which also supports the Naval Research Laboratory.

SCHEMATIC SUMMARY OF UPPER-ATMOSPHERE AND SPACE RESEARCH

In the material that follows, particularly in Section 6, in which the scientific disciplines are discussed, there are many references to atmosphere terms, upper atmosphere and space phenomena, and past and future experiments. The subject is indeed complex, and Figures 1-6 may serve as a guide for threading one's way through it.

4. ROCKET AND SATELLITE VEHICLES AND AUXILIARY SYSTEMS

ROCKETS

Some of the rockets currently used for research have been developed especially for scientific work. The most notable example is the liquid-fueled Aerobee, developed in the late 1940's and still in wide use today. A few small solid-fuel rockets have been developed in recent years specifically for research purposes—for example, the Arcas, which has proven to be a successful vehicle for the meteorological rocket network. Several new and somewhat larger rockets, such as the Black Brant III, Tomahawk, and Hydac, have also been produced recently, largely in response to needs for modest-sized vehicles to carry payloads to altitudes in the 100-150 km range.

Many rockets in use today employ readily available boosters that had been developed for other purposes along with appropriate second stages to accomplish the payload-altitude specifications of experimenters. One of the most successful and widely used of such combinations is the Nike-Cajun. The Nike family has grown to include the Nike-Apache, with somewhat greater peak altitude, and the Nike-Tomahawk and Nike-Iroquois, with still greater payload and altitude capabilities. (See Table 2, giving characteristics of some currently used rockets.)

The Nike booster has thus been instrumental in the growth of the medium size-rocket program, but has now been discontinued for military production. This has caused some concern among users of the Nike-class rockets. However, there is still a large stockpile of these boosters, and, additionally, the Army's Radford Arsenal, in Virginia, which makes them, has agreed to continue producing Nikes at the current price on a "surplus" basis (about $5,000). Nevertheless, this situation has raised an important issue concerning the choice between continuing to use a well-tried booster or rocket vehicle and developing an improved one.

Once a system like that of the Nike-boosted family has been taken through the process of aerodynamic design and correction of a multitude of problems (such as instability, resonances, excessive coning or other nose-cone motions, vibration, and dispersion) and proves useful to the scientific community, there is great reluctance by that community to experiment further, except when it is absolutely necessary to correct some fault. The long-term success and stability of the rocket program requires tried-and-true readily available vehicles, which may have some known idiosyncrasies that

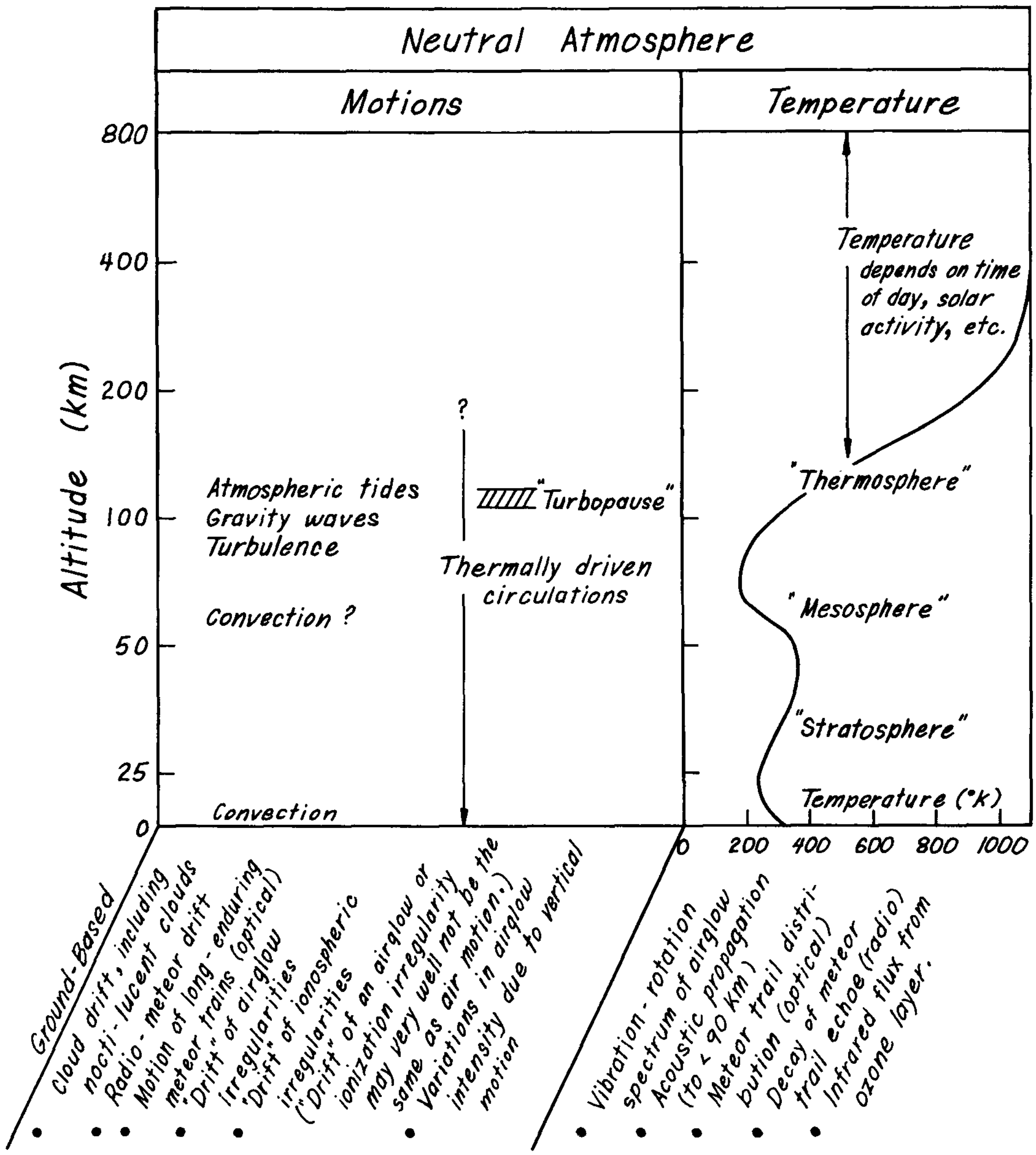

Figure 1. Neutral atmosphere.

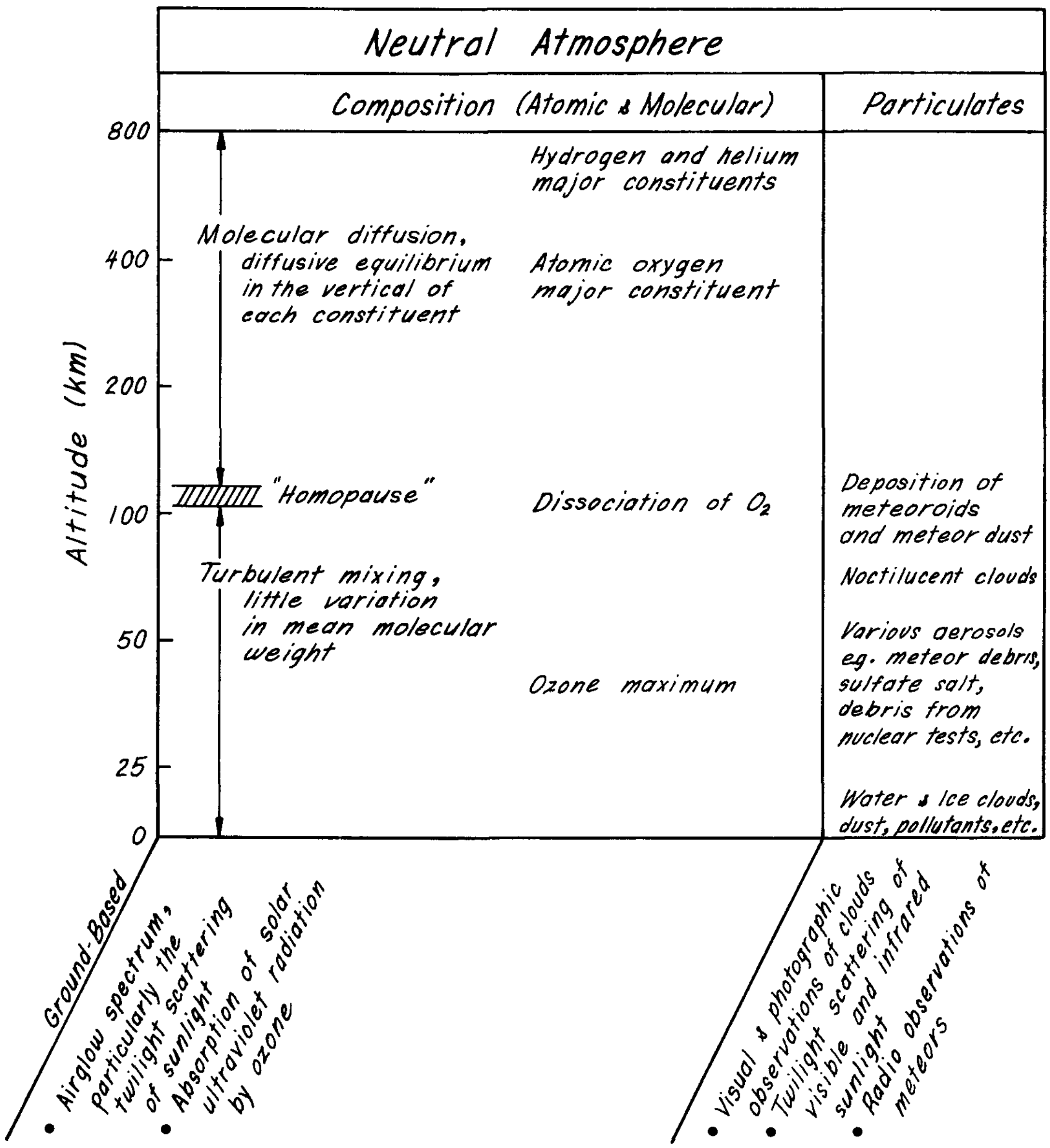

Figure 2. Neutral atmosphere.

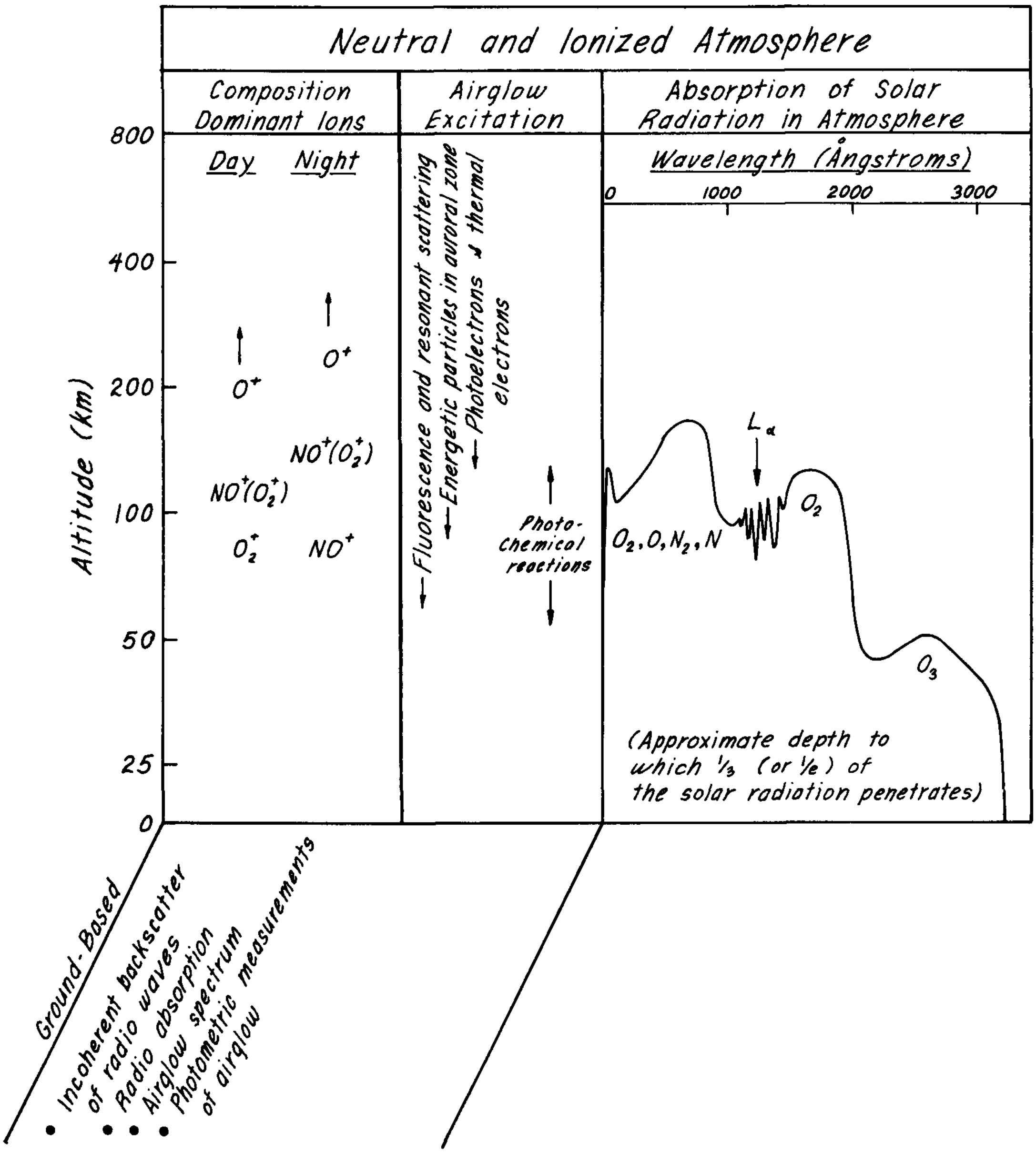

Figure 3. Neutral and ionized atmosphere.

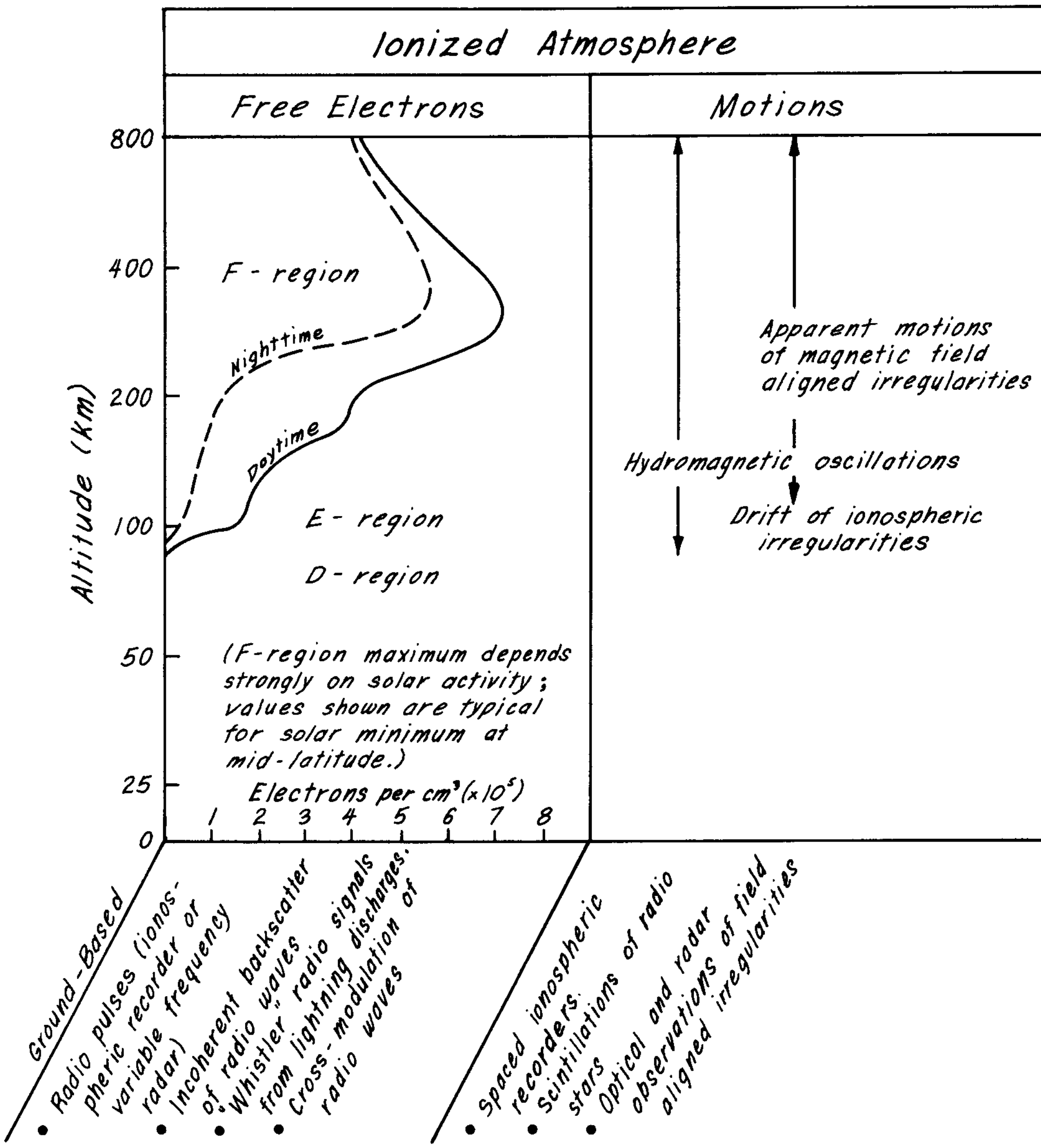

Figure 4. Ionized atmosphere.

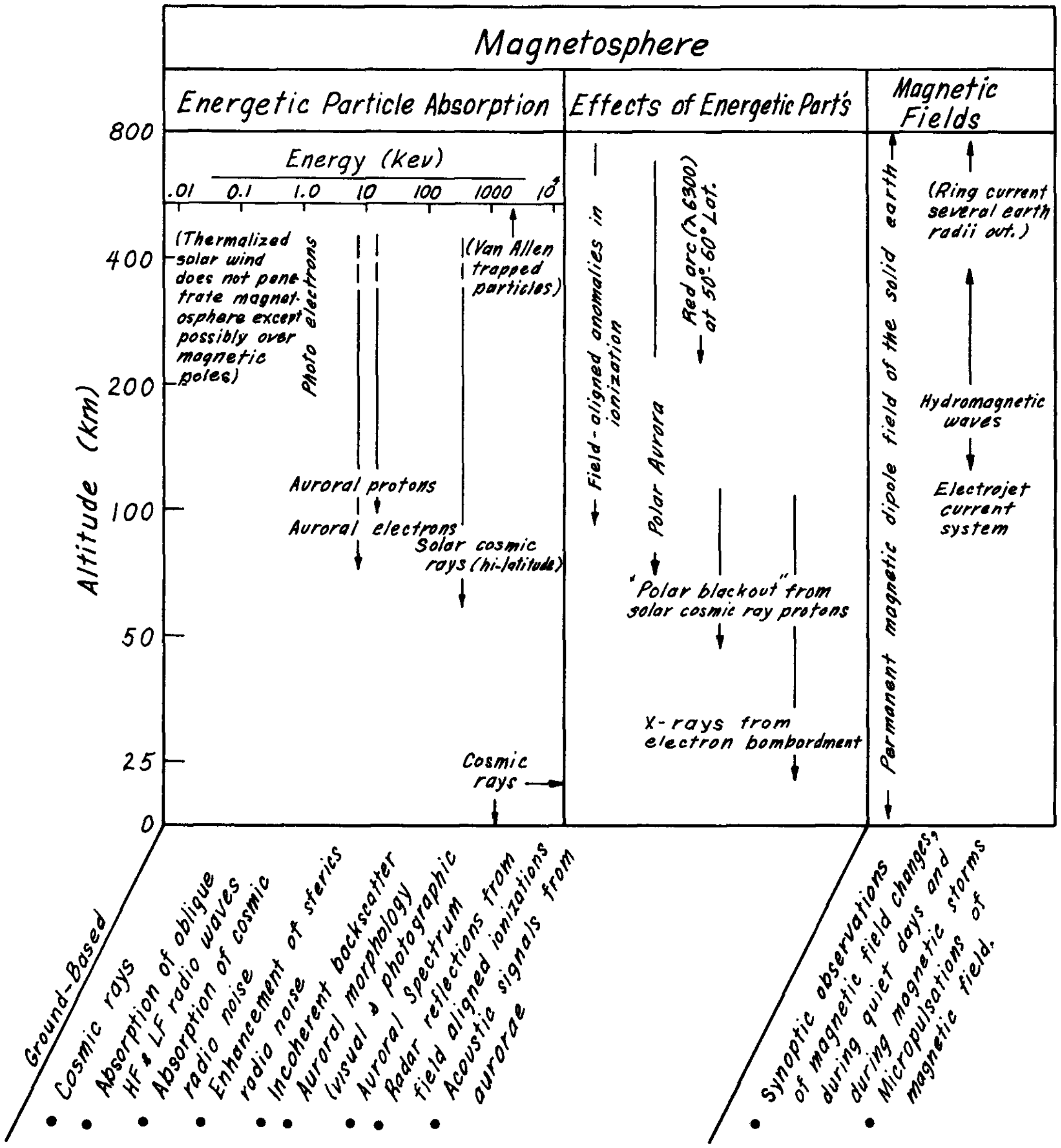

Figure 5. Magnetosphere.

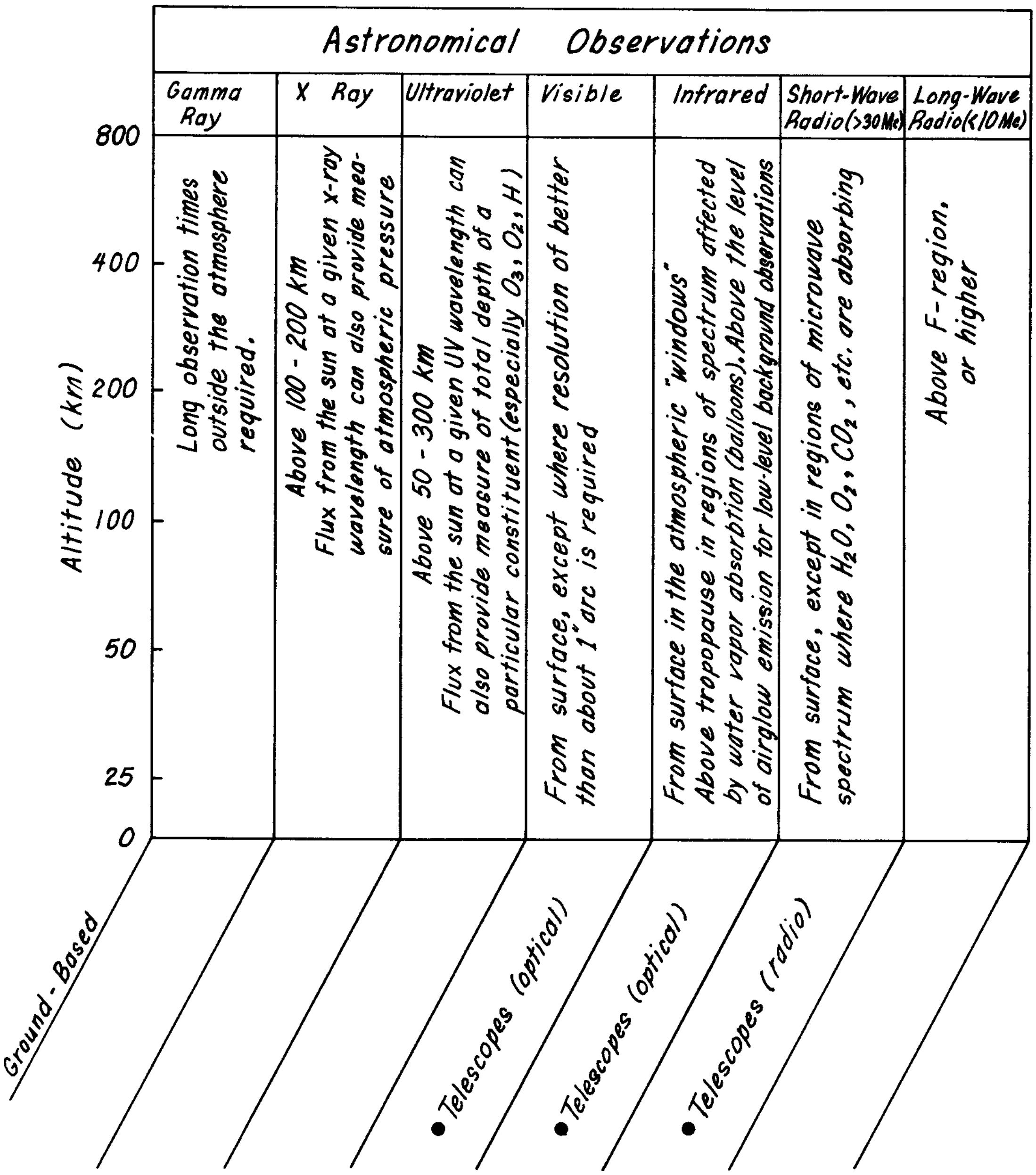

Figure 6. Astronomical observations.

can be corrected on an empirical basis. Any new rocket, or even a modification of a proven system, has a host of problems that are sometimes due to very subtle and difficult-to-uncover causes. Consequently—as in the case of the Nike, for example—there arises the difficult decision: is it better to subsidize the production of obsolete Nike boosters after their prime purpose has been fulfilled, or is it better to develop a new booster, using newer and more potent fuels, and face the necessity of a long check-out period before the reliability of the new vehicle is established? If the latter course is considered, it should be borne in mind that development of new rockets will also incur additional expense at all ranges in adapting launchers and ground-support and handling equipment, as well as redesign of fins, interstage adapters, etc. (unless the new propellant can be adapted to the old rocket case without changing the external configuration). The Committee saw no overwhelming reason for investing in a new Nike-type booster development at this time.

A similar decision is needed for the Aerobee-150 rocket, a liquid-fuel rocket using a booster that is now obsolete and has to be manufactured on a special basis. There are solid-fuel rockets that match the capability of the Aerobee itself at somewhat lower cost, but which cannot at this time be launched at White Sands Missile Range because of range restrictions and range safety requirements. The scientist users, especially astronomers, prefer the Aerobee and are convinced that they will have heavy need and justification for the Aerobee for the next decade. This is largely because the Aerobee has a low-G takeoff, gentle ride, low vibration, well-known characteristics, and reasonably well-developed auxiliary equipment such as recovery packages and pointing systems. The astronomers argue that the Aerobee is so well suited to their needs (but not completely) that they would prefer to invest in increasing its reliability and its capability by a modest amount, perfecting the auxiliary systems, and providing for a new booster, rather than change to a different rocket system.

With respect to this question, NASA has estimated the cost of improving the Aerobee-150 to be between $300,000 and $500,000, which would involve engineering improvements to raise its reliability above the present 90%, improving stability by redesign of the fins, and developing a modern booster and a corresponding strengthening of the tail section. The Committee agreed with the astronomical users that this improvement would be most worthwhile, since the costs of this system and its improvement represent a small investment relative to the costs of the scientific experiments that will be flown by the Aerobee-150 in the next decade, there being no replacement in sight. The Committee therefore makes the following specific recommendation:

Recommendation 10. The Aerobee-150 should be improved by providing a modern booster of increased specific thrust, strengthening the tail section, and making other engineering changes to raise the present reliability.

The Committee noted that NASA has developed an Aerobee-350 with much higher performance than the regular Aerobee-150. It is being launched on a test basis from Wallops Island. If recovery of payloads from this vehicle is required, it should be launched from White Sands, where recovery with conventional parachute methods can be achieved over land, or an air-snatch recovery system should be implemented at Wallops Island (see the discussion of auxiliary systems, below, concerning recovery system development). The cost of installing an Aerobee-350 launcher at White Sands would be about $2 million.

AUXILIARY SYSTEMS

The discussion of problems and needs for auxiliary rocket technology is contained in the scientific discussions, particularly in the section on astronomy. The two main areas in which improvements would provide large scientific payoffs are those of pointing or attitude control and of payload recovery. Another area that deserves attention is that of ground support and tracking at the launch sites.

Several pointing controls ranging from the Inertial Attitude Control System (IACS, with about 4° accuracy) to more elaborate systems based on IACS (but with accuracy to minutes of arc) have been developed and are in use, with various levels of satisfaction. A stellar pointing system has been developed by NASA with accuracies of about 30 sec of arc and capable of selecting and pointing at five stellar objects. An analogous solar-pointing control is planned for the fiscal year 1966 and fiscal year 1967 NASA budgets, at a total cost of $1 million. Other agencies (Air Force, Sandia Corporation, and Kitt Peak National Observatory) have also been involved in pointing-control development. A wide variety of demands is current (from about 1 min of arc in a nonmagnetic system for atmospheric-electric-current measurements to 5 or 10 sec of arc for long-focal-length optical systems for solar, stellar, and planetary work). The Committee therefore puts forth the following recommendation:

Recommendation 11. Full support should be given to the perfecting of various families of pointing or attitude-control systems as a high-priority item, particularly for astronomical observations.

Certain rocket experiments require recovery to obtain data taken on film (spectrographs, etc.), and dust, aerosol, and gas samples. There have been great improvements in automating many detectors and telemetering data, but there appears to be a continuing requirement for recovery of data for some experiments, particularly astronomical. In addition, recovery of detectors permits recalibration, with attendant increase in confidence in measurements, and recovery of expensive, complex instruments and of auxiliary technological equipment such as pointing controls. This last aspect must be analyzed not only with regard to cost of recovery versus saving in recoverable equipment, but in terms of the precious, highly skilled technical manpower needed to build new instruments. Normally, recovery involves parachute descent of the payload, thus limiting operations essentially to White Sands, where difficulties nevertheless do arise (payloads are sometimes dropped in the mountains or dragged through desert sands). The air-recovery system has been demonstrated in other programs and has proven successful, albeit costly and complex. Nonetheless, the Committee feels that the technique may prove valuable to rocket research, especially for costly experiments. The following recommendation is therefore submitted:

Recommendation 12. A detailed study, including a cost analysis, should be made of all factors involved in air recovery in the belief that such an analysis will prove that air recovery can be suitably employed in many rocket experiments.

Moreover, it is recommended that a special effort be made to improve the reliability of parachute recovery techniques and to decrease the weight penalty of the existing system.

Tracking facilities at certain frequently used ranges are now inadequate. Good tracking of the vehicle is required for some experiments, and also for precise cutoff of the Aerobee rockets to achieve the desired apogee. The falling-sphere experiment to measure densities (and winds) by the drag on a sphere requires precise knowledge of the sphere's trajectory. The excellent FPS-16 radar is available at most launch sites, but not at Ft. Churchill (otherwise, a most active site) nor at many sites operated by other countries. This inhibits any synoptic observations of densities in the 60- to 120-km region by this valuable technique. Some other ranging systems that do not require an expensive radar are being developed by Sandia and Goddard, and completion of their development will cost $100,000 to $200,000.

An added advantage of a good tracking facility is the means it affords to direct the telemetry receiver antenna in the right direction. This can be done by slaving the antenna to a tracking radar. As the telemetry systems go to higher frequencies (which they will be forced to do) and the receiving antennas become more directional, this capability becomes increasingly useful.

LAUNCHING SITES ABROAD AND AT SEA

As described in the Introduction to Section 3, rocket experimentation is conducted actively in more than 15 other countries, and at least 30 rocket ranges are maintained around the world. Figure 7, prepared by M. Dubin, shows the locations of most of these foreign launch sites. As shown in Table 1, there are also about 20 stations in the meteorological rocket network making regular or intermittent flights. (See, also, Appendix 2, "Sounding-Rocket Launching Facilities" by M. Dubin, which gives further information on the international rocket program.)

The United States has already entered into cooperative rocket programs with other countries. In such cooperative endeavors, the United States may provide some supporting services or equipment, but the host country funds the operation in its country. NASA is the prime U.S. agency for these programs abroad, but the DOD has also undertaken some.

The Committee is highly in favor of this type of undertaking, as it extends the number of ranges available to U.S. experimenters, permits rocket launches in regions of interest to which we would not otherwise have access (conjugate points, tropical and arctic locations, etc.), and encourages contacts with scientists in other countries. (See Recommendation 6, Section 2.)

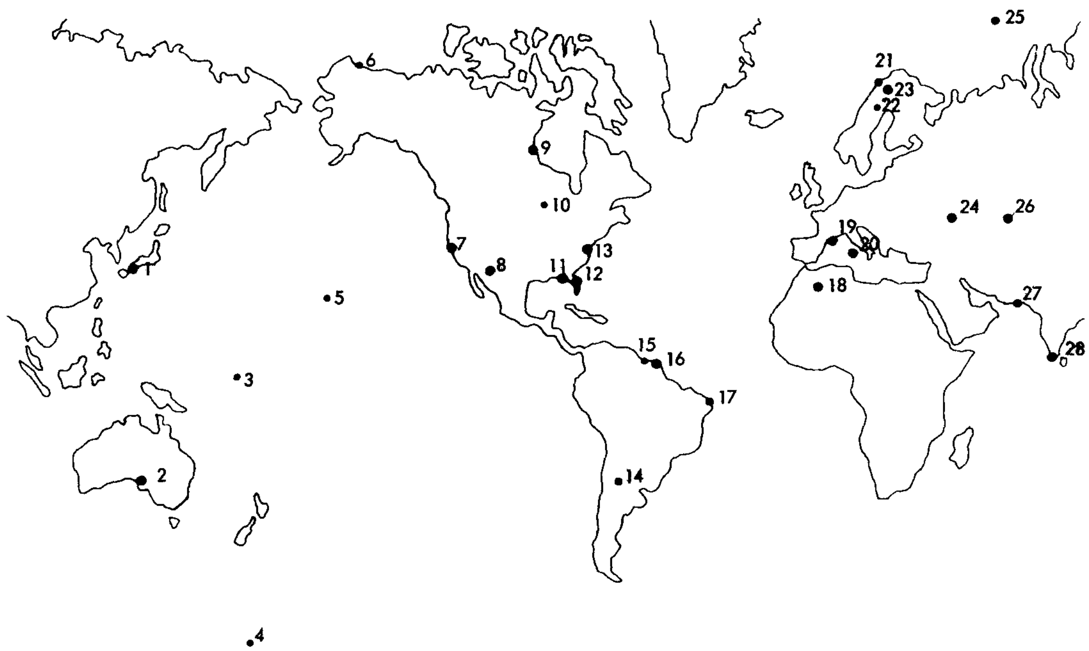

Figure 7. Sounding rocket launching sites.
1, Uchinoura; 2, Woomera; 3, Kwajalein; 4, McMurdo Sound; 5, Kauai; 6, Point Barrow; 7, WTR, Point Mugu; 8, White Sands; 9, Fort Churchill; 10, Keweenaw; 11, Eglin; 12, ETR, Cape Kennedy; 13, Wallops Island; 14, Chamical; 15, Surinam; 16, French Guiana; 17, Natal; 18, Hammaguir; 19, Ile du Levant; 20, Sardinia; 21, Andöya; 22, Krönogard; 23, Kiruna; 24, Kapustin Yar; 25, Franz Joseph Island; 26, Tyuratam; 27, Sonmiani; 28, Thumba. (Prepared for COSPAR by M. Dubin, NASA, May 1965.)

For some purposes, these land-based sites are not enough, however. For special events, such as solar eclipses and occultations of other astronomical sources by the Moon, and for observations at points that are magnetic conjugates to U.S. launch sites, a ship-launcher is essential. Shipboard launchings have already been carried out by the Navy and by NASA, and the Committee recommends that provision be made for such a ship expedition every year or two. (See Recommendation 8, Section 2.)

Table 1. Meteorological Rocket Launching Sites
Used by the United States*

Name	Latitude	Longitude
Ascension Island AFB, B.W.I.	07° 59' S	14° 28' W
Antigua AAFB, B.W.I.	17° 07' N	61° 47' W
Kindley, Bermuda	32° 22' N	64° 40' W
Grand Turk Island	21° 27' N	71° 09' W
San Salvador Island	24° 04' N	74° 31' W
Wallops Island, Virginia	37° 50' N	75° 29' W
Cape Kennedy, Florida	28° 14' N	80° 36' W
Eglin, Florida	30° 23' N	86° 42' W
Keweenaw, Michigan	47° 26' N	87° 42' W
Fort Churchill, Canada	58° 47' N	94° 17' W
Holloman AFB, New Mexico	32° 51' N	106° 06' W
White Sands Missile Range, New Mexico	32° 23' N	106° 29' W
Tonopah Range, Nevada	38° 00' N	116° 30' W
Point Mugu, California	34° 07' N	119° 07' W
Fort Greely, Alaska	64° 99' N	145° 44' W
Point Barrow, Alaska	71° 18' N	156° 47' W
Barking Sands (Kauai), Hawaii	21° 54' N	159° 35' W
Eniwetok, Marshall Islands	11° 26' N	162° 20' E
McMurdo Sound, Antarctica	77° 51' S	166° 39' E
Kwajalein, Marshall Islands	08° 44' N	167° 44' E

* About nine of these are used regularly; flights are made intermittently at the others.

SUMMARY OF INFORMATION ON ROCKET AND SATELLITE VEHICLES

This section reviews briefly the range of vehicles available to U.S. upper-atmosphere and space experimenters, and gives some cost information. More-detailed information was available to the Committee, but this will provide the facts needed to understand the main issues that are involved in vehicle selection.

With respect to costs, the base cost for a rocket is quite misleading as an indication of the cost of a successful rocket experiment. Taking NASA's figures on annual rocket firings, success ratios, and annual budget (not including fixed costs such as that for the Goddard personnel involved in engineering tests and development, and range and launch personnel), an average cost is derived of approximately $130,000 per successful flight. This cost includes the basic cost of the rocket (most of the rockets launched in recent years are of the Nike family or Aerobees), payload costs, project personnel, data reduction, etc. This figure will increase for rocket experiments that are more sophisticated, for experiments that are combined into integrated packages, and for experiments using pointing controls of advanced design for astronomical experiments.

Table 2 lists some of the research rockets in use today, with an estimate of their annual use and base costs. The last figure is for the rocket motor, fins and nose cone, but not for telemetry and command systems, nor for the payload; shipping costs are also excluded. There is also a brief resume of the particular virtues and drawbacks of each rocket. Included in the list are some rockets currently under development or test, which seem likely to be added to the group of available sounding rockets.

The word "payload," in the table, refers to the experimental equipment plus recovery gear (if needed), telemetry, and batteries, but does not include the nose cone and its associated supports or adapters. For example, to obtain the total weight of the nose section of a Nike-Cajun or Nike-Apache, add 12 kg to the "payload"; add about 35 kg for the Nike-Tomahawk, and 2 kg to the Deacon-Arrow.

Table 3 summarizes the main satellite boosters being used by NASA for scientific satellites, along with their base costs. The total cost of a satellite flight is many times the cost of the booster, but due to the large booster cost, the factor is usually less than for sounding rockets (where it is a factor of 5 to 10 for exploratory experiments).

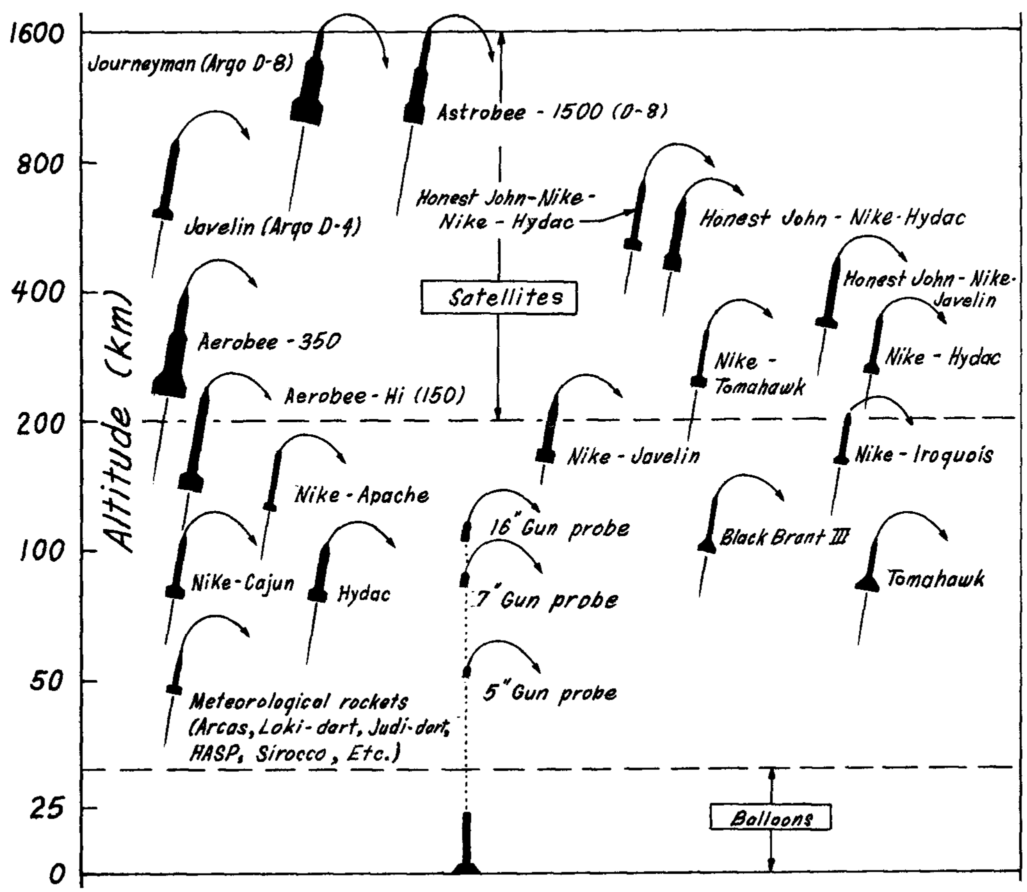

Figure 8. Currently used sounding vehicles for upper-atmosphere and space research. Note: The apogee altitude of a rocket depends on its payload, and the altitudes shown here are those attained with minimum payloads. All rockets shown have actually flown a number of flights, so their characteristics can be considered as proven. There are other rocket combinations that have been used experimentally.

Table 2. Characteristics of Currently Used Sounding Rockets

Existing Rockets	Annual Use	Base Cost	Payload, Altitude	Virtues	Drawbacks	Comments
Arcas Met	~2000	~$2,000	5 kg, 80 km	Relatively inexpensive, reliable; suitable for routine use	Still too expensive for expanded routine use; limited to rocket ranges or ship launches	One-way rockets under study to provide more launch versatility; boosted Arcas could be used for D-, E-region studies for compact payloads
Judi-Dart	~100	~$700	2 kg, 80 km	Less expensive than Arcas; less dispersion; could become routine met. rocket when all instrument problems solved	Small payload space; has taken some time to perfect solid-state high-frequency telemetry system	Very useful for passive payloads (chaff) for wind studies
Nike-Cajun	~100-200	~$6,000	25 kg, 120 km	Readily available, reliable system; adequate payload volume for many experiments	Cumbersome to handle, high spin rate; production of Nike booster discontinued but may be stockpiled for several years	General "work-horse" rockets; suitable for wide variety of experiments
Nike-Apache	~100-200	~$6,000	25 kg, 160 km	Same as for Nike-Cajun	Same as for Nike-Cajun	Same as for Nike-Cajun

Table 2. Characteristics of Currently Used Sounding Rockets (Cont.)

Existing Rockets	Annual Use	Base Cost	Payload, Attitude	Virtues	Drawbacks	Comments
Black Brant III	10-20	~$10,000	20 kg, 160 km	Single stage, good payload volume		Still unproven in quantity; may become popular rocket for E-region work if reliability is satisfactory; can be staged with other BB rockets to provide good range of capability
Tomahawk, Hydac	45	~$10,000	40 kg, 100 km	Single stage; good payload volume	Limited altitude-payload trade-off; unstable at low payload weights	Successful second stage but of limited use as single stage
Deacon-Arrow or Kisha-Judi	10	$2,500	6 kg, 110 km	Inexpensive; good performance (high acceleration)	Small payload weight and volume	Deacon booster not in production; higher performance Kisha available
Nike-Tomahawk	50	~$15,000	30 kg, 300 km	Good payload to volume capability for ionospheric studies; solid propellant	Some flight parameters not established; handling problems associated with large rockets, but not as severe as with liquid propellant types	Increasing demand for rockets of this payload-altitude range; in over 100 flights has proven stable and reliable

Table 2. Characteristics of Currently Used Sounding Rockets (Cont.)

Existing Rockets	Annual Use	Base Cost	Payload, Altitude	Virtues	Drawbacks	Comments
Nike-Iroquois	(new)	~$10,000	20 kg, 220 km 100 kg, 100 km	Good payload-altitude versatility; non-magnetic; good altitude characteristics		Still unproven in quantity
Aerobee 150/150A	50-60	~$30,000	75 kg, 240 km 200 kg, 120 km	Gentle take-off and ride; controlled cut-down for range safety; altitude controllable by shut-off command; large payload space for many kinds of optical experiments	Can only be recovered readily at White Sands or Ft. Churchill; not large enough for large spectrograph; apogee too low for some observations	Obsolete booster, in spite of many improvements in 20 years; still needs some re-engineering to increase performance and reliability
Aerobee 300		$ 38,000	25 kg, 400 km	Same as for Aerobee 150/150A	Same as for Aerobee 150/150A	Being phased out by NASA in favor of Nike-Tomahawk
Aerobee 350	1	$ 140,000	70 kg, 450 km	Low-g; high payload volume	No pointing control available; can only be launched from Wallops	Under development; first flight successful
Argo D-4 (Javelin)		$ 50,000	45 kg, 1000 km	High reliability	Complex rocket (4-stage)	Fills need for the few experiments requiring this altitude

Table 2. Characteristics of Currently Used Sounding Rockets (Cont.)

Existing Rockets	Annual Use	Base Cost	Payload, Altitude	Virtues	Drawbacks	Comments
Argo D-8 (Journey-man)		\$ 140,000	60 kg, 1800 km		Cost too high for many experiments.	Being replaced by the Astrobee-1500
Blue Scout Jr. or equivalent		~\$1,000,000	?	Can perform critical rocket measurements in portion of magneto-sphere in coordina-tion with satellite observations	Expensive, poor reli-ability	

Table 3. Characteristics of Satellite and Space-Probe Boosters Currently being used by NASA

Booster System	Cost Launched (millions of dollars)	Capability	Type of Mission
Scout	1.4	110 kg, 550 km	Explorer, vertical probes, re-entry
Delta	3.1	360 kg, 550 km 48 kg, escape 41 kg, Mars, Venus	Explorer, meteorological, communications, OSO
Improved Delta	3.1-3.4	430 kg, 550 km 68 kg, escape 55 kg, Mars, Venus	Explorer, meteorological, communications, OSO, Pioneer
TAT Improved Delta	3.4	550 kg, 550 km 370 kg, escape 72.5 kg, Mars, Venus	Same as above
Thor-Agena	5.7	550 kg, 550 km (WTR)[a] 725 kg, 550 km (ETR)[b]	POGO, Nimbus, Isis
TAT-Agena	6.2	815 kg, 550 km (WTR)[a] 1000 kg, 550 km (ETR)[b]	Same as above
Atlas-Agena	7.9		Lunar and planetary, EOGO, Lunar Orbit, ATS, OAO
Atlas-Centaur	11.9	3950 kg, 550 km 910 kg, escape 590 kg, Mars, Venus	Surveyor
Saturn-1B	29.8	16,000 kg, 192 km	

[a]WTR, western test range.
[b]ETR, eastern test range.

5. MANPOWER AND ACTIVE RESEARCH GROUPS

Some thirty universities are currently conducting rocket-research or rocket-technological-support programs, supported largely by NASA and AFCRL, with a few programs under NSF support. Several federal agencies, principally NASA, NRL, AFCRL, BRL, Army Research and Development Activity (White Sands and Fort Monmouth), and AEC, have extensive rocket programs.

The number of rocket groups is growing rapidly, as some senior scientists move to new locations and start new groups, and as more universities become interested and are able to undertake programs in atmospheric and astronomical research using rockets.

Listed below, by scientific discipline, are the groups known now to be engaged in, or planning to be engaged in, rocket programs. In many cases, there are several independent groups at one university or federal agency, and many groups are interested in more than one research area. Assuming that a research group can prepare payloads and launch at least two rockets, it is seen that there are enough groups to call for a minimum national program involving the launch of some 150-200 rockets per year, not including meteorological rockets, vapor-trail experiments, and other experiments that are quasi-operational and standardized. Here, the usage is some thousands per year, launched by federal agencies. The data, however, are published and available to scientists for analysis.

U.S. ROCKET PROGRAM: CURRENT ACTIVE EXPERIMENTERS

(* Indicates New Groups)

Meteorology (12 Groups)

Air Force Cambridge Research Laboratories
Air Force Air Weather Service, Scott Air Force Base
Army Ballistic Missile Agency, Redstone Arsenal
Army Ballistic Research Laboratory
Army Research and Development Activity, White Sands
NASA, Goddard Space Flight Center
NASA, Langley Research Center
NASA, Marshall Space Flight Center
Navy Pacific Missile Range, Point Mugu
Georgia Institute of Technology
Geophysics Corporation of America
Sandia Corporation

Aeronomy (24 Groups)

Air Force Cambridge Research Laboratories (4)
NASA, Ames Research Center
NASA, Goddard Space Flight Center (4)
NASA, Lewis Research Center
Naval Research Laboratory, Hulburt Center
Dudley Observatory
Jet Propulsion Laboratory
Johns Hopkins University
Kitt Peak National Observatory
National Center for Atmospheric Research
Northeastern University
Southwest Center for Advanced Studies
University of Michigan (2)
University of Minnesota
University of Pittsburgh
Geophysics Corporation of America
Sandia Corporation

Particles and Fields (17 Groups)

Air Force Cambridge Research Laboratories (2)
NASA, Goddard Space Flight Center (4)
Rice University (2)
University of Alaska (2)
University of California
University of Minnesota
University of New Hampshire (2)
American Science and Engineering
Radio Corporation of America
Sandia Corporation

Radio Astronomy (2 Groups)

NASA, Goddard Space Flight Center
University of Michigan

Astronomy (12 Groups)

NASA, Goddard Space Flight Center
Naval Research Laboratory, Hulburt Center
*Smithsonian Astrophysical Observatory
*Columbia University
*Cornell University
Kitt Peak National Observatory
Princeton University
*Stanford Research Institute
*University of Arizona

University of Wisconsin
American Science and Engineering, Inc.
Lockheed Missiles and Space Company

Ionosphere (17 Groups)

Air Force Cambridge Research Laboratories (3)
Army Ballistics Research Laboratory
NASA, Goddard Space Flight Center (3)
Naval Research Laboratory, Hulburt Center
National Bureau of Standards, Central Radio Propagation Laboratory
Pennsylvania State University
Southwest Center for Advanced Studies
University of Illinois
University of Maryland
University of Utah
Geophysics Corporation of America
Lockheed Missiles and Space Company
Sandia Corporation

Solar Physics (12 Groups)

Air Force Cambridge Research Laboratories
NASA, Goddard Space Flight Center (2)
Naval Research Laboratory, Hulburt Center (2)
Harvard College Observatory (2)
Kitt Peak National Observatory
*Pomona College
University of Colorado
*University of Hawaii
*Western State College of Colorado

Test and Support (Not Counted)

Air Force Cambridge Research Laboratories
NASA, Goddard Space Flight Center (2)
State University of New York
Sandia Corporation

(Number in parentheses after names of some laboratories indicates number of groups in those laboratories in that discipline.)

Total number of groups involved in research with rockets: 95 groups (8 of these are new groups).

6. SCIENTIFIC DISCUSSION BY DISCIPLINES

THE NEUTRAL ATMOSPHERE

Members: C. O. Hines, F. S. Johnson, L. M. Jones, W. W. Kellogg, and L. B. Smith.

Contributors: N. W. Spencer and J. W. Townsend.

Introduction

The neutral (un-ionized) atmosphere of the Earth extends from the ground to extreme altitudes. While it can thus be thought of as constituting a single subject, in fact, the treatment of the atmosphere must change in many respects with increasing altitude. For example, the atmosphere from the surface to about 100 km is well mixed by turbulence and has a fairly constant mean molecular weight; above 110 km, turbulence is apparently suppressed, and molecular diffusion takes over, causing the amounts of the heavier gases to decrease more rapidly with increasing height than do the lighter gases; above about 500 km in the exosphere, the mean free paths become so large that the atoms can go into ballistic orbits, or escape completely from the Earth. There are other factors that are related to altitude, such as the dominance of the ionized part of the atmosphere in the upper ionosphere, the importance of chemical reactions to the composition and energy balance, the intensity of internal gravity waves in the mesosphere and ionosphere, etc.

In this report, therefore, a clearer understanding of the problems is attempted by dividing the atmospheric phenomena to be studied into the following classifications:

(a) Pressure, temperature, density, and composition from 60 to 200 km.
(b) Pressure, temperature, density, and composition above 200 km.
(c) Motions above 60 km.
(d) Pressure, temperature, density, and winds below 70 km.

Category (d) concerns an extension of the techniques of upper-atmosphere synoptic analysis from the ceiling of the balloon radiosonde network (about 30 km) to the ceilings of the small rockets currently referred to as "meteorological rockets." This is an important area of study in which a great deal of activity is already under way, and it is gaining increasing support from the meteorological community throughout the world. The status of this effort is reported briefly at the end of this section, but it is not discussed further in the Committee's conclusions and recommendations as it is in a somewhat different category than the rocket research above 60 km, both scientifically and administratively.

This section concentrates on upper-atmosphere research into the dynamics and structural parameters above 60 km. The rocket flights devoted to this area in the past 5 years have been examined (Table 4). Observations

Table 4.

Summary of Sounding Rockets Launched Between the Beginning of 1960 and the End of May 1965[a]

	1960	1961	1962	1963	1964	May 1965	Totals
PTρC	7	17	23	25	39	43	154
Motion (vapor trails and chaff)	75	14	63	39	30	15	236
Motion and T (grenades)	4	13	14	8	33	13	85
Totals	86	44	100	72	102	71	475

[a] For the purpose of measuring the pressure (P), temperature (T), density (ρ), composition (C), and motion of the neutral atmosphere above 60 km. (Five satellites devoted primarily to the neutral atmosphere were launched between 1960 and 1965.)

required in the coming 5 years to advance this important field adequately are discussed in terms of altitude, temporal and geographic coverage, and so on. The role of satellites is also considered. From this, a picture of the content of the scientific program that could be pursued can be drawn.

Since a scientific program requires capable investigators and institutions interested in carrying it out, the manpower situation is also discussed. The result, when the content and scope of the program are considered in conjunction with the probable demands from the scientific community, is an estimate of the level of effort that should be supported in this area. Breakdowns of the recommended numbers of rockets are given in Tables 4 and 5 of this section.

Past Level of Effort

In order to give some insight into the level of effort that has been devoted to studies of the neutral atmosphere above 60 km, the following summary for the past 5 years is presented. These include launchings by NASA, universities, DOD, AEC (Sandia Corp.), and others. As there is no central record of all of these programs, a few launchings may have been overlooked.

The grenade experiment, which measures both temperature and wind to about 95 km, is listed separately because the inclusion of its relatively large numbers in either PTρC or Motion would distort the totals. The number of other dual-purpose flights is small.

In this report, a distinction is made between the region 60 to 200 km and the region above 200 km. Table 4, however, does not separate out the eleven pre-1965 launchings yielding data for attitudes above 200 km.

Neutral-Atmosphere Pressure, Temperature, Density, and Composition from 60 to 200 km

Introduction. The atmosphere between 60 and 200 km can be considered as a complex region of transition between the lower atmosphere and the exosphere. It encompasses most of the photochemical changes of the atmosphere, and includes the zone of transition from turbulent mixing to diffusion. It can also be considered as an entity, for purposes of this report, since it is subject to measurement by a group of rockets and techniques of similar complexity and cost.

The suggested lower bound of 60 km is below the altitude of both appreciable oxygen dissociation and diffusive separation, and the region includes the upper mesosphere, where the recombination of atomic oxygen contributes to the thermal budget. It is thought that most photochemical reactions are complete above 120 km, but measurements should carry us high enough to observe the thermospheric temperature gradient, as this is important for upward modeling. ("Modeling of the atmosphere" in this sense means calculating the vertical distribution of all constituents in the region governed by molecular diffusion, above about 110 km, from a knowledge of their abundances at a particular level combined with information about the complete temperature distribution.) Establishing the base conditions of temperature and composition for upward or downward modeling is, in fact, one of the principal reasons for measurements in the region to 200 km. Another is to complete our understanding of the photochemical reactions taking place in the upper atmosphere.

It is necessary that the temperature, density or pressure, and composition of the atmosphere be measured above 60 km as functions of altitude, location, and time, in sufficient detail to disclose the general patterns and the systematic variations that occur with (i) time, (ii) season, (iii) sun-spot cycle, (iv) location, and (v) the condition of other parameters (meteorological, auroral, geomagnetic, and perhaps others). The limits and characteristics of random variations that cannot be associated with other geophysical parameters should also be determined. In general, vertical profiles at several latitudes and horizontal mapping at several levels will be required to give a complete picture.

Progress to date. The determination of mass density is carried out by measuring the drag on falling spheres and by pitot tubes, to heights of 120 or 130 km, and, to 150 km, by radiation absorption in the x-ray and ultraviolet regions. The grenade experiment yields temperatures (as well as wind) to 95 km. Some temperatures in the thermosphere have been obtained from chemical releases. In general, the average conditions thus obtained, and given by the "U.S. Standard Atmosphere, 1962," are acceptable. A very important recent contribution has been the improved measurements of atomic oxygen, which show O/O_2 ratios of 1:1 at 117 km. These results, which were correctly anticipated by optical-absorption measurements in 1955, appear to give acceptable values for mean molecular weight throughout the lower thermosphere. As a consequence, average temperatures can be calculated from densities. Recent rocket flights that give density profiles of nitrogen, thus permitting the computation of temperature in the region of diffusion equilibrium, are a forward step.

The ozone region is thought to be reasonably well understood conceptually. However, there has not been adequate mapping of the upper portion (above about 50 km), which is normally considered to be the photochemical equilibrium region.

Aside from ozone, the varying concentration of water vapor, and a relatively small amount of oxygen dissociation on approaching 100 km, the atmosphere is thought to be well mixed to an altitude slightly in excess of 100 km. Above 115 or 120 km, diffusion equilibrium is generally thought to prevail for all but a few lighter atmospheric constituents, whose equilibrium is perturbed by flow through the atmosphere and escape near its top. Only recently has any detailed understanding of the transition region between the mixing region and the diffusion-equilibrium region developed. However, it is now recognized that the effects of mixing and molecular diffusion can be combined and applied to the transition region; this has been done only in a conceptual sense, as the experimental data that are available are not sufficient to provide for a test of the theory. The effects of large-scale circulation on the transition region have not been worked out, although it appears that this problem is amenable to attack.

While some of the chemistry of the region above the ozonosphere is understood, there are serious gaps in our knowledge of the chemical processes involved, and many of the needed rate coefficients are missing. However, reactions have been identified that appear adequate to account for most of the chemical changes that are known to take place in the upper atmosphere, for example, the recombination of atomic oxygen and the role played by the oxides of nitrogen. The magnitudes and variations of these reactions should be established.

Outstanding Problems

a. The vertical profiles of minor constituents of the atmosphere below 120 km need to be measured as functions of latitude and time. The constituents that particularly need to be measured include atomic oxygen, argon, water vapor, methane, and perhaps also helium and atomic hydrogen. Since the mean free path below 100 km is not sufficient to permit the operation of conventional mass spectrometers without pumping systems, the means to make these measurements still need to be developed.

The importance of these measurements is that they constitute the vital parameters that describe the transition between mixing and molecular diffusion as the predominant physical processes that control atmospheric composition. The upward flow of atomic hydrogen, the downward flow of atomic oxygen, and the static behavior of light and heavy gases (such as helium and argon) all enter into the combined (eddy and molecular) transport equation of the transition region in different ways. Furthermore, the main source of atomic hydrogen is the dissociation of water vapor and methane in the 60 to 100 km region, and the dissociation and recombination rates are so poorly known that a direct measurement of their concentrations is needed. Any large-scale vertical motion associated with a general circulation in this atmospheric region can be expected to affect some of the above constituents differently from others, and this circumstance presents the interesting prospect that accurate measurements of the vertical profile of some or all of these minor atmospheric constituents will be interpretable in terms of the large-scale circulation of the lower thermosphere.

b. Variations of conditions in the lower thermosphere are not well known. Only recently, an equatorial series of density results with falling spheres was obtained and a day-night pair of composition flights at Churchill was made. Flights at AscensionIsland and White Sands have been carried out, and the NRL density measurements during the IGY cover this region. In general, however, coverage of the lower thermosphere is sporadic, in contrast to the regions below 90 km and above 200 km, where densities are well known from satellite drag observations.

c. Measurements are needed of the vertical profiles above 100 km and the horizontal distributions of major constituents, including O_2. For the vertical profiles, much greater accuracy is required than has been attained in the past; relative accuracy somewhere in the range of 2-5 percent should be attained. The accurate vertical profiles will provide a much more rigorous test of the theory of diffusive equilibrium and the deviations from it that are expected on the basis of flow, mixing, etc. The horizontal mapping, to the extent that the measurements can be extrapolated downward to the transition region, may be interpretable in terms of large-scale circulation and the intensity of mixing in the lower thermosphere.

d. Direct temperature measurements are needed for the region above 80 km, up to perhaps 200 km. Some vapor-trail data are available, but these are generally only for occasional altitudes, whereas the vertical profiles of temperature are what is actually needed. These profiles should be obtained with sufficient accuracy to provide an answer to the question of the extent to which internal gravity waves disturb the temperature profile. Vertical profiles of density or pressure can substitute for temperature only if done with great precision—more precision than seems likely to be achieved.

e. The effects of the field of motion on composition and temperature must be determined experimentally, especially for large-scale motion. As mentioned above, combination of improved theory and accurate measurement will probably provide the means of determining the large-scale field of motion from composition and temperature measurements.

Increasing note is being taken of the problem of the heights of the turbopause in establishing thermospheric conditions. The question arises of whether to use the small coefficient of molecular diffusion or the larger coefficient of turbulent diffusion in upward modeling. Chemical releases are helpful in this area.

f. The dynamics of noctilucent clouds should be investigated in terms of the motions and temperatures associated with them, and also in terms of the composition of the air (especially water vapor) and the character of the dust nuclei that are responsible for their formation.

Future Role of Various Observations

1. Ground-Based Measurements

So far as data on the neutral atmosphere are concerned, airglow measurements are apt to be the chief source of information from ground observations. To the extent that properties of the ionized medium can be related to the neutral atmosphere, some radio measurements may also contribute.

A valuable source of data from ground-based measurements is the reaction-kinetics laboratory, where reaction rates for atmospheric processes can be determined. This type of research, conducted in connection with lower thermospheric studies, should be encouraged. Such laboratories may also provide some of the new techniques that will be needed to make some of the more-difficult atmospheric measurements.

2. Rockets

The accurate determination of the vertical profiles of atmospheric constituents below 200 km must be done with rocket vehicles, since satellites cannot reach this region. At altitudes much above 100 km, extraordinary means must be taken to avoid contamination and distortion of ambient composition.

Temperature and density profiles up to 200 km must be acquired from rockets. The vertical profiles of minor constituents in the 80-100 km region can only be obtained with rockets. The same is true of the variation in the profiles with geographic location and with time. The techniques for making the required measurements, especially below 100 km, have not been developed.

A special case is ozone. Ozone distributions for the 60-100 km altitude region can be obtained with rockets, and possibly also with satellites, by observing ultraviolet absorption. If adequate instrumentation can be provided for use in the meteorological rocket program, the ozone measurements might be carried out in that program. Otherwise, they should probably be performed in satellites.

The situation with respect to experimental techniques and rocket vehicles in the 60 to 200 km region is mixed. The grenade experiment for wind and temperature between 30 and 95 km, usually carried aboard the Nike-Cajun, is well established. It is carried out typically about 15 times per year, and together with other methods has probed variations of meteorological significance as well as contributing to aeronomy. Falling spheres carried by Nike-Cajuns, when used with high-quality radars, yield densities to 120 km, temperatures to 100 km, and winds to 70 km. The low cost of the payload permits high-resolution time-scans at certain locations. Chemical releases are among the most frequently flown experiments. Some temperature data are obtained, but the principal contribution is in understanding motions, and these experiments are discussed below, in the section on motions of the neutral atmosphere in the mesosphere and above. Pitot-tube experiments also yield densities and temperatures to the 100-km region, and have often been employed simultaneously with grenades. Mass spectrometers have been used increasingly in the past 5 years for studies of the principal constituents in the range 90 to 200 km.

Some developments that would be of particular significance are:

(i) The inclusion of more grenades in a payload, for better vertical resolution.

(ii) The proliferation of high-quality radars and the development of a thin-film surface transponder (DOVAP) for inflatable falling spheres, to increase the geographic distribution of measurements.

(iii) The extension of mass spectrometer techniques to minor constituents and to the D-region of the atmosphere.

(iv) The further development of techniques for measuring temperature to the 200-km level.

(v) Development of experiments to measure the various gas kinetics coefficients in situ.

The achievement of any or all of these developments would permit the improved observations recommended in the body of this section without excessively increasing cost in money and manpower.

3. Satellites

As noted above, ozone measurements can in principle be made from satellites by observing the ultraviolet spectrum of scattered sunlight. Also, it has been suggested that airglow measurements from satellites could give the temperature of the mesopause region.

Neutral Atmosphere Density, Temperature, and Composition Above 200 km

Research Area. The primary problems for the neutral atmosphere are to map the physical structure above the level (about 120 km) where photochemical activity is still sufficiently rapid to affect the composition, and to understand the motions of the atmosphere to 300 to 400 km. In this region, controlled by molecular diffusion, upward modeling can, and has been, carried out on the basis of conditions (partly measured and partly assumed) at 120 km. Such modeling is probably valid on a static basis but dynamic conditions of heat conduction and mass transport impose problems not yet solved.

Progress to Date. Knowledge of the neutral-gas density in the upper thermosphere and exosphere has reached a stage at which definite empirical relations have been deduced that correlate the variation of atmospheric density with solar activity, time of day, latitude, geomagnetic activity, etc. The picture has emerged of an atmosphere in diffusive equilibrium within which the neutral composition is determined by photochemical process occurring below 120 km, and where the atmosphere responds to heating by expanding and contracting in the vertical direction.

This basic model and the empirical data related to the quantitative behavior of the atmospheric density have been obtained largely through the painstaking analysis of the drag-induced perturbations to satellite orbits. These analyses have led to the discovery of a number of basic spatial and temporal variations in the density of the thermosphere and exosphere the origins of which can be traced to the Sun and the solar plasma. If the gross simplification is made that the composition at 120 km is known and invariant with time, the theoretical models mentioned above become feasible. It can be shown, for example, that composition is a function of pressure alone for any temperature distribution to be expected, even in time-dependent atmospheres. (Thermal diffusion is assumed to be negligible.) This means that the entire effect of solar heating is to cause the atmosphere to swell or "breathe" in such a manner that every subsequent state is the result of merely distorting the scale height of the previous state.

The heat, which upon liberation in the thermosphere causes the vertical expansion of the atmosphere, was at first thought to be transferred to the atmosphere mainly by absorbed solar radiation in the extreme ultraviolet (XUV) range. When this radiation penetrates the atmosphere, it is absorbed in the form of thermal energy and in various molecular and atomic excitations, some causing dissociation and ionization. The energy of dissociation and ionization is only partially recovered as heat in the thermosphere; thus, only a portion of the energy lost from the incoming radiation heats the thermosphere. Even so, it has been thought that this energy would suffice to cause the observed behavior of the atmosphere. Major tests of this hypothesis are the comparison of diurnal and seasonal variations of atmospheric density, which are predicted by the theory, using only XUV heating with the density behavior observed by satellites.

It has been demonstrated by many independent investigations that the atmosphere responds rather vigorously to the differences between heating during the day and during the night. At mid-latitudes, the diurnal density variation is a strong function of altitude, increasing with height and peaking at about 14h local time. The data of L. G. Jacchia indicate that the constant-density surface rises about 200 km from night to day in the vicinity of 800 km. Similarly, responses to solar activity of short time variation and also related to the 11-year solar cycle have been brilliantly revealed.

Geophysical Problems. I. Harris and W. Priester used their theoretical model to see if the results of solar heating and the basic assumptions would compare favorably with the experimental data. The result of their numerical experimentation was not a confirmation, but indicated that, with solar radiation alone, the density should reach a maximum at 17h local time. It was found that, in order to increase the correlation between observed and theoretical results, an additional heat source must be added to the thermosphere heating that peaks at 9h local time. The 9h peak is in phase with peak intensities of micropulsations in the magnetic field, although the physical explanation is somewhat vague. Harris and Priester's second heat source is often called "corpuscular," but the actual heating is not expected to be directly the result of precipitation of energetic particles, since the results cited here apply to mid-latitudes and equatorial regions, where energetic particles would not penetrate.

The "second source," as a physical process, has not been discovered at this time. This is undoubtedly the result of a number of factors, probably the foremost being that the "second source" is not a single entity, but a complex combination of factors not accounted for in the theoretical model.

Future Observations. What data would be of primary interest in a physical theory such as has been outlined? First, composition data are of great importance, for, although the density results obtained from satellite drag have been good indicators, composition will help to answer fundamental questions concerning the photochemical processes occurring in the lower thermosphere. One of the primary factors to be evaluated would be the diurnal variation of composition. This information would indicate, it is believed, a phase lag behind the photochemical process of the order of 1 day, an effect due to the relatively long time required for upward diffusion to occur. It is possible that this phenomenon could be partially responsible for the phase change found in the density data that has required the "second source." In a similar investigation of dynamical response, the variation of composition combined with measurements of corpuscular fluxes during the commencement of a magnetic storm would shed considerable light on the nature of the mechanism by which the solar plasma affects the atmosphere. It has been shown that the density of the atmosphere is correlated with the magnetic index A_p; however, the correlation is different for magnetic storm events and for the normal more placid annual and semiannual variations. However, the question of the mechanism by which the solar wind affects the atmosphere below the magnetopause is still unanswered. The measurement of composition with high spatial and temporal resolution could provide the key to the puzzle.

The second quantity of particular interest in a study of upper-atmosphere energy balance and heating processes would, of course, be temperature. Unfortunately, it has not been possible to obtain accurate temperatures directly from density by using models of the atmosphere. It should be noted that it is possible that, when using the models to obtain temperatures from densities, one is prejudging the atmosphere and forcing the data to fit a preconceived composition model. What is needed is to extend the measurement of temperature, obtained directly from the distribution of molecular velocities, to higher altitude in the region accessible to satellites.

From composition and temperature data, the following studies should be undertaken:

a. A model should be formulated of the atmosphere downward from the satellite altitudes to the 120-km level to provide approximations of the variations in this critical region for use in the evaluation of photochemical thermal processes taking place there. In particular, the composition and temperature variations should be compared with XUV measurements from the Orbiting Solar Observatory and other sources. Also, the composition and temperature measurements should be compared with the low-energy proton and electron energies and fluxes. The relative contributions of the XUV flux (from other experiments) and of the corpuscular flux should be compared with the thermal variations.

b. Mean-molecular-weight profiles should be calculated for the minimum purpose of verifying the static diffusion-controlled model atmospheres and the temperatures derived from them.

Satellites can do the best job of mapping the patterns of composition and temperature changes corresponding to the density variations on which the new but incomplete physical models have been based. Such mapping is essential to an ultimate understanding of the dynamics of the atmosphere above 200 km and to the creation of adequate models for the entire thermosphere. The time-dependent variations of such mapping can be used for correlations with other phenomena, such as changes in x rays from the Sun and in particulate flux, for evaluating thermal sources.

Sounding rockets reaching into the thermosphere are required for precise and instantaneous vertical profiles and for single-level comparisons with results from satellites whose orbits they intercept.

Motions of the Neutral Atmosphere in the Mesosphere and Above

Definition. The motions discussed are those of the neutral gas, which are controlled primarily by ordinary hydrodynamic forces to heights of 100-110 km, but which become increasingly subject to hydromagnetic (ion-drag) effects at higher levels. They include systems of large horizontal scale (rotation, circulation, tidal oscillations, and high-latitude convection), systems of an intermediate scale (about 10-1000 km horizontally, so-called irregular winds), and systems of a micro scale (turbulence). The neutral-gas turbulence appears to terminate near 100-110 km, and to be absent above; the tidal oscillations and irregular winds appear to maximize at about this transitional level, or slightly above; the problems of rotation (or absence of rotation), of circulation, and of high-latitude convection, have yet to be established by even the most preliminary observational studies, although theory suggests that they will be challenging, particularly for attitudes above 150 km, where hydromagnetic effects can predominate.

These motions are intimately related to many, or most, other studies of the upper atmosphere, both for the neutral and the ionized constituents, and, in some cases, also for the energetic particles of the magnetosphere. Observations on a global scale are necessary if the large-scale systems are to be defined adequately; observations on a more-local basis are adequate to elucidate the nature of the medium and of micro-scale systems, although a full latitudinal coverage of these systems will be required if global variations of related dependent parameters (e.g., composition) are to be understood.

Diurnal variations in all but, possibly, rotation and circulation constitute an essential part of the area of interest, and preliminary results suggest that seasonal variations might also be an essential factor; variations with solar activity, solar cycle, and lower-level meteorology provide further areas of probable but unexplored interest.

Progress to Date. Ground-based measurements of meteor trails have served to outline some of the problems that are to be encountered (e.g., anomalous pattern of tides, irregular winds, turbulent diffusion), but such measurements have been made at only a few sites and they extend only up to heights of about 105 km. A number of sodium-vapor trails have been released over the years, to permit wind studies to 200 km, but the infrequency of their release and their operational limitation to dawn and dusk observations have vitiated much of what one would hope to gain from a program of seeding. Recent trimethyl-aluminum (TMA) releases have permitted the clear identification of tidal oscillations on the two occasions when sequences of releases were possible, and they have opened the way for further identification and clarification of the various systems, but even they are not operationally effective for daytime studies. A theory of the irregular motions, based on a gravity-wave interpretation, has been developed in considerable detail but has not yet been tested adequately. The identification of the "turbopause," above which turbulence does not extend, has been made on a number of occasions, but not always without ambiguity, and with totally inadequate temporal and geographic distribution. Indirect techniques of measuring motion from the ground have been attempted (e.g., ionospheric "drifts"), but their relevance to the neutral-gas motions has yet to be established. A degree of association between the wind systems and other phenomena (e.g., sporadic-E ionization, quiet-day current systems) has been indicated observationally, but not established or exploited.

Outstanding Problems

a. A postulated inhibition of rotation at high latitudes at 150 km and above has yet to be tested; if it exists, it would have major implications with respect to the geomagnetic-field configuration, auroral precipitation, field-aligned ionization irregularities, and further diverse phenomena.

b. The general circulation has yet to be established, as have its consequences with respect to vertical motions and related photochemical reactions.

c. The dominant tidal components must be identified, their variations established, and the reasons for these variations explored; such studies are necessary to an understanding of variations in overlying ionization distributions, and may yield valuable information on the temperature and wind structure below. The transition from hydrodynamic to hydromagnetic control of the tides should be determined.

d. The utility of airglow and ionospheric "drift" measurements as techniques for determining tidal oscillations cheaply must be evaluated by direct comparison with other measurements.

e. The degree to which the neutral atmosphere is set in motion at the auroral zones, during magnetic storms, must be established as a step in the understanding of storm phenomena.

f. The gravity-wave theory of irregular motions should be tested critically by comparison of wind and structure measurements.

g. The spectrum of turbulence should be determined and the nature of the turbulence established; variations in the height of the turbopause—diurnally, seasonally, and geographically—should be measured and correlated with possible sources (tides, gravity waves) and consequences (composition, heating) of the turbulence.

Future Roles

a. Ground-Based Measurements

Radar studies of meteor-trail drifts provide directly applicable information, and may provide the cheapest means of obtaining global coverage, but are available only to 105 km. Measurements of "drifts," of structure in noctilucent clouds, and of airglow cells, may be useful, but their interpretation has yet to be established. No crucial experiments seem possible by ground-based techniques.

b. Rockets

The most direct measurement of winds over most of the relevant height range is provided by seeding experiments, established via rockets or guns. Falling-sphere and grenade experiments are feasible to heights of 70 to 90 km, but not above. Much of the information called for above would be obtained directly from an adequate exploitation of these techniques, with the seeding experiments favored. The rocket program available for these purposes alone could be usefully expanded by a factor of 2 immediately, and a much further expansion over the course of years would be vital to a thorough study. Critical experiments to be conducted for some of the outstanding problems, discussed above, require complementary studies, primarily rocket-borne—e.g. temperature gradients and/or ionization motions for (a) and (e), ground-based drift measurements for (d), simultaneous winds, temperature, and density and/or pressure measurements for (f). New techniques must be applied, and sometimes developed, even in the seeding experiments—e.g., the laying of horizontal trails to reveal vertical motions, the exploitation of the long twilight (for sodium) or winter night (for TMA) at high latitudes, and the development of an effective daytime seeding technique.

c. Satellites

No satellite techniques have yet been devised that can contribute to this subject. One analysis exists in which drag data are interpreted to imply an eastward prevailing wind at 200-300 km, but the measurement is inherently so coarse and unrelated to other measurements as to be of little value in elucidating the significance of the wind, and the interpretation is in any event of quite doubtful validity.

Meteorological Rockets

As explained in the introduction to the neutral-atmosphere section, meteorological rockets are considered, for purposes of this report, to be in a separate category both scientifically and organizationally. However, in many countries, they are an important aspect of emerging space research programs and this report would not be complete without a brief review of their status.

Definition of the Particular Research Area. Meteorological rockets have been developed to study motions and temperatures in the atmosphere above the levels usually obtainable by meteorological balloons. Thus, their data become most useful above 30 km (about 10 mb), and the current technology of these small rockets and their associated observing systems has placed an effective upper limit of about 70 km for wind observations and about 60 km for temperatures.

There are now about 20 launching sites in the U.S. Meteorological Rocket Network (see Table 1). Not all of these are used regularly, but about nine are making at least one rocket ascent per week; most of these make three flights per week during the World Geophysical Intervals and special alerts of the IQSY, and daily flights are being made at White Sands (except on weekends). The data so obtained provide sufficient coverage in time and space to permit synoptic analyses of the atmosphere to 50- or 60-km altitude and to show day-to-day variations in conditions in the upper stratosphere. On a few occasions, rocket flights have been made around the clock at 1- or 2-hour intervals, and these have given important new information on the diurnal variations of temperature and winds.

Data from the U.S. Meteorological Rocket Network are collected centrally by Texas Western College, under contract with the U.S. Army. These data are published monthly and are also available in punch card form. No corresponding central collection of meteorological rocket data exists elsewhere (unless it is in the Soviet Union, about which we have no information).

Progress to Date. Since the beginning of the Meteorological Rocket Network in 1958, there have been about 4,000 ascents, and these have given us a fairly good picture of the mean conditions in the stratosphere up to about 60 km. At mid-latitudes, the winds in this region are generally westerly in winter and easterly in summer. A notable breakdown of the westerly winds usually occurs in late January or February, during which they temporarily shift to the east at most stations in the network above 30° latitude. Subsequently, the west winds return, and hold until the changeover at the time of the equinox. This late-winter reversal of the winds is accompanied by a sudden warming in the stratosphere above some high-latitude stations; the phenomenon is generally referred to as a "stratospheric warming," or "stratwarm."

The large-scale motion implied by these winds suggests that this part of the atmosphere is operating as a heat engine, with heat added at low latitudes and removed at high latitudes in winter, and with heating at a maximum at high latitudes in summer. The motion in this region, like the motion in the troposphere, is unstable on a global scale, and moving disturbances occur, particularly in the winter.

In the tropics, the wind system behaves quite differently, with a reversal in the zonal wind from east to west and back again every 26 months. The explanation for this peculiar periodicity has not been definitely established, but may have to do with the nonlinear interactions between solar heating, ozone in the stratosphere, and stratospheric motions that transport ozone meridionally.

The most pronounced short-term disturbance in the stratosphere occurs in the late winter at middle and high latitudes, as already mentioned. These stratospheric warmings are now fairly well documented, though we have never had an adequate set of meteorological rocket observations in the region most affected by the change in circulation. During the IQSY a system of "stratwarm alerts" was established in which the U.S. Weather Bureau notified the Meteorological Rocket Network (and other stations in the IQSY international communication system for "geophysical alerts") when a stratospheric warming was beginning. Unfortunately, last year, with the system in full operation for the first time, the stratospheric warming was relatively unpronounced.

An interesting recent development has been the observation from White Sands, by means of serial ascents throughout the day and night, of a surprisingly large diurnal temperature variation (about 20° at 50 km) and a correspondingly large diurnal variation of the wind.

Outstanding Problems. Even though the U.S. Meteorological Rocket Network is the most extensive in the world, and covers a large part of the North American continent and the adjacent oceans, there are large gaps in this network. Also, it is still impossible to draw a hemispheric analysis in the stratosphere, since, even during the IQSY when an effort was made to encourage other countries to engage in meteorological rocket ascents, there were too few observations at other longitudes to fill in the hemisphere. Thus, with such phenomena as stratospheric warmings, for example, in which the center of the large-scale perturbation moves from one part of the world to another while it is taking place, it is still impossible to observe the complete picture in the upper stratosphere. The best that has been done is to piece together the information from the global balloon network, up to 30 km (or 10 mb), and to take advantage of the observations in the North American sector of the hemisphere at higher altitudes. Our current three-dimensional picture of this phenomenon is, therefore, far from complete, and so is the theory to explain it.

The recent observation of the large diurnal variation of temperature and winds has raised a number of questions concerning the heat budget of the stratosphere and its tidal response to such a thermal forcing function, as well as whether the current practice of making observations at the same Greenwich time at all stations puts a large bias in the wind statistics due to the local diurnal variation, which will have a different phase at each station, depending on its longitude.

Of great practical importance is the question of the relationship between the stratosphere and the lower atmosphere, on the one hand, and between the stratosphere and the mesosphere and thermosphere on the other. There is some evidence that disturbances propagate vertically, but these are poorly documented due to the difficulty of obtaining a coherent three-dimensional picture of the entire atmosphere. This is a subject that requires intensive observational and theoretical study, and may lead to an understanding of how variations in solar or geomagnetic activity can have an effect on the pressure patterns in the lower atmosphere. (There is some rather impressive evidence that there are some connections.)

The gravity waves that are believed to be important at higher levels, as already discussed in the section above, on motions of the neutral atmosphere in the mesophere and higher, are likely to have their origins at the lower levels where meteorological rockets operate. The details of how these gravity waves are generated cannot be elucidated, however, until we have more refined observations of the winds at the lower levels.

Future Roles of Meteorological Rocket Observation Systems. the development of the U.S. Meteorological Rocket Network, from its infancy some seven years ago to a full-fledged network, has been somewhat analogous to the development of the upper atmosphere radiosonde network. It seems inevitable that, as requirements for information above 30 km become clearer, expansion of the network, both in density and in coverage of the globe, will continue. Other countries are beginning to establish their own stations, and the World Meteorological Organization has already taken steps to insure that the data from the emerging hemispheric rocket network will be properly disseminated and achieved.

There is a continuing need for the development of cheaper vehicles and payloads, in order to reduce the cost of this extensive rocket program. A new miniaturized rocketsonde package has been developed by the Army for use with the relatively small Judi rocket; this package is compatible with the standard GMD-2 ground tracking and telemetry set. This is but one of the new developments that holds promise of improving the operation and reducing its cost.

The development of either frangible or consumable rockets that will reduce the hazard of falling objects has been under way for some time, but it is not yet clear that the hazard can be eliminated in this way. If it could, it would be possible to set up meteorological rocket launching stations in regions where they would not now be permitted. An alternative approach is to use gun-launched probes, in which the accuracy of the trajectory is sufficient to insure that the probe will fall to Earth within a limited area. There is a possibility that gun-launched probes will also be cheaper. Developmental work on this launch technique is being carried out by the U.S. Army, in conjunction with the Canadian government.

Summary of Conclusions and Recommendations Regarding Research on the Neutral Atmosphere

Sampling Requirements. The sampling requirements for the outstanding problems of the neutral atmosphere are:

Vertical profiles of the spot type at particular times and with good precision to examine fundamental physical and photochemical processes.

Temporal-variation samplings to identify diurnal, seasonal, and solar activity effects.

Temporal- and geographical-variation samplings, for time, place, and altitude modeling.

Problem Areas. The major problem areas, in terms of the two altitude ranges discussed in this report are:

a. 60 to 200 km

Most of the challenging problems in the study of the neutral upper atmosphere—both quasi-static and dynamic—lie in the height range 60-200 km, and require direct probe observations for their elucidation. These facts immediately point to rocket (or gun) techniques as opposed to satellite techniques as the means of observation. Fortunately, for a physical understanding, much that is required can be derived from vertical soundings, and so can be gained by proper utilization of rockets launched from individual sites. Even in those studies in which geographical variations are important, however, rockets must still be employed because of the height requirements; use of multiple launchings from spaced sites (including mobile shipborne sites) is therefore essential.

b. Above 200 km

Problems relating to the levels above 200 km, while somewhat less complex, are nevertheless real and pressing. Rockets will be the primary vehicles for the attack on these problems also, because of the importance of vertical scans to an understanding of operative factors. The difficulties of multiple launches can be somewhat relieved, however, for satellites can provide a valuable service in the detection of geographic variations and interpolation between more widely placed rocket sites.

Recommendations. A detailed discussion of the research program, past and future, on the neutral atmosphere is contained in the section on the neutral atmosphere, in Section 6, above. Excluding meteorological rockets, which are technically and organizationally in a somewhat different category than the rockets of the Nike class and larger, the number of important scientific problems identified and the number of groups eager to undertake these problems are both on the increase. While an increased number of launchings will make a substantial advance in the science of the neutral atmosphere, the limit on the number will probably be determined by the capabilities of the groups likely to be in the field. The Committee therefore makes the following recommendation:

Recommendation 13. (a) The number of sounding rockets to be launched for probing the neutral atmosphere in the period 1966 to 1970 should be about 950, which is approximately double that launched in the period 1960-1965. Since these will in many cases carry more sophisticated payloads than earlier rockets the anticipated costs will be three times that of the 1960-1965 period. (See Tables 5 and 6 for a further breakdown of rocket requirements, and see also General Recommendation in Section 2.)

Table 5.

Recommended Sounding-Rocket Launchings from 1966 to 1970 Compared with the Yearly Average for the Period from the Beginning of 1960 through May 1965[a]

							Yearly Averages	
	1966	1967	1968	1969	1970	Totals	1966 to 1970	1960-May 1965
PTρC, 60-200 km	56	51	60	62	66	295	59	27
PTρC, above 200 km	10	18	19	19	19	85	17	2
Motion above 60 km	66	70	93	97	97	423	85	44
Grenades	30	30	30	30	30	150	30	16
Totals	162	169	202	208	212	953	191	88

[a]These rockets are for the purpose of measuring the pressure (p), Temperature (T), Density (ρ), and composition of the neutral atmosphere from 60 to 200 km and above 200 km and the motion of the neutral atmosphere from 60 km upward.

Recommended satellites to be launched between 1966 and 1970: seven, about evenly spaced in time.

Table 6. Breakdown of Projected Rocket Launchings by Agencies

	Goddard SFC	Michigan HAEL	Marshall SFC	Sandia	AFCRL	GCA	New
60 to 200 km							
1966	10	16		15	12		3
1967	10	16		10	12		3
1968	10	20		10	15		5
1969	12	20		10	15		5
1970	12	24		10	15		5
Above 200 km							
1966	5	2	5	3	2		1
1967	5	2	5	3	2		1
1968	5	3	5	3	2		1
1969	5	3	5	3	2		1
1970	5	3	5	3	2		1
Motions							
1966				12	30	20	4
1967				12	30	20	8
1968				12	40	20	21
1969				12	40	20	25
1970				12	40	20	25
Grenades							
1966	30						
1967	30						
1968	30						
1969	30						
1970	30						

(b) A total of about seven scientific satellites devoted to study the neutral properties of the atmosphere should be launched between 1966 and 1970. This number is judged to be adequate for the requirements of this discipline and is in accord with current plans. (See General Recommendations in Section 2.)

(c) In order to increase the scientific productivity of the total program it is recommended that coordination and comparison of experiments be arranged among the investigators. (See General Recommendations in Section 2.)

THE IONIZED COMPONENT OF THE ATMOSPHERE: IONOSPHERIC PHYSICS AND AERONOMY

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Introduction

Certain areas of ionospheric physics, such as electron-density investigations using ground-based radio techniques, have had a long history of study prior to the advent of space vehicles. The continuing motivation for this study was the need for reliable radio communications using the reflection of radio waves from the ionosphere. Because a great abundance of ground-based data was available, it has not proved necessary to adopt the synoptic approach to rocket and satellite ionosphere experiments that has been necessary in the study of the neutral upper atmosphere.

The basic limitations of the ground-based radio techniques are fourfold, as follows:

a. Radio waves are reflected by a fixed electron density, depending on their frequency. Accordingly, it is not possible to study the height region in the valley between two layers (such as the E and F) or above the F-peak by a reflection technique. The invention of the incoherent scatter sounding technique, while it requires complicated and expensive ground installations, has to some extent alleviated this problem.

b. The sensitivity of the radio soundings is not sufficient to measure the low electron densities (less than 10^4 cm^{-3}) that occur in the D-region, at altitudes below 90 km, and in parts of the E-region. Wave frequencies low enough to be appreciably affected have a wavelength so great that simple ray optics does not apply.

c. Radio waves are not well suited for study of the fine structure of the ionosphere, particularly in the vertical direction.

d. The distribution of ground stations is far from ideal for a study of the global morphology of the ionosphere.

Significant progress in the measurement of electron temperatures and energy distribution was first made during the past decade by means of rocket measurements. Investigations of the positive and negative ion densities, temperatures, and energy distributions in the ionosphere were initiated more recently and, except for the incoherent-scatter technique (an indirect measurement), have been carried out primarily by rockets.

The systematic rocket and satellite study of many ionospheric properties, such as ionic composition, motions, and collision frequencies for a great variety of neutral and charged-particle interactions, has begun in the past few years. The understanding of many special phenomena, such as aurora, the effects of energetic particles on the ionization and heating of the atmosphere, magnetic storms, and solar flare phenomena, is far from complete. Much remains to be done.

Among the phenomena of importance to the understanding of the ionosphere may be included:

Production and Loss of Ionization. Solar radiation is the principal source of ionization at all levels in the ionosphere, while charged particles are lost as a result of many types of interactions that are highly altitude dependent. On the day side of the ionosphere, for example, solar x-ray and ultraviolet fluxes are on the order of 1000 times greater than fluxes of all other sources of ionization. At night, ionization production by meteor bombardment may be significant in the height range 80 to 130 km. In the auroral zones, polar cap regions, and throughout the night hemisphere, proton and electron fluxes are important sources of ionization. In the D-region, attachment of electrons to neutral particles produces negative ions.

Below 80 km, among the principal ion-loss mechanisms may be included ion-ion recombination, photodetechment of electrons from negative ions, collisional detachment, and three-body attachment processes.

Dissociative recombination, that is, processes of the type

$$xy^+ + e \rightarrow x' + y' \ ,$$

is important in all regions where significant molecular ion concentrations exist. Radiative attachment of electrons to atomic particles becomes increasingly important with increasing height.

Throughout the ionosphere, a great number of ion-molecular reactions, such as charge exchange and ion-atom interchange, are important in modifying the electrical properties. In the F-region and above, where atomic species become predominant, radiative recombinations of the type

$$x^+ + e \rightarrow x + h\nu$$

must also be considered.

Uncertainties that now exist in the ion and neutral-particle composition and in the values for many of these rate coefficients are a serious barrier to the understanding of ionosphere phenomena below about 250 km.

Dynamical Processes. Spatial and temporal variations of the production and loss mechanisms produce continual changes in the dynamical state of the ionosphere that manifest themselves in mass and wave motions of the ionized constituents. The neutral wind drives the ion motion in the D- and lower E-regions through the strong collision coupling, while the geomagnetic field controls the ion motion in the upper E- and F-regions.

Neutral wind and wind shear effects produce local variations in the electron and ion densities that grow and decay under the influence of transport mechanisms (convection and diffusion) and of the production and loss mechanisms mentioned above. The transmission to the ionized motions of the striking periodicities of the neutral wind field is apparent in such phenomena as sporadic-E layer movements, traveling ionospheric disturbances, and daily and seasonal trends in the E-layer electron-density profiles. The inverse coupling from the ionized motion into the neutral motion appears to manifest itself in the hydromagnetic damping of the neutral wind profile in the upper E- and F-regions.

Thermal Structure. An understanding of the thermal structure of the ionosphere requires the study of the spatial and temporal behavior of ion- and electron-temperature distribution, the physical relations governing these quantities, and their relation to the neutral gas temperature. During the past few years, satellite measurements have made a major contribution to the determination of the spatial and time variations of ion and electron temperatures above a few hundred kilometers. Much less is known about charged-particle temperatures at lower altitudes, which can only be investigated by in situ rocket measurements. While the principal mechanisms important in the heating and cooling of the electron and ion gas are broadly understood, further data are needed on these atmospheric processes.

Airglow Emission. Optical emissions of NO, N_2, N_2^+, helium, atomic oxygen, and nitrogen originating from above 120 km are, in some cases, the result of excitation by ionospheric reactions. In others, such as N_2^+, the emissions are the result of resonance scattering of sunlight by pre-existing ions. Emissions of O_2, OH, atomic oxygen, sodium, potassium, and lithium from altitudes between 30 and 120 km, and emissions of Lyman-alpha and the resonance line of atomic oxygen extending to beyond 500 km, are not closely coupled with the ionized component but are the result of photochemical reactions and resonant and fluorescent scattering of sunlight. While the study and interpretation of both classes of emission is necessary for the proper understanding of our own atmosphere, such an understanding is also necessary for the proper interpretation of airglow observations of other planets, the nature of whose atmospheres is much less adequately known.

Interaction with Radio Waves. The protonospheric plasma is the seat of two types of naturally occurring radio signals: the whistling atmospheric or whistler, in which an electromagnetic impulse from a lightning flash is dispersed by the plasma into a continuous tone of varying frequency; and extremely low-frequency emission waves generated, for example, by amplification in a protonospheric field tube.

Special Phenomena. There are many naturally occurring phenomena, such as solar flares, magnetic storms, auroras, and proton events that dramatically alter the electrical properties of the upper atmosphere. The complete understanding of these phenomena often requires the simultaneous measurement of several parameters over the appropriate height and time intervals, preferably accompanied by monitoring of the Sun and near-space by means of satellites.

Kinds of Observations Required

The following types of observation are important for an understanding of ionospheric processes:

- Electron density
- Electron temperature or energy distribution
- Electron-collision frequency
- Positive- and negative-ion densities
- Ionic mobilities

- Positive-ion temperatures
- Positive- and negative-ion composition
- Flux of solar ionizing radiation
- Magnetic and electric fields
- Flux of optical emissions
- Tracing of dynamical processes
- Energetic-particle fluxes
- Cosmic radiation

In addition, it is generally necessary to study the following variations and correlations of these parameters:

- Diurnal, seasonal, sunspot cycle
- Geographic, geomagnetic
- Sporadic effects
- Transient effects
- Altitude variations
- Small-scale structure
- Correlation with other geophysical phenomena
- Correlation with ground-based measurements

Recent Progress in the Ionospheric Field

The ionospheric area is too broad and complex to be covered fully in a brief survey of this kind. However, some outstanding advances have been made in recent years, and are listed summarily below. This list is not intended to be exhaustive.

- Thermal structure
- Gravity waves and traveling disturbances
- Topside electron-density morphology
- Ion composition above the F-region peak (He^+, H^+)
- Day airglow (nitric oxide)
- Presunrise effects in lower ionosphere
- Persistent stratification of night E-layer
- Wind shear and sporadic-E
- Stratosphere-ionosphere relationships
- Conjugate-point effects
- D-region positive-ion composition
- Field-aligned enhancements of upper-ionospheric ionization
- Magnetospheric "knee"
- Negative-ion density in D-region
- Subprotonospheric whistlers
- Plasma line (incoherent scatter)
- Mid-latitude current system

Outstanding Physical Problems

Some of the problems of the greatest geophysical interest in the ionospheric area are listed below.

1. D-region
   Negative-ion to electron ratio
   Ion composition
   Detailed ionization profiles at special times, e.g., solar flares, pca events, solar eclipses, presunrise and postsunset effects
   Effects of neutral-composition changes
   Contribution to ionization by cosmic rays other than primaries
   Role of hard x rays in the normal D-region, particularly at high sunspot activity
   Stratosphere-ionosphere coupling, and the role of gravity waves
2. E-region
   Dynamical effects and neutral-ionized coupling
   Maintenance of night E-layer
   Postsunset decay-rate measurements
   Instabilities (electrojet and other)
   Thermal structure
   Ion composition (diurnal variation)
   Testing of dynamo theory of ionospheric motions
   Origin of airglow
   Formation of sporadic-E of all types
3. F-region
   Thermal structure
   Seasonal anomaly
   Maintenance of night F-layer
   Equatorial anomaly and other field-aligned enhancements of ionization
   Conjugate-point effects
   Airglow
   Effects of gravity waves
4. Outer ionosphere
   Thermal structure
   Ionization distribution
   Charged-particle fluxes and energy distributions (thermal and non-thermal) of electrons and protons
   Angular distribution of photoelectrons
   Hydromagnetic convection of ionization
   Geocoronal hydrogen distribution
   Electromagnetic-wave effects

Future Roles of Ground-Based, Rocket and Satellite Observation Systems

During the early development of space-vehicle experiments, a rocket was often flown with a single-experiment payload. In the past few years, with improved instrumentation techniques and the improvement of rocket and satellite performance, there has been a shift to multiple-experiment payloads. For further progress in understanding the physical processes of many ionospheric problems, the simultaneous measurement of all parameters relevant to the process being studied is essential. These correlated experiments are of three types:

(1) Measurement of several related parameters in the same space vehicle.

(2) Simultaneous measurements in the same region of the upper atmosphere by coordinated ground-based and space-vehicle experiments.

(3) Simultaneous measurements by more than one space vehicle: either using the same combination of experiments at two places or using two different space vehicles at the same place.

Each of these approaches has its particular merits, depending on the nature of the problem. For example, a significant advance in the clarification of many outstanding problems connected with the emissions originating below 350 km (i.e., everything except Lyman-alpha) could be made by the simultaneous use of rocketborne airglow photometers and of neutral and ion spectrometers and electron detectors, thus removing uncertainties about the variability of these parameters.

As further examples of the types of space experiments for which there is a strong scientific need, to aid in solving some of the problems related to a better understanding of neutral-atmosphere pressure, temperature, density, and composition in the 60-200 km altitude range, the following list has been prepared. It is not intended to be exhaustive, and represents perhaps one-third to one-half of the experiments urgently needed for each of the ionospheric regions.

1. D-region

a. Polar-cap absorption events: require rocket measurements of electron and ion density, proton and electron flux, and ionospheric absorption during a pca event.

b. Solar-flare photoionization: requires rocket measurement of electron and ion density, hard x-ray flux and ion composition at several times during flares of various magnitudes.

c. Solar-eclipse effects: require rocket measurement of electron density and temperature, positive-ion composition, soft x-ray flux, and ultraviolet flux in the Lyman-beta and Lyman continuum regions, preceding, during, and following a total solar eclipse.

2. E-region

d. Postsunset electron decay: requires rocket measurement of electron density and temperature and of positive-ion composition at a series of times following sunset.

e. Ionospheric electric currents: require rocket measurement of the magnetic field and of electron density and temperature, together with ground-based magnetic measurements.

f. Sporadic-E ionization: requires rocket measurement of electron density and temperature, ion composition, and neutral motions.

3. F-region

g. Conjugate point effects: require combined rocket and satellite measurement of electron density, electron temperature, and photoelectron energy spectrum at two geomagnetic-conjugate points when one is in a presunrise and the other in a postsunrise condition.

h. Oxygen-red-line dayglow: requires rocket measurement of electron density, electron temperature, oxygen-red-line flux, atomic-oxygen concentration.

Comparison of this list with the list of unsolved physical problems, above, shows clearly the magnitude of the over-all rocket effort that is called for to make a successful attack on these problems.

To these must be added a continued systematic study of the ionospheric variations, as outlined above in the section on kinds of observations required, to allow correlations with other geophysical phenomena.

Suggested Rocket and Satellite Program

Referring to the list of outstanding physical problems, above, it can be seen (i) that rockets provide the only suitable vehicles for the in situ study of the D- and E-region problems, (ii) that satellites have outstanding advantages for the study of the outer ionosphere, and (iii) that both rockets and satellites are appropriate for the study of F-region phenomena.

To provide a basis for estimating the required effort for rockets, the outstanding physical problems listed above are classified below in terms of general programs, and an approximate annual requirement for rockets is indicated for each.

Program	Rockets
1. D-Region	
Solar flares and PCA events	20
Solar eclipses	5
Diurnal variations	16
Ion and neutral composition	10
Ionization production	5
2. E-Region	
Electric currents	10
Sporadic-E	15
Diurnal variations	6
Airglow	10
Thermal and ionic structure	10
3. F-Region	
Diurnal, seasonal, and geographic variations	-
Conjugate-point effects	4
Thermal and ionic structure	10
Airglow	5
4. Outer Ionosphere	
Charged-particle fluxes and energy distribution	-
Whistler and ELF wave field	-
Electric fields and motions	2
Geocoronal airglow	-

The figures shown above point to an annual rate of approximately 115 rockets per year, building to about 130 per year during solar maximum.

This may be compared with the past annual effort in the ionospheric area, as shown in the table below:

Year	1959	1960	1961	1962	1963	1964
Payloads	18	17	21	45	36	36

It is seen, therefore, that the proposed and scientifically justifiable rocket program is approximately three times greater than that currently funded.

Because most satellites are now of the observatory type and carry experiments covering many disciplines, it has not proved feasible to project the number of satellites required to solve specific problems in the ionospheric area. However, the rate at which scientific results have been produced over the past years from satellites, as well as the planned program for the near future, indicate satisfactory support of satellite research at this time.

Manpower Supply

It must be asked whether there is a sufficient number of interested groups to sustain this magnitude of effort. The groups that have engaged (prior to the end of 1964) in rocketborne investigations in this area of research number about 17 (see Section 5 for details). In addition, at least three ionosphere groups are now in the process of developing programs but had not launched rockets prior to the end of 1964.

In view of the number of groups now actively engaged in rocket research, the increased level of effort indicated in the estimate given above does not appear to be unreasonable. It corresponds to an average of seven rockets per group per year, and a few groups are already at this level of effort. The estimate of the annual level of satellite activity, primarily for ionospheric-physics research, corresponds approximately to the level currently planned.

Specific Recommendations

(a) A comprehensive outline of the research program, past and future, on the ionized component of the atmosphere, including ionospheric physics and aeronomy, is given above in this section of the report. Interest in this field has increased, as has the number of research groups capable of conducting ionospheric research with rockets. The Committee thus makes the following recommendation:

Recommendation 14. That the number of rockets provided in support of research concerned with the ionized components of the atmosphere, including airglow observations, be increased from the present level of about 36 per year to over 100 per year in the next 5 years, reaching about 130 per year during the forthcoming period of solar maximum (about 1970).

(b) It is important to relate direct ionospheric measurements with rockets to ground-based measurements of related ionospheric parameters.

This can be accomplished typically by ionosondes (with low-frequency capability) and riometers, vertical-incidence-pulse absorption, and very low-frequency phase measurements. It is therefore further recommended that emphasis be placed on providing instrumentation for related ground-based measurements at all major sites from which ionospheric rockets are launched.

(c) In view of the complexity of the physical processes taking place in the D- and E-regions of the ionosphere, many of which are not now understood, it is recommended that increased emphasis be placed on the use of rockets for measurements of those ionospheric parameters critical to the understanding of the basic phenomena.

FIELDS, PARTICLES, AND PLASMAS IN THE MAGNETOSPHERE

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Introduction

This section discusses magnetic and electric fields and charged particles in the magnetosphere and the interactions of the magnetospheric phenomena with phenomena in the ionosphere and the neutral atmosphere. Studies in this subject area may be made by ground-based instruments and by instruments carried by balloons, rockets, or satellites. Particular attention to the rocket and satellite investigations is given here. The domains characterized by the many interesting phenomena under discussion are sketched in Figures 9 and 10. Many facets of these phenomena are not well understood and require continued and expanded experimental and theoretical study. The committee has singled out the following major outstanding problems:

a. The coupling of particles and magnetic fields in the solar wind with those in the magnetosphere is little understood, and yet it is precisely this coupling that leads to auroras, geomagnetic storms, Van Allen radiation, and a multitude of other phenomena. More-intensive coordinated rocket studies of auroras and satellite studies of the magnetosphere must examine particles and fields over the widest possible ranges of energies and of frequency spectra in order to understand these plasma phenomena that cannot be simulated in the laboratory.

b. The energy balance between the particles that cause auroras and the multitude of related phenomena is not understood. For example, less than 1 percent of the energy deposited in the atmosphere by the energetic particles is radiated as visible light. A few tens of 1 percent may be dissipated as heat, and other energy goes into ultraviolet light, sound, magnetic fields, radio emissions, and so on. A very comprehensive coordinated study of individual auroral events should be made with adequate instruments on the ground, in rockets, and in satellites.

c. The causes of the patterns of particle precipitation into the atmosphere and of the resulting auroral forms are not known. The statistical

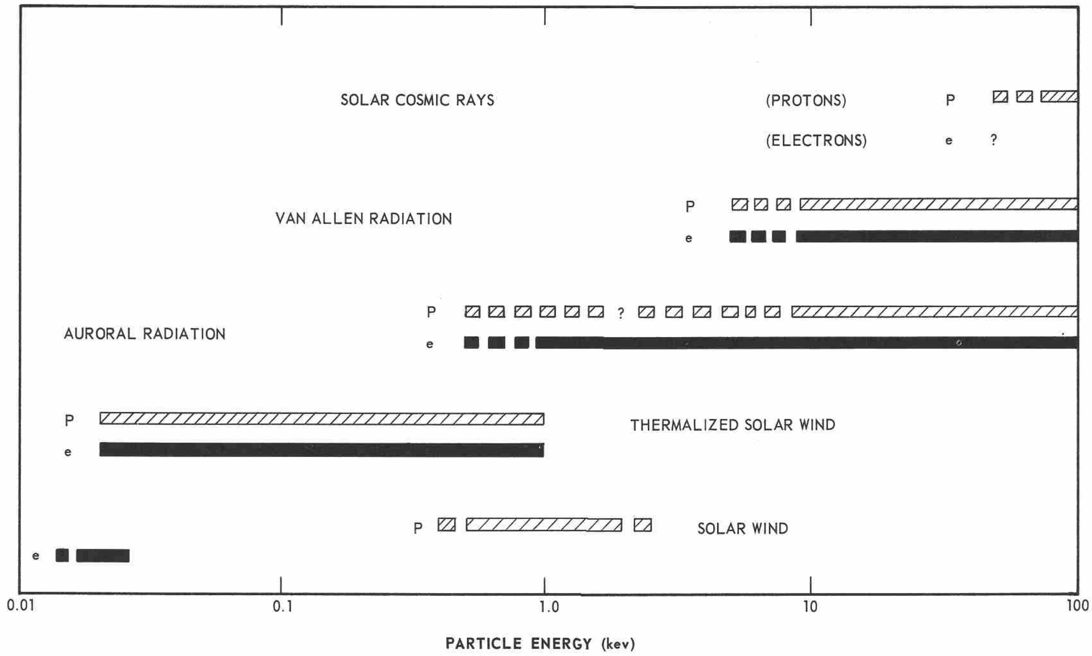

Figure 9. Representative energy ranges of interest.

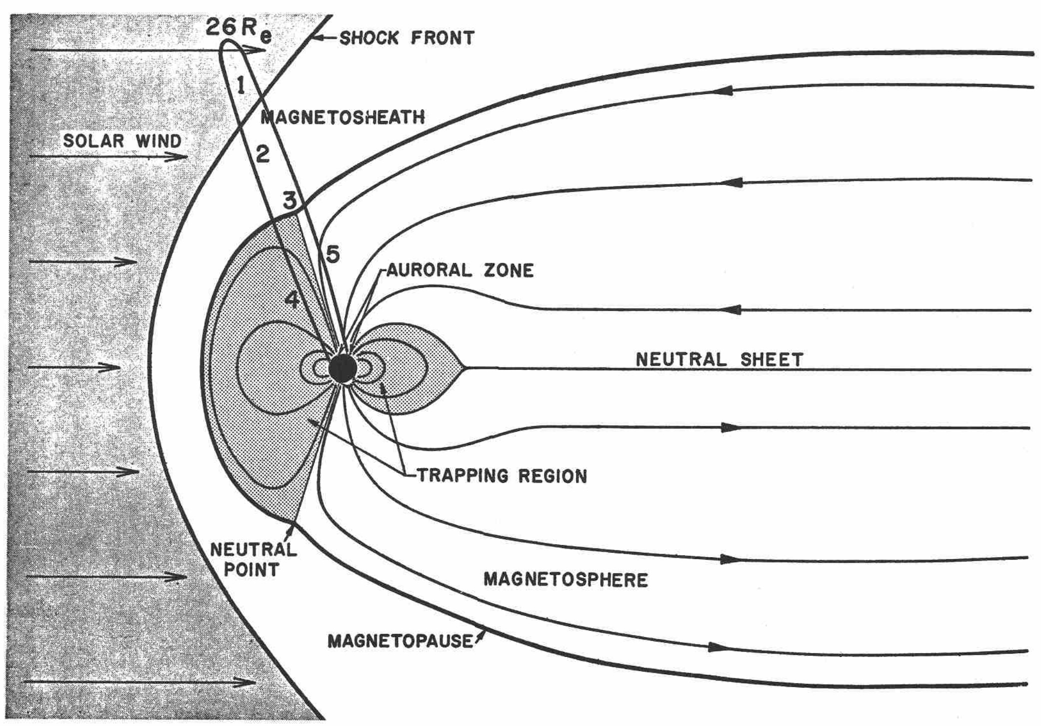

Figure 10. Diagram of the magnetosphere.

distribution of auroras in the auroral zones and over the polar caps is reasonably well known, largely as a result of optical ground observations made over the years. With the advent of rocket and satellite particle-measuring instrumentation, information now is accumulating about the distribution of particle precipitation within certain energy ranges. At present, there is a lack of understanding as to why auroral displays appear as collections of discrete auroral forms having a fine vertical columnar structure (called "rays"), and very little information is available regarding the exact connection between individual auroral forms and incoming particle streams. Coordinated ground-based, rocket, and satellite observations are required for the study of these problems. Improved instrumentation in the form of better detectors of low-energy particle fluxes and satellite instrumentation for optical determination of instantaneous auroral distributions are being developed for use in the immediate future.

d. The possible existence of electric fields in the ionosphere and the magnetosphere should be studied with rockets and satellites. At present, characteristics such as charged-particle fluxes and magnetic fields can be measured; the remaining important parameter is the electric-field vector, which must be measured with instrumentation now under development.

The following sections discuss the inner and outer magnetosphere and the auroral phenomena that apparently are generated near the boundary between these two regions of the magnetosphere. Attention is drawn to some examples of detailed experiments that should be carried out by rockets and satellites.

Auroral Phenomena

Historically, the aurora referred to structured visible phenomena in the atmosphere, but now the term is applied to the whole host of intricately interrelated phenomena resulting from the interaction of energetic particles with the Earth's atmosphere. These high-latitude phenomena take place primarily in the 60-200 km region, but are related to processes occurring in the whole of the region extending from the Earth's surface to the Sun.

At present, we have at least a crude knowledge of the general characteristics of auroral phenomena and their geographical extent. However, detailed descriptive knowledge is lacking. The mechanisms by which energetic particles interact with the atmosphere to produce the auroral phenomena are not well understood, nor is it known how energy is transferred from the solar wind and how particles are accelerated into the atmosphere.

A variety of observational techniques are currently used in the study of the aurora and related phenomena; the list is indicative of the complexity of the subject:

a. Optical Observations—visual, photographic (direct and indirect), photometric, spectroscopic. (Spectral region includes infrared, visible, ultraviolet and x ray). Ground, rocket, and satellite techniques are useful for all of these, except that the far ultraviolet, x rays, and regions of the infrared spectrum are not detectable on the ground.

b. Radio Observations—radar, radiowave propagation, and cosmic-noise absorption are best observed from the ground. Radio-frequency, audio, and low-frequency emissions from the aurora and auroral particles should be observed on the ground and with satellites.

c. Acoustic-Wave Observations—observed from the ground.

d. Field Observations—magnetic fields observed by all of these techniques: ground-based, rockets, and satellites. Electric fields should be measured from rockets and satellites.

e. Observations of Energetic Particles—type and spatial and temporal distribution of energetic particles, as well as their intensity, angular distribution, and energy distribution, should be observed with rockets, satellites, and, indirectly, by balloons.

f. Observations of Thermal Particles (neutrals, ions, electrons primarily in the atmosphere)—composition, density, temperature, and motions. Rockets and satellites.

g. Observations of Ionization—Ground, rockets, and satellites.

A number of outstanding problems exist in the field of auroral research; several examples are given in Table 7 together with the types and locations of measurements needed for their solution. Table 7 is incomplete, but clearly demonstrates an essential requirement of auroral research: namely, the need for coordinated observations both by a single vehicle and by coordinated ground, rocket, and satellite techniques. Certain of the observations must be made *in situ* in the 60-200 km region, accessible only to rocketborne instrumentation; others require vertical profiles at specific times and locations, again a capability limited to rocket vehicles. Observations of spatial and temporal variations can be best made, usually, by ground-based or satellite instrumentation, and most of the observations within the magnetosphere are best accomplished by satellites or, occasionally, by high-altitude rocket probes.

It is therefore recommended that combined experiments using properly instrumented rockets and satellites, plus ground-based observations, be designed to study auroral phenomena. The satellite orbits and rocket trajectories should be planned so as to give meaningful observations along the same magnetic-field lines. It would be desirable if the same group of scientists (not necessarily from one organization) planned and controlled such a combined experiment from its inception to completion.

Inner Magnetosphere—The Stable Trapping Region

The inner magnetosphere is a doughnut-shaped region populated by the stably trapped charged particles (Region 4 of Figure 10). Near the outer edge of this region, there are large fluctuations in magnetic field and changes in particle fluxes; at lower altitudes within this region, the main field is more rigid and fluctuations of fields and particle fluxes are relatively minor.

Source and Acceleration of Trapped Particles. The structure of the magnetic field and the average population of trapped particles within the magnetosphere are now reasonably well known. The source of the outer trapped

Table 7. Typical Specific Problem Areas in Auroral Phenomena

Problem Area	Location and Type of Observation		
	Ground	Rocket (60-200 km)	Satellite (200 km and up)
Energy balance	Magnetic field and optical emissions; sound waves; RF emissions	Total particle-energy input. Total ionization, atmospheric temperature, optical and radio emissions.	
Acceleration mechanisms for auroral primaries	Optical detection of auroras; magnetic measurements	Magnetic & electric field observations, & flux characteristics of precipitated particles in & above ionosphere	Magnetic and electric-field observations, and flux characteristics of particles in magnetosphere
Morphology & temporal variations of auroral electrojets & relation to auroral structures	Optical location of auroras; magnetic measurements	Magnetic field observations and detection of precipitating particles	Temporal variations of particle flux, and particle characteristics in magnetosphere
Exact relationships between visible auroral structures and particle streams	Optical studies of auroras	Location and characteristics of precipitated particles in upper atmosphere, and altitude profile of luminosity	Location and characteristics of particles in magnetosphere, & spectroscopy & photography looking down
Interaction of energetic particles with the atmosphere (excitation de-excitation processes)	Location & temporal variations by optical means; laboratory studies of cross sections, etc.	Optical and particle measurements	
Determination of characteristics of the auroral atmosphere	Optical determination of existence and location of auroras	Composition, density, temperature versus height profiles	
Relation of auroras to trapped or untrapped particles	Optical location and characteristics of visible auroras	Charactersitics of precipitated particles	Characteristics of trapped energetic particles in magnetosphere

particles and the reasons for the large changes in particle flux are unknown. It is unknown whether the changes in particle flux are due to introduction of fresh particles into the magnetosphere or to acceleration of existing ones. A connection between field fluctuations and particle changes is suspected but has not been demonstrated.

Investigation of the inner magnetosphere has been somewhat neglected in plans for future satellite missions. Elliptical orbits with apogee from 5 to 8 Earth radii, or circular orbits at altitudes from 3 to 7 Earth radii, are needed to examine the various regions of the inner magnetosphere. The satellite payloads should include instruments to make charged-particle measurements from thermal plasma energies to 10 to 100 MeV. Pitch-angle distribution and high time resolution are also needed, as are definitive measurements of types of particles, e.g., electrons, protons, and alpha particles. Magnetic-field measurements should extend from precise vector measurements of the static field through hydromagnetic waves to electromagnetic signals of several kilocycles. It has been proposed that electric fields may be an important factor in magnetosphere plasma motions and possibly in particle accelerations. Measurement techniques must be developed to investigate this relationship.

Magnetic Storms. One facet of the particle-flux changes is closely related to the occurrence of magnetic storms. The worldwide depression of the Earth's magnetic field has been attributed to a ring current of charged particles. It is now generally accepted that the motions of trapped particles produce this current, which may also be thought of as "inflation" of the geomagnetic field by charged particles. Inflation of the storm-time field by charged particles has been shown by magnetic-field observations to occur in the region between 2 and 5 Earth radii, but the responsible particles have not been observed. Presumably, they are of lower energies (less than 10 keV electrons, less than 100 keV protons) than particles observed by previous experiments. One proposed mechanism for the formation of a ring current involves introduction of neutral hydrogen from the Sun into the magnetosphere. Measurement of this constituent should be considered.

Ionospheric Currents. Although ionospheric electrical currents are properly in the study area of dynamics of the ionosphere, the high-latitude currents, and possibly also the low-latitude currents, appear to be driven by magnetospheric plasma motions. Hence, they will be considered here as part of the inner-magnetosphere studies, although there is overlap with auroral and with ionospheric studies.

During magnetically undisturbed periods, electric currents exist in the lower ionosphere. These currents produce magnetic variations at the Earth's surface that indicate that the ionospheric currents arise by dynamo action related to atmospheric motions caused by solar heating (Sq currents) and by lunar tides (L-currents). Under disturbed conditions, more-irregular magnetic variations are observed and these are presumed to result in part from ionospheric currents related directly or indirectly to plasma motions in the magnetosphere and to interaction of energetic particles with the atmosphere. These disturbance (DS) currents tend to be the more important component at higher latitudes while the Sq currents are the main current component at low latitudes.

On the basis of ground-based magnetic observations, the existence of ionospheric currents has been suspected for many decades. It was not until rockets and rocket instruments were developed that the existence of the ionospheric currents was proven. Even as late as 1963 it had not been experimentally confirmed that the currents responsible for middle-latitude Sq magnetic variations are in the ionosphere.

The detection of ionospheric current is indirect, depending upon measurement of the magnetic field of the current; it is necessary to make an in situ measurement, usually along a near-vertical profile through the current region (90-150 km). A difficulty in making this measurement is the relatively intense geomagnetic background field, which places the stringent requirement of very high accuracy on the rocket-trajectory determination and on the precision to which the geomagnetic field is known. Several basic problems concerning ionospheric currents persist and solution of these requires coordinated rocket measurements and ground measurements made simultaneously:

a. Sq- and Lunar-Current Generation Mechanism. Coordinated measurements are required of magnetic fields, neutral air motion, electron density, and, perhaps, electric fields, by rocketborne instruments. For the purpose of making these measurements, improvements in the methods of measuring air motions and electric fields are needed.

b. Morphology of Disturbance (DS) Currents and Their Relationships to Auroral Precipitation and Ring Currents. Coordinated rocket and ground measurements are needed to determine the locations and distributions of the DS currents and to delineate their exact spatial and temporal relationships to other phenomena. The mechanisms by which the disturbance currents are generated are most likely quite different from those causing the Sq- and lunar-currents.

Once the behavior and mechanisms of the disturbance currents have been determined by coordinated ground, rocket, and satellite techniques, routine observation of the magnetic variations, with inexpensive ground stations, hopefully will serve as a monitor of as yet unknown but important dynamical processes in the outer magnetosphere or in the interplanetary medium. The knowledge obtained by such observations may be useful for prediction of certain time-varying parameters of the Earth environment and of the interplanetary environment.

Outer Magnetosphere and Miscellaneous Phenomena

Interaction of the Solar Wind with the Magnetosphere—Shock Front, Magnetosheath, and Magnetopause. The wind of plasma from the Sun contains the energy that produces the magnetosphere phenomena. Studies of the magnetosphere depend on continued measurement of the solar wind and the interplanetary magnetic field. In the shock front and the magnetosheath occur important changes in the properties of the solar wind—decrease in proton streaming energy; increase in proton and, in particular, electron temperatures; possible generation of hydromagnetic waves; and irregularities and turbulence in the solar wind. All of these effects are important in understanding how the solar-wind particles and energy are introduced into the magnetosphere.

The magnetopause is the skin of the magnetosphere proper. Its shape and the variation in time of the boundary location have been delineated by past experiments. The nature of the boundary is still not well understood. According to early theories, the solar wind is excluded from the magnetosphere, although energy in the form of hydromagnetic waves could penetrate the magnetopause. The impenetrability of the boundary by the solar wind must be investigated particularly by measuring the high-energy tail of the electron distribution in the magnetosheath. Regions of low magnetic field in the magnetosphere tail, particularly in the neutral sheet, may allow direct entry of solar-wind particles.

Our present knowledge of the outer regions of the magnetosphere and of the interactions between the solar wind and these regions results from satellite and high-altitude-probe techniques; these methods of experimental investigation will continue to dominate in the coming years.

Solar Cosmic Rays. Occasionally during disturbed periods, the Sun emits a stream of charged particles that may range in energy from a few tens of keV to the BeV range. The higher-energy particles can be indirectly measured from the ground, and particles with energies of order of 100 MeV per nucleon can be detected with balloonborne instrumentation. But a complete study of the phenomenon requires rocket payloads (including recoverable emulsion packages) and satellites inside and outside the magnetosphere. The importance of comprehensive studies of these events—particularly in the forthcoming solar maximum—can be gauged from the facts that (i) the isotopic composition of solar cosmic rays provides useful information on the composition of the Sun; (ii) the particles can be used as interplanetary probes because their arrival times, etc., are controlled by interplanetary conditions; (iii) the particles provide an occasional radiation hazard for manned space flight; and (iv) the particles cause significant changes in the atmosphere, including polar-cap radio blackouts.

Recommendations on Present and Future Levels of Effort in Satellite and Rocket Studies

In the foregoing subsections an attempt has been made to list the phenomena of interest, to indicate briefly the present state of our knowledge concerning these phenomena, and to point out the major outstanding problems together with what appear to be the logical experimental methods of solution. Giving emphasis only to the immediate future (the present to 1971), we now direct our attention to the methods of obtaining the solutions to the problems outlined.

Satellite Level of Effort. Table 8 lists representative satellite missions directed to specific problems and demanding carefully coordinated experiments to achieve the most useful and timely results. Each experiment should be carried out by a small number of investigators who are prepared to work closely together in all phases of the coordinated experiment.

The inner-magnetosphere measurements can be made principally by satellites, although high-altitude probes will be useful for occasional specific tasks. The boundary phenomena will continue to be investigated

Table 8. Future Satellite Mission Requirements for Magnetosphere Studies

Objective	Orbit	Flights	Type
Boundary, shock front and transition region	Elliptical, 15 R_e Circular, 12 R_e	2 per year 1 per year (the boundary should be monitored continuously through the next solar maximum; the number of flights indicated above is estimated to achieve this)	IMP or Pioneer
Tail	Elliptical, 30-40 R_e (also useful for interplanetary studies if lifetime is at least 1 year)	2 per year continuous observations through 1970	IMP or Pioneer
Auroral	Elliptical, 6 - 8 R_e Inclination, 60° - 80°	1 per year	IMP, small Explorer, or Pioneer
Inner magnetosphere	Elliptical, 5 - 8 R_e Circular, 3 - 8 R_e	2 per year	Small Explorer

by missions already planned and approved, i.e., the EGO and IMP series of satellites. Plasma, magnetic fields, and the more-energetic charged particles are measured by these satellites. Better coverage is needed of particles of energies intermediate between the energies of plasmas and energetic particles. Magnetic-field measurements of higher time resolution are needed for magnetopause, magnetosheath, and shock-front studies.

Orbits needed for boundary and transition-region studies should not exceed 15 to 20 Earth radii at apogee. Circular orbits in the range 10 to 15 Earth radii are useful to obtain a rapid study in local time around the boundary region. More-eccentric elliptical orbits are indicated for interplanetary-medium and magnetosphere-tail investigations.

Investigation of boundary phenomena, at distances of 10-20 Earth radii, obviously require satellite experiments. Unguided space probes could reach these distances, but the information return from a single pass usually does not warrant the experiment preparation and launch effort.

Because of the necessarily long lead times involved in satellite studies, the national satellite program for the next few years is already relatively fixed. With the exceptions noted in the text immediately above, it appears that the planned level of effort for the forthcoming few years is roughly compatible with the need as outlined in Table 8.

Level of Rocket Effort. In attempting to assess the needed level of effort in rocket studies of fields and particles, the Committee has used the criterion of available manpower, as it is clear that this rather than scientific need is the limiting factor. Since 1959, the number of groups actively engaged in rocket research in this discipline has grown from one to 14 (8 university and 6 governmental). The present rate of rocket launchings for such studies is about 30 per year (see Table 9). Table 9 shows also that not all groups actually launch rockets each year. The relatively low number of rocket launchings in the few years after Explorer 1 (launched 1958) is attributed to the fact that "fields and particles" became the most active discipline in satellite research during those years. In addition to the currently active groups, about five more are likely to enter rocket research in the next few years and it is estimated that perhaps six more (as yet unknown) may enter the field by 1970. On the average, each group can be expected to use from 2 to 3 rockets each year, assuming that at least some of the groups will also be active in other fields (e.g., satellite research). Therefore, we can reasonably expect the requirement for rocket vehicles to rise from the present level of 30 per year to 50-75 vehicles per year over a 5-year span. An estimate of the numbers and types of rockets likely to be used in specific areas within this discipline is given in Table 10. It is anticipated that there will be a tendency towards more sophisticated and more expensive payloads owing to the need for coordinated observations of related phenomena.

Table 9. Fields and Particles Rocket Groups and Flights, 1959-1971

	Past							Estimated for Future					
	1959	1960	1961	1962	1963	1964	1965	1966	1967	1968	1969	1970	1971
Rocket flights	7	28	7 solar protons	5	8	34	40 mobile launcher	20	50	60	75 solar maximum	75	65
Groups launching each year	1	5	3	2	5	8	7	9	10	12	15	15	12
Total number of active groups			14							19		25	
university			8							10		15	
governmental			6							6		7	
industrial			0							3		3	

Table 10. Proposed Rocket Flights, 1965-1971

Objective	Peak Altitude (km)	Number	Type
Solar cosmic rays	200	20 per year in 1969 and 1970 solar maximum	Nike-Apache
Auroral studies	200	15 per year by 1967 increasing to 30 per year by 1970 solar maximum	Nike-Apache, Nike-Tomahawk (Aerobee for stabilized payload)
	7000	3 per year by 1967 to 6 per year by 1971	Javelin
Inner magnetosphere	1000	1 per year	Javelin
	300	20 per year in 1967 for South American anomaly expedition	Nike-Tomahawk
Ionospheric currents	200	5 per year in 1967 to 15 per year in 1971	Nike-Apache

Because of the importance of obtaining improved understanding of the many complex aspects of solar-terrestrial relationships with regard to fields and particles, and because of the growth in interest described above, it is recommended that adequate support be provided for a doubling of the number of rocket launchings devoted to these studies over the next 5 years. (See General Recommendation 14, p. 11.)

ASTRONOMY

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Introduction

Astronomical research requires mainly that instruments be lofted beyond the absorbing, radiating, or perturbing portion of the terrestrial atmosphere, to altitudes determined by the wavelength of the electromagnetic spectrum observed in a particular experiment.

Astronomical satellites, of course, provide this capability very well, but at the same time they are very costly, have long lead times, and provide only limited opportunities for participation, particularly the advanced systems now being planned.

Rockets offer a very useful, less costly, more timely supplement to satellite astronomy, as discussed in detail in the following sections. Much of this material was prepared by the several working groups of the Summer Study. Material bearing directly on the question of rockets versus satellites for astronomical research has been taken from their reports and included here. The working groups also prepared some material especially for this rocket-and-satellite study.

The Role of Rockets and Satellites in Solar Astronomy

1. Introduction

The role of sounding rockets in solar physics was clearly stated in the report of the 1962 Space Science Summer Study at Iowa. If anything, the case for sounding rockets presented there has become even stronger with the passage of three years, yet the earlier recommendations have not yet been fully implemented. In 1962, the role of the sounding rocket was considered in relation to the Orbiting Solar Observatory (OSO) and Advanced Orbiting Solar Observatory (AOSO) satellite programs, and the conclusion was reached that orbiting observatories had not caused sounding rockets to become outmoded, and were not likely to in the foreseeable future. Now, after further study at Woods Hole, this conclusion has been reaffirmed both by the Solar Working Group and by this Committee. We have concluded further that the advent of manned orbiting telescopes requires further growth in both the size and quality of the rocket program.

There are a number of strong reasons why sounding rockets are clearly preferable to satellites for studies in certain well-defined areas of solar observation. First, the sounding rockets make it possible to investigate many solar phenomena at a moderate cost; the precious observing time of large, limited, and costly orbiting facilities should not be used to obtain data that can just as readily be secured with relatively cheap sounding rockets. Second, sounding rockets can be employed for observations that can be accomplished in a few minutes and that do not require extensive monitoring. Third, recovery of data and instrumentation from sounding rockets is relatively easy although not yet fully reliable. Recovery makes it possible to examine the results, to correct the instrumentation for a repetition, if necessary, or to modify and fly the equipment again for other purposes, with a great saving in cost. Most important, recovery permits the use of photographic methods that are very difficult from orbiting spacecraft. Fourth, a large, complicated effort such as that involving a Manned Orbiting Telescope (MOT), and AOSO, or even the OSO requires extensive preliminary study and testing to ensure effectiveness of the flight hardware. Intelligent planning of sophisticated instruments that can take full advantage of the capabilities of AOSO and MOT must be based on preliminary rocket experiments as well as on ground observations, laboratory studies, and experiments with smaller satellites. Therefore, if the space program is allowed to increase in complexity without full support of the more straightforward techniques, we may find ourselves unable adequately to plan the complex ventures. Fifth, rockets can play an important role in the periodic checking and recalibration of many long-lived orbiting experiments. Sixth, the sounding rocket program provides a much easier entree into the space program for young and inexperienced scientists than does the highly complex orbiting-observatories program.

The last point is a very important one if new experimenters are to be attracted into the solar-physics program in sufficient numbers to take advantage of the opportunities now clearly visible. The NASA space flight program in solar physics must be viewed as one element in a broader national program of research into the physical nature of the Sun. Other major research activities in which most of the country's solar physicists are now engaged are ground-based observations, laboratory experiments, and theoretical investigations. If more solar physicists are to be attracted into the NASA program as experimenters, they must be given an opportunity to do solar physics in space on a reasonably short time scale. Many of them are unwilling to devote a major fraction of their research time to projects requiring a lead time of several years. Eventually, when a large permanent solar observatory has been erected in space, the problems associated with long lead time may be solved, because observational data can then be called forth by radio command. But in the interim decade or more, the solar physics program must continue to provide opportunities for experiments with rockets as well as with small satellites.

2. Solar Problems—Observation with No Spatial Resolution

(a) The Extreme Ultraviolet Spectrum. Although the solar spectrum has been recorded with few gaps from 3000 Å all the way to about 13 Å in soft x rays, much work remains to be done. Less than half of the lines have been identified, and there are many strong lines of unknown origin. This is particularly true for the spectral region from 2000 Å to shorter wavelengths. Increased resolving power is required in order to obtain more-precise wavelengths and to separate the many blends. This work must be carried out by photographic spectroscopy and with rockets that permit recovery. For wavelengths from 500 Å to soft x rays, photographic spectroscopy is conducted with grazing-incidence spectrographs. There is still a great deal that can be done with these small instruments, which are flown with the currently used biaxial pointing control (BPC) system, without third-axis control. Soon, however, it will be necessary to fly much larger grazing-incidence spectrographs to study special regions at very high resolution. This will require three-axis stabilized rockets, in order to provide sufficient room for the equipment. For use farther down in the x-ray region of the spectrum, the Bragg crystal spectrometer has been shown to be extremely valuable. This can be flown with the BPC, but cross-spin stabilization, to produce triaxial stabilization control, is highly desirable; at these short wavelengths most of the emission comes from the solar plage regions, and rotation of the solar image, if not eliminated, complicates the interpretation of the results. The region 500 to 3000 Å is one where the potential capability of the BPC is nearly exhausted. Here, the triaxially stabilized Aerobee is greatly needed in order to provide the space required for large normal-incidence spectrographs, which will provide resolving powers approaching those attained with 21-ft grating spectrographs on the ground. Eventually, it will be necessary to use a larger rocket, such as the Aerobee-350, in order to obtain sufficient space for very large spectrographs. Triaxial stabilization of the vehicle, and recovery will, of course, be needed.

(b) Measurement of Intensities in the Spectrum. The precise measurement of intensities is extremely difficult, largely because methods of satisfactory calibration have not been worked out for the extreme ultraviolet. Photoelectric recording is somewhat more precise than photographic recording. Generally, the intensity data become less precise the farther one proceeds below 2000 Å. Much work on intensities can still be done with the BPC system. The "improved" Aerobee-150 is greatly needed here in order to reach altitudes of 250-300 km, where there is complete absence of atmospheric attenuation at all wavelengths. The BPC is really too small for this type of work, however, since calibration sources and more-sophisticated instrumentation should be flown in order to attain high precision. Furthermore, use of the BPC involves a limitation on the zenith angle of the Sun that is undersirable. This limitation can be removed by using the triaxially stabilized rocket, which can be pointed at the Sun regardless of the zenith angle. Pointing accuracy to 1 min of arc is sufficient for this purpose. Recovery, however, is essential in order to check the calibration of the system after flight.

(c) Studies of Atmospheric Composition Through Measurements of the Attenuation of Solar Radiation. Measurement of the attenuation of solar radiation is one of the most powerful methods of determining the altitude distribution of the atmospheric gases. In order to perform this type of measurement, a triaxially stabilized, "improved" Aerobee-150 is required, to provide sufficient space for the instrument. Furthermore, this vehicle permits making measurements at any zenith angle from 0° to 90°, whereas the BPC is limited in this respect. The triaxially stabilized system is also needed because rotation of the solar image carries with it a possibility of error in intensity measurement caused by changes of illumination of the grating system. Pointing accuracy to 1 min of arc is sufficient. Photoelectric recording is needed rather than photographic in order to attain maximum precision.

(d) Monitoring of the Solar Spectrum. It is of interest both to solar physics, as well as to aeronomy and ionosphere research, to monitor the Sun in many wavelengths as continuously as possible, and over an entire solar cycle. It is in monitoring, of course, that satellites are most useful; rockets are needed only for occasional checking of the satellite experiments. A large part of this work can be done with small satellites such as the solar-radiation-monitoring satellites of the Naval Research Laboratory. Although this type of vehicle is expected to be improved so as to provide more-nearly continuous monitoring, the number of wavelengths that can be monitored will still be limited, as will the accuracy and quantity of the data that can be stored and transmitted. OSO is potentially able to provide more-complete solar monitoring, especially if placed in a fully sunlit, retrograde polar orbit. For monitoring purposes, a less costly and more-quickly prepared version of OSO, launched with a Scout vehicle, should be considered.

3. Solar Studies of Special Regions

(a) Measurements of the Spectrum from the Center of the Disk to the Limb. It is important to measure changes in the spectrum from the center of the disk to the limb, and beyond the limb. From this type of observation, much can be learned about the solar model. Rather high spectral resolution is needed in the wavelength range below 2000 Å in order to separate out regions of the spectrum that are reasonably uniform—for example, the continuum between emission lines—or to study particular emission lines. Accurate pointing, of the order of 10 sec of arc or better, is required as the change in spectrum within 10 sec of the limb is important. Triaxial stabilization is also required as it is usually necessary to avoid active regions and to observe the Sun's limb at a single point during the course of the observations. For this purpose, the proper vehicle is the triaxially stabilized Aerobee-150 with 5-10 sec of arc pointing accuracy. It is planned to make measurements of this type from OSO and AOSO. However, there is great value to the use of rockets for this kind of research, especially in the exploratory stage as it is then comparatively easy to change the spectral region or the spectral resolution, or other parameters, from flight to flight. The lack of greater pointing accuracy is a limitation at the present time to the use of rockets for this purpose.

(b) Active Regions, such as Plages, Prominences, Coronal Condensations. It is of great interest to observe the spectrum of the various types of active regions from 2000 Å as far into soft-x-ray region of the spectrum as possible. At present, this type of work requires a normal-incidence spectrograph, and is limited to wavelengths longer than approximately 170 Å. Obviously, accurate pointing is required, and it is necessary also to provide offset pointing in order to place the specific region on the entrance aperture of the spectrograph. This could be done with a triaxially stabilized rocket equipped with offset pointing. A system with 1-min guiding accuracy would make it possible to obtain plage spectra. For prominence and condensation spectra, the spatial resolution and guiding requirements are far more severe. It appears that triaxially stabilized rockets will be of great use for exploratory research on such phenomena. However, orbiting-type vehicles will be required in order to conduct the long series of observations needed to study the many different phenomena. Some observations can be made from an OSO-type spectrograph with command-offset pointing. Others, however, require the higher pointing and guiding accuracy of the AOSO spacecraft, together with the larger instrument space.

(c) Flares. Study of the extreme-ultraviolet spectrum of a flare and of the change in spectrum with time are greatly needed to help understand the flare phenomenon. This type of research cannot be conducted easily from rockets. OSO and AOSO are required for extensive studies of flares.

4. Study of Line Profiles

The study of the profiles of spectral lines requires spectrographs of very high resolving power. Furthermore, these measurements should also be made with high spatial resolving power in order to observe the change in line profile from point to point on the Sun. This work requires photographic recording; hence it must be accomplished with rockets. Only the strongest spectrum lines, or lines that are well isolated, can be studied with spectrographs sufficiently small to fit into the BPC system. Generally, therefore, profile work must be carried on with a triaxially stabilized Aerobee-150. Pointing accuracy to 1 min of arc is sufficient for exploratory research. However, in order to record line profiles with the great spatial resolution necessary to study the detailed chromospheric structure, the Aerobee-150 and 5 sec of arc pointing is required. Eventually, even higher spatial resolution will be needed.

5. Spectroheliograms

Exploratory spectroheliograms of the Sun have now been made in a number of different wavelengths—first, at the Lyman-alpha line of hydrogen, and more recently, with small objective-type grating spectrographs equipped with aluminum filters, at He II, 304 Å; Fe XV, 284 Å; and Fe XVI, 335-361 Å. In the soft-x-ray range, spectroheliograms obtained using the pinhole camera technique, with the wavelength band selected by using a filter have shown that these emissions come from the active regions on the Sun, with little or none coming from the disk. The first such pictures were made using the BPC. Sharp images resulted only when exposures were possible that were short enough to stop the image from rotating. More recently, however, stop-gap triaxial systems have been used to permit longer exposures without image rotation. The first such pictures were made by British scientists using the Skylark rocket stabilized with the attitude-control unit made by Elliott Brothers. These have resulted in excellent x-ray images. More recently, images in the K line of Mg II, 2800 Å, and also x-ray images, were obtained by the Goddard Space Flight Center (GSFC), using the combination of the BPC and the ACS. Most recently, x-ray images of the Sun with better than 1-min of arc resolution were obtained by the Kitt Peak National Observatory with the combination of BPC and cross-spin stabilization. Further experiments of this sort would be of great value and will probably be carried out with the combination BPC and cross-spin stabilization. However, this limits the size of the instrument and usually provides no better than 1-min of arc pointing accuracy. For obtaining satisfactory spectroheliograms the triaxially stabilized Aerobee with 5 sec of arc or better pointing accuracy is greatly needed. This will permit flying much larger spectroheliographs, of focal length sufficient to produce images of the Sun that can be reduced to precise intensity values, and having the resolving power necessary to show the solar network. Spectroheliograms of this sort are important because they make it possible to map the Sun at various different levels in the chromosphere and the corona, by proper selection of extreme ultraviolet and x-ray emission lines.

Observations of this type are being planned for OSO, which provides 1-min of arc resolution, and for AOSO, with 5 sec of arc resolution. The purpose here, however, is to obtain spectroheliograms frequently enough to monitor the changes taking place on the Sun from day to day and to pick up transient effects such as flares. Rockets can be used to perform the exploratory research necessary to determine which wavelengths are the most interesting to study and to provide some experience for the work to be carried out with satellites, before the instrument is completely designed and in orbit, as well as to perfect the instrumentation for the satellite experiments.

6. The White-Light Corona

The white-light corona cannot be seen from the Earth's surface, except at the time of a total eclipse. Balloon altitudes are sufficient for observing the inner portion of the corona but not the outer corona, where the brightness is low and the light is principally zodiacal in origin. It is here, however, where the streamers are located, and it is this region through which transient effects associated with flares and other solar activity are expected to be detected. Therefore, it is necessary to monitor the white-light corona from much higher than balloon altitudes. For this purpose, orbiting vehicles are required. A white-light coronagraph operated successfully aboard OSO-II. However, the space available in OSO is barely sufficient for an instrument of this type, and limits the observations to distances from the Sun's center greater than about three solar radii. The experiment is best adapted, therefore, for AOSO and is indeed being so planned.

It is important, however, to fly coronagraphs in rockets for preliminary research leading to the perfection of the designs for orbiting coronagraphs. The first such coronagraph was flown by NRL with a BPC mounted on an Aerobee-150, on June 28, 1963. The size of this instrument was about the same as that of the instrument flown aboard OSO-II. The results of this flight, however, were valuable to the work of perfecting the design of the instrument for OSO-II. Many further improvements are envisioned, however. For testing them, the triaxially stabilized Aerobee is required, with pointing accuracy of the order of 15 sec of arc. Another use of the rocket-borne white-light coronagraph will be in connection with eclipses of the Sun. One project has already been tried whose aim was to photograph, using rocketborne equipment, the solar corona with the Moon in the field of view, blocking out part of the corona so as to permit discrimination between the portion of the zodiacal light located between the Earth and the Moon and that portion beyond the Moon. This rocket could not be flown, however, because a violent windstorm.

7. Rockets for Solar Research

Although small rockets such as the Nike-Cajun have found many uses in space research, they have little application nowadays to research concerned with the Sun. Practically all of the results until now have been obtained with the Aerobee-150 launched from White Sands. Here, recovery of photographic films and instruments is possible; this is essential in solar research.

The Aerobee-150 will certainly continue to be extremely useful in solar research for a number of years. No substitute for this rocket is needed or desired. There are, however, various improvements that should be made in the Aerobee-150. Its reliability record is rather good but is still not as should be possible. Recovery systems for use with the Aerobee-150 are far from perfect, and their failure record has been rather high. There is hope that this situation will improve in the future. The principal improvement in updating the Aerobee, however, is to strengthen the vehicle and to equip it with a new booster so as to increase its peak altitude and reduce the dispersion. At present, as used in solar research, the peak altitude obtainable is about 200 to 220 km. This is a little short of what is desired, since at various wavelengths there is still significant attenuation by the atmosphere above 220 km. The Aerobee can be modified at relatively small cost so as to obtain an altitude of 275 km carrying reasonably large optical payloads. A new fin structure is also needed. The White Sands range now realizes that improvements can be made in impact prediction and is taking steps to put them into effect. This should reduce the chance of early cut-down of a rocket by range safety personnel.

The four-fin version known as the Aerobee-150-A is launched from Wallops Island, where recovery has not yet been found to be practical. For this reason, and because of the dampness associated with the nearby salt water, White Sands is the preferred launch range for most Aerobee rockets used for astronomical research, at least until an air recovery system is employed.

There is an eventual need in solar research for a vehicle that will carry a greater payload and provide greater space for instrumentation than the Aerobee-150 and that will also provide more observing time by attaining a higher altitude. The Aerobee-350 may be satisfactory for this purpose, provided that it can be flown on the White Sands missile range. Assurance of recovery is highly important, however, if it is to be accepted by the research community.

8. Pointing Controls

Almost all kinds of solar research at present require some device to keep the instrument pointed at the Sun during the observations. The simplest method for doing this might appear to be to stabilize the rocket itself on three axes, pointing the long axis precisely at the Sun. This is the so-called solar-pointed, or triaxial, stabilized rocket that is so greatly needed for solar research. The development of an attitude control system (ACS) for providing 2° to 4° pointing in yaw and pitch, and commensurate stabilization for rotation around the solar vector, appears now to be nearly completed. It is, however, of little use for solar research, since, by itself, it fails to provide sufficiently accurate pointing by two orders of magnitude, or more. More-sophisticated systems based on the ACS are under development, but it is questionable whether they will ever prove to be completely satisfactory for solar use as they are designed fundamentally for use with stars and are more complicated, about twice as expensive, and also probably less reliable than they need to be for use with the Sun.

The biaxial pointing control (BPC) is the device with which most of the results of the last ten or more years on the extreme ultraviolet solar spectrum have been obtained. It exists in several forms and is constructed by the Ball Brothers Research Corporation and by the University of Colorado. However, the instrument space and weight-carrying capacity that it provides are severely limited. It is difficult to fly in this rocket instruments more than 36 in. long, at the very most, and perhaps 7 x 11 in. in cross-section. The pointing accuracy that it attains is approximately 1 min of arc in yaw and pitch. There is no stabilization in roll around the solar vector. Usually, the instrument rolls slowly, the amount and rate depending on the particular precession cone assumed by the rocket after burnout. The BPC is used with the Aerobee-150. Generally, it is recovered and can be used again.

Several stop-gap measures have been undertaken to provide stabilization for the third axis for the BPC, which may fill part of the need for stabilization until the triaxially stabilized rocket becomes available. One of these measures is to combine on a single rocket the BPC and the ACS systems. With the latter, the rocket is pointed in some convenient direction and the precession cone is eliminated. Under these conditions, the instruments in the biaxial pointing control will not rotate about the solar vector. This provides, in effect, three-axis stabilization. Most recently, another system has been used that appears to accomplish the same results, but with less complication and expense. This is the cross-spin stabilization system developed by the Kitt Peak National Observatory and the Hughes Aircraft Corporation. Using gas jets activated by gyros, impulses are provided at the correct times to close the yaw cone down and eliminate the precession nearly completely. The first flight of this system showed that it operated very well. Nevertheless, these systems are purely interim measures, since they do not result in an increase of instrument space, payload capacity or reliability. Indeed, the peak altitude attained will be reduced because of the extra weight.

The triaxially stabilized Aerobee rocket has been the subject of much developmental work, but this work has been directed mainly toward its application for stellar research. A triaxially stabilized Aerobee designed optimally for use with the Sun is greatly needed. There will be applications for which the pointing accuracy must be no greater than 2 min of arc in yaw and pitch, with comparable accuracy in roll. There are, however, applications for which the utmost in pointing accuracy is required; 5 sec of arc absolute accuracy in yaw and pitch has been requested, together with a comparable accuracy in roll in order to match the specifications of the AOSO system. At present, no such system is really in sight, but several promising avenues should be investigated. One is the use of the Elliott Brothers stabilization system designed for the United Kingdom Skylark rocket. A second is the inertial-wheel system proposed by the Ball Brothers Research Corporation. A third may be the Stellar Tracking and Pointing (STRAP) system and the Fine Attitude Control System (FACS) being developed by GSFC, but these are primarily designed for stellar work, and are based on the ACS. It is hoped that the eventual triaxial systems will be adaptable to the Aerobee-350, if it turns out to be operational at White Sands.

9. Satellites

The first solar-monitoring satellite was the small Solrad satellite employed since 1960 by NRL for solar monitoring. This satellite is being improved by adding a system to control the spin axis so as to keep the Sun on the vehicle equator. A simple memory system is being incorporated; the real-time telemetry system is limited in capacity but provides useful opportunity for international participation. The second vehicle is OSO. This is apparently an extremely well-engineered system; the first two have performed almost perfectly. Improvements are possible, however, and should be made in order to make this vehicle even more useful. Some of the improvements that have been requested are increased pointing accuracy, which may reach a value of 15 sec of arc rather than the present value of 1 min of arc, longer life, increased power, somewhat increased instrument space, a more-flexible format for the telemetry system, and greater telemetry capability. A retrograde polar orbit has also been proposed in order to provide continuous sunlight. Another possibility is an Earth-synchronous orbit with continuous real-time, high-capacity telemetry. Still another, and less costly improvement, is a stripped-down version designed primarily for monitoring, and launched by a Scout. Following this, there is to be the AOSO, which will take much larger systems than OSO. Here, the pointing accuracy specification is 5 sec of arc. Much more power and telemetry will be available than with OSO. This vehicle is expected to satisfy all of the research needs in solar physics research, of the type that can be conducted from unmanned satellites.

10. Numbers of Rockets and Manpower for Solar Research

The present annual use of Aerobee-150 rockets for solar research is, very approximately, as follows:

GSFC	2	U. K.	3
AFCRL	6	France	2
NRL	8		
U. of Colorado	2		
Harvard	1		
Kitt Peak	1		
	20		

The best estimate of growth, made by the Solar Astronomy Working Group of the Woods Hole study, is that these numbers will more than double within the next 5 years. Factors causing the increase are discussed in the introduction: increased reliability may tend to decrease, slightly, the number of rockets used per year. New groups, however, are expected to enter the field—for example, the University of Hawaii, Lockheed, the Western State College of Colorado, and Pomona; this will increase the number.

The total budget will rise sharply, owing to the ever-increasing complexity and sophistication of experiments and vehicles. Most of the simple, inexpensive experiments have been done. A reasonable estimate is that the cost of vehicles and instrumentation will rise gradually to an amount per rocket 2 to 4 times greater than now, 5 years from now. The factor 2 is based on excellent recovery of equipment. This is entirely justifiable, however, solely on the basis of the ground work that will be done by rockets, preparatory to the far more expensive vehicles, OSO, AOSO, and MOT. In addition, a great deal of valuable solar results will be obtained that cannot easily be secured from satellites.

Available manpower is very limited at present, but is bound to increase, gradually, as more space scientists are trained by the university research efforts.

The estimate of satellites for solar research is:

Solrad—(NRL) 2 per year
OSO —1 every 9 months, at least through 1972
AOSO —1 per year

There will also be some solar experiments, conducted by astronauts, in Apollo and Apollo Extension.

Note: Each OSO may be expected to require prior use of four triaxially stabilized Aerobee-150 rockets by experimenters who are preparing flight equipment, whereas each AOSO should require 8 or 10.

Role of Rockets and Satellites in Optical (Nonsolar) Astronomy

In agreement with the Working Group on Solar Astronomy and with the Optical Astronomy Panel, of the Woods Hole Summer Study, this Committee commends NASA for its foresight and imagination in developing the large and sophistocated spacecraft that are needed to support big optical instruments in orbit. Only by means of systems like OAO, AES, and MOT can stellar and galactic astronomy achieve the high spatial and spectral resolution required for the solution of fundamental problems. At the same time, however, there are also many problems that can be attacked with observations of lesser power and lower cost. It is unwise to use the precious observing time of a very large instrument on programs that can be carried out just as effectively with smaller and cheaper equipment. Although this principle has long been followed in ground-based astronomy, where small telescopes continue to be usefully employed despite the existence of telescopes in the 100-to 200-in. class, it does not always seem to have been taken into account in NASA program planning.

Virtually all of the optical astronomy results on nonsolar-system objects (stars and nebulae) achieved by the U.S. space program to date have been obtained from rocket flights. Although, hopefully, orbiting spacecraft will begin to play a dominant role in the near future, rockets will continue to perform an important function for specific research problems that do not require large telescope capabilities, and for definition of new areas worthy of intensive investigation with orbiting observatories. Rockets are of particular value in obtaining ultraviolet and infrared observations of the following types: (i) exploratory sky mapping in new spectral regions; (ii) filter photometry; (iii) spectroscopy and spectrophotometry; and (iv) wide-field, low-resolution ultraviolet photography of extended sources and star fields.

A significant beginning already has been made in these areas. Since 1955, when ultraviolet radiation from early type stars was first detected, exploratory sky surveys have been carried out, and broad-band photometry, scanning spectrometry, and photographic spectroscopic observations have been performed. One impact of this work has been the revision of model atmospheres for early type stars. An improved temperature scale for these stars, and information about the ultraviolet interstellar scattering law is now available. Thus far, no observations have been made in the infrared. It is anticipated that in the near future the optical-astronomy rocket program will provide photographic spectra of numerous bright stars, improved spectroscopic resolution, increased photometric accuracy, and wide-angle direct ultraviolet photography; that efforts will be made to detect interstellar molecular hydrogen; and that tests will be made of the optical depth of the interstellar medium in the Lyman continuum. In addition, results from the first far-infrared sky surveys should soon be available, and this program will ultimately expand to include far-infrared spectrometry.

Sounding rockets also have a significant role to play in planetary research. Although the Mariner direct fly-by of Mars has provided spectacular new understanding of this planet, rocket experiments have also contributed useful knowledge. Ultraviolet spectral information obtained by relatively simple rocket experiments, however, has set an upper limit on the ozone content of the planet's atmosphere. From this, an upper limit of the oxygen content can be inferred, providing vital information pertaining to the question of Martian life. Rocket planetary investigations should continue to play an important role in preliminary exploratory observations before the instrumentation for complex deep-space probing is fully committed, with respect to sensitivity, spectral regions of interest, etc.

In the broader national program of research in optical astronomy, ground telescopes, balloons, rockets, and satellites all have important and complementary roles. The relative importance of these programs may be expected to change, so that the emphasis placed on each should be reviewed from time to time. This Committee concurs with the Optical Astronomy Panel and the Working Group on X-Ray and Gamma-Ray Astronomy concerning the current role of rockets in astronomical research (see the discussion of the role of rockets, on page 31), and wishes to reemphasize two unique advantages that rockets have for optical astronomy:

(i) At present, rocket flights provide the only means of recovering on-board-recorded data on a regular and reliable basis. This situation is likely to persist until the AES is made available for astronomical research. The use of photographic film can greatly expand the versatility of ultraviolet observations, and major portions of the optical-astronomy program will depend on the use of photography.

(ii) Rockets afford the best means of making absolute spectral-energy measurements on selected stars, to be used as photometric standards for calibration of orbiting telescopes. Establishment of such standards is a major program in its own right, and deserves the attention of several independent groups of experimenters.

Numbers of Rockets. The rocket program in optical astronomy has been a very modest one thus far. In the past three years, the program produced a total of 21 Aerobee flights, carried out by five groups (see Table 10). The smallness of this number is at least partly due to the extended time required and the limited manpower available to construct payloads and to perform the laborious data-reduction needed for unstabilized rockets. The increased use of recoverable instrumentation and the advent of reliable control systems is markedly increasing the flight potential of each experimenter in the program. Moreover, the initiation of far-infrared-region observations, plus the improved performance capabilities of the control systems currently available and planned for the near future, open up many new and attractive observational possibilities that will undoubtedly result in increased demand among existing groups and increased participation by new groups. Counting only those groups known to have definite flight plans at this time, the Committee estimates that the optical-astronomy program will require not less than 50 Aerobees over the next 3 years (Table 11). The introduction of new groups into the field is very likely to increase the total usage well beyond this figure. This usage does not include the solar, airglow, planetary, or x-ray programs. The Committee concludes that the optical-astronomy program will require a rate of about 17 to 20 Aerobee flights per year. About five rockets per year will require the currently available coarse-attitude-control system, while about 10 per year will require a higher-precision pointing capability based on the use of star trackers; some flights will be with unstabilized rockets.

Table 11. Optical Astronomy Aerobee Estimate

Institution	Past 3 Years	Next 3 Years
NRL	5	9
Kitt Peak	1	3
Goddard	10	24
Princeton	3	6
Wisconsin	2	3
Cornell	0	3
Arizona	0	3
	—	—
	21	51 Aerobees[a]

[a] 44 of these (all but 7 of the NRL rockets) will carry the Attitude Control Systems (ACS).

Technical and Operational Requirements. The most critical requirement for nearly all optical-astronomy rocket programs is good pointing; reliable control-system operation is essential. Several classes of systems with differing degrees of pointing accuracy should be available. Coarse-pointing accuracy to ±2° of arc with ±1/4° jitter is the minimum requirement, and ±1 min of arc pointing accuracy with ±1/4 min of arc jitter would be useful for a large number of experiments. An ultimate goal of 20 sec of arc accuracy with ±2 sec of arc jitter is highly desirable in order to achieve the ultimate potential of rockets in astronomical research.

The increased number of astronomical rockets requires a corresponding increase in nighttime range operations. This need should be recognized as a legitimate and necessary aspect of the astronomical rocket program. The success of many optical-astronomy experiments also depends upon recovery of film. While the current daytime recovery system may be adequate, the development of a reliable nighttime recovery procedure would significantly improve the probability of successful night observations using photography.

The Role of Rockets in X-Ray and Gamma-Ray Astronomy

Introduction. X-ray astronomy is still in its infancy, but enough progress has been made to permit a straightforward estimate of rocket requirements for several years. Future progress must follow the classical lines of observational astronomy—detection of sources, position location, definition of size and shape, spectral resolution, determination of polarization, and measurement of temporal flux variations.

The sensitivity of detection depends on the product of the aperture of the detector and the time of measurement. Most of the observations thus far have been made with Aerobee rockets lacking stabilization or having only crude stabilization. Scans of the sky with unstabilized rockets permit only a few tenths of a second of count integration per discrete x-ray source. A well-stabilized rocket can provide about 300 sec for a single source, thus gaining more than a thousandfold in count integration. Larger rockets than the Aerobee can provide space for larger-area detectors. Increase by a factor of 10 could readily be obtained with existing rockets at about twice the Aerobee cost.

Position location requires reference of the x-ray source to optical star positions. A smoothly controlled slow-drift motion applied to an Aerobee rocket would permit position measurement to an accuracy of about 1 min of arc.

Focusing x-ray optics now under development have achieved 5 sec of arc image resolution under laboratory test conditions. Fine pointing and small jitter rate would permit photography of the strongest sources already observed. Modulation collimators may provide similar definition even within the dimensional confines of the Aerobee rocket.

The strongest x-ray source in Scorpius is bright enough so that Bragg crystal spectrometry could be employed to obtain 0.1 Å resolution for observations in the range 0.5-10 Å in the 300-sec observing time available with a stabilized rocket. Proportional-counting techniques and differential-filter photometry could be applied profitably with cruder stabilization.

Polarization measurements appear to be marginally feasible through the use of the Thomson scattering effect on liquid hydrogen. If the polarization of x rays from the Scorpius source exceeds 3 percent, the effect is estimated to be detectable from a stabilized Aerobee.

Temporal flux variations are likely to be present on a time scale of months to years. Absolute photometry of high precision would permit the detection of secular changes from flight to flight.

The foregoing statements clearly indicate the broad observational approach that is possible with rockets. At present, three laboratories, NRL, MIT-ASE, and Lockheed, are actively engaged in x-ray astronomy, and use about six Aerobees per year. Columbia University, Stanford University, and the Smithsonian Institution are preparing to conduct rocket flights. The demand for x-ray astronomy rockets will very likely approach about 12 per year within the next 2 years, and high-quality stabilization will be essential.

The immediate opportunities in rocket astronomy should not mask the importance of satellite observations in the coming years. Explorer-type satellites, operating for several months, can provide count integration orders of magnitude greater than is obtainable on rocket flights. Important experiments have been proposed for OAO, the AES, and from a lunar base. However, strong support for the rocket program will guarantee a rapid refinement of our knowledge of all of the basic parameters of the major x-ray sources and point the way to the most-effective use of the more-expensive orbiting systems.

Role of Rockets. The discovery of the first discrete x-ray sources was accomplished with a rocketborne instrument, and it is anticipated that a very substantial fraction of x-ray research will continue to use rockets. Rockets, balloons, and satellites supplement each other. Each has an important role that may change. At present, the Committee sees the following important points in connection with the role of rockets in x-ray research:

(a) As with balloonborne instruments, but in distinction to those borne by satellites, rocketborne instruments can be conceived, built, and flown on a relatively short time scale. New developments can be investigated promptly. This has the important function, among others, of insuring that the more-elaborate and more-expensive satelliteborne experiment will be as up to date as possible.

(b) Rocketborne experiments are much more adaptable to graduate-student training than are satelliteborne experiments. A graduate student has a reasonable chance to participate in conception, construction, flight, and data-analysis aspects of a rocketborne experiment. This amount of participation is now practically impossible in satellite experiments and is fast becoming entirely impossible.

(c) The relatively short flight time for rocket experiments is partially offset by the fact that the instruments are recoverable for further flights.

(d) Technology will certainly be in a continuing state of rapid development and innovation. Rockets provide an ideal tool for the trial of new instrumentation prior to satellite assignment.

(e) Over the next several years, rockets will continue to be a primary x-ray exploration vehicle. More groups may be expected to participate in this important work and should be encouraged to do so. Even the number of rockets available for use by the currently active groups is barely adequate at a level of six per year. At least a dozen rockets each year are considered essential for maintaining the present pace of scientific progress and as necessary preparation for ambitious satellite missions.

(f) As the work progresses, rocket vehicles of increased capabilities are required. Increased volume and weight-lifting capabilities beyond those of the Aerobee class of rockets appear desirable for survey experiments. Existing military hardware should be exploited for this purpose.

(g) Greater pointing accuracies are required to exploit the fine angular resolutions that can be achieved with present instrumentation. Pointing accuracies finer than $1/2^\circ$ of arc, and small jitter rates, are essential. Pointing accuracies of 1 min of arc have been achieved in developmental systems. Such systems should be made available for use in x-ray astronomy and their performance improved, particularly with respect to jitter or drift rates.

The Use of Sounding Rockets and Satellites in Radio and Radar Astronomy

Not much space radio astronomy has as yet been done, but a considerable proportion of what has been done has, in fact, used sounding rockets. For example:

(a) The University of Michigan group led by F. T. Haddock made measurements in September 1962 of the cosmic background noise levels at 0.75, 1.225, and 2.0 MHz using a radiometer carried to 1700-km altitude in a Journeyman four-stage solid-fuel rocket. The scientific payload weighed 98 lb.

A second rocket flight by this group took place in late June, from Wallops Island, Virginia.

(b) The Harvard College Observatory group (Huguenin et al.) in 1962 used a military rocket to fly a radiometer for observations of the background noise at 0.7 and 2.2 MHz.

(c) The Goddard Space Flight Center group led by Stone launched a cosmic-noise background radiometer, instrumented for measurements at 1.91, 2.85, 3.60, and 4.70 MHz, to 1100 km above Wallops Island in 1964. A four-stage Javelin rocket was used.

The Soviet Union and the British (using Skylark rockets) have made measurements of antenna impedance of dipoles.

Satellites have also been used to measure long-wave radio-frequency cosmic-noise levels, as follows:

(i) Harvard College Observatory on a military satellite
(ii) Various workers using results from the Alouette top-side sounder
(iii) The Michigan group on OGO-1 (launched March 1964)
(iv) The British on Ariel II (launched March 1964)
(v) The Soviet Union on Elektron II and Elektron IV.

No sounding-rocket or satellite observations in the fields of millimeter-wave radio astronomy, nor in radar astronomy, have yet been carried out.

Future Possibilities

(a) Millimeter-Wave Radio Astronomy. It is probably safe to assume that there will not be a great demand for sounding-rocket experiments in the future development of millimeter-wave radio astronomy. A relatively small amount of testing of cryogenic systems and detectors is possible for the millimeter-wave work; here, pointing precision of 1 to a few min of arc with payloads of about 100 lb carried to an altitude of 300 km would appear to be adequate. Serious observations, for which parabolic dishes of more than 10 ft in diameter are needed, are not likely from rockets.

(b) Space Radar Astronomy. Again, rockets are likely to be needed only for limited testing of the spaceborne component of, for example, a bistatic radar system. No worthwhile major experiments seem likely to generate a rocket program. Most require planetary orbiters.

(c) Long-Wave Space Radio Astronomy. Here, the rocket program is likely to be more extended. It should supplement (a) Radio Astronomy Explorer satellite experiments, and (b) the development of the large long-wave antennas envisaged in the Radio Astronomy Working Group's report.

In serving millimeter-wave radio astronomy, rockets would be used for tests of radiometer systems, antenna deployment, and similar tasks. A special task is the measurement of the complex antenna impedance at various levels in the outer ionosphere and in the interplanetary plasma. These tasks are also needed in space radar astronomy. Altitudes may have to be considerable; certainly, payloads of 50-75 lb need to be taken to

20,000 km. Pointing precision is unimportant, but the rockets should not spin or tumble badly. Later developments of space radar astronomy will call for equipment and antenna tests up to very great heights, perhaps to 100,000 km with 50-100 lb of payload. The effects of the radiation belts or of the plasma tail of the Earth may need special study.

If long-wave space radio astronomy grows, as the Working Group suggests and hopes it will, the availability of high-altitude sounding rockets will be a valuable stimulus and aid for conducting short-time exploratory radio-astronomy experiments. A few rockets per year at solar maximum could certainly complement valuably RAE satellites in studies of solar-burst phenomena. Rockets may provide a useful way to check the instrumental calibration of satelliteborne radiometers.

It is not possible to give any substantiated estimate as yet of the number of rockets the radio-astronomy program might generate, but it seems clear that several per year could be usefully employed. It is not out of the realm of possibility that the field might blossom, as have the other aspects of rocket astronomy, following new discoveries or new technological developments.

APPENDIX 1: LIST OF PARTICIPANTS

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APPENDIX 2: SOUNDING-ROCKET LAUNCHING FACILITIES—WORLDWIDE DISTRIBUTION

Maurice Dubin

SUMMARY

Sounding rockets continue to serve as a major method of space research for investigating aeronomical and astronomical problems. The distribution of sounding-rocket sites and the general level of site development have been reviewed. Scientific problems which depend on the geographical locations of the sounding rocket launching site, and problems of a nature requiring synoptic observations, have also been reviewed. It has been found that, although many scientific and synoptic problems may be investigated from existing sounding rocket facilities, in some cases additional launching sites in specific geographical areas appear desirable.

INTRODUCTION

The number of operational facilities for launching sounding rockets has continued to increase rapidly during the last few years, with several countries engaged in space research undertaking the technological developments required for scientific investigations using sounding rockets and satellites. The location of sounding-rocket ranges has been determined mainly by logistic and safety requirements. In some cases, such as that of the auroral site at Fort Churchill, ranges have been constructed specifically to undertake research on special scientific problems. A number of scientific problems involving coordinated launchings of sounding rockets from several sites have been carried out, beginning during the International Geophysical Year. During IGY, World Days were set aside for coordinated launchings of sounding rockets. Through COSPAR's Working Group II Panel on Synoptic Sounding Rockets, several synoptic scientific investigations have been proposed and worldwide cooperative flights have been undertaken. It has been from studies of this nature that the advantages derived from the simultaneous or coordinated sounding-rocket investigations at various geographical sites have been established. It is now possible to investigate problems in meteorology and aeronomy by means of simultaneous or consecutive flights (from several launching sites) for the study of solar-terrestrial relations and effects from latitude variations.

The distribution of sounding-rocket sites has become all the more important in the correlation of observations obtained from satellites with observations of phenomena which vary with altitude. The capacity for undertaking such comparisons depends on the geographical distribution of sounding rocket launching facilities and the state of development of these facilities.

A detailed review of sounding-rocket facilities has been published in a series of two articles by Mitchell R. Sharpe, Jr., and John M. Lowther.* Accordingly, the main purpose of this review is to examine the geographical distribution of launching sites and the possibilities of using these sites on a cooperative basis for undertaking special problems of scientific research.

LAUNCHING FACILITIES

The detailed description of the launching facilities for sounding rockets are given in the survey by Sharpe and Lowther. In the present review, the range description has been kept as short as possible. A major range is a launching site usually having several rail launchers, launching towers, or pads with modern tracking radars, telemetry, communication systems, and a blockhouse complex.

The launching sites and range descriptions have been listed alphabetically by country, with some descriptive highlights. Several sites listed in the Sharpe-Lowther survey have not been included in the tabulation because it appears that active scientific programs using sounding rockets have not been performed at these ranges. A number of ranges were not included in their survey: the ranges in Surinam, Brazil, and French Guiana, now under construction; the Pt. Barrow, Alaska range; and the mobile launching range on the USNS Croatan. The tabulation of sounding-rocket sites has also been supplemented by including the locations of most of the meteorological-sounding-rocket ranges used by the United States.

Argentina

Chamical (30.5°S, 66°W). First Centaure launchings November 1962. Nike-Cajun, Nike-Apache, Telemetry DOVAP, Radar, Recovery.

Australia

Woomera (31°S, 137°E). United Kingdom launching site, major range. About 15 years in existence. Skylark rockets used mainly; capability for launching vehicle such as the Blue Streak. Main range in the Southern Hemisphere. Recovery.

Brazil

Natal (5°S, 35°W). Under construction, operational mid-1965. Nike-Apache, Nike-Tomahawk, Dragon planned.

*Part I, "Survey of Facilities in the United States," Advances in Space Science and Technology, Vol. 6, pp. 248-436, and Part II, "Survey of Facilities Outside the United States," Advances in Space Science and Technology, Vol. 7, pp. 2-145, F. I. Ordway, III, ed. (Academic Press, New York, 1965).

Canada

Fort Churchill (58.8°N, 94.3°W). Major range; constructed in 1956 for IGY. Nike-Cajun, Nike-Apache, Black Brant, Aerobee, Aerobee 300, Nike-Tomahawk, and Javelin. Main range in auroral zone. Recovery over land. Auroral-observation equipment available.

France

Ile du Levant (43°N, 6°E). Major range. Belier, Centaur, Dragon — limited altitude.

Hammaguir, Algeria (31°N, 3°W). Major range, Belier, Veronique, Centaur, Dragon, (Rubis); recovery. Second range site 700 km away, at Raggan.

French Guiana (5°N, 53°W). Major range, under construction; operational 1967 or later.

India

Thumba (8.5°N, 77°E). New range, at magnetic equator. First launches 1963; DOVAP, telemetry, radar; Nike-Cajun, Nike-Apache, Centaure.

Italy

Sardinia (39.6°N, 9.5°E). Major range, with limited altitude. Nike-Cajun, Nike-Apache, Centaure; rail and tower launchers available.

San Marco Platform. Ocean transportable range, under development. Capability for launching large rockets and Scout satellites; Nike-Cajun, Nike-Apache.

Japan

Uchinoura, Kagoshima (31.3°N, 131°E). Major range, new construction. Kappa series, K-8, K-9M types; Lambda series, L-3; Mu series.

Norway

Andöya (69.3°N, 16°E). New range. Nike-Cajun, Nike-Apache; Telemetry; in auroral zone.

Netherlands

Dutch Guiana, Surinam (5°N, 55°W). Operational mid-1965; Nike-Apache.

Pakistan

Sonmiani (26°N, 67°E). First launchings in June 1962. Nike-Cajun, Nike-Apache.

Sweden

Kronogard (66°N, 18°E). Interim range; summer 1962, 1963, 1964. Nike-Cajun, Nike-Apache.

Kiruna (68°N, 21°E). New major range, may be operational 1965; recovery; peak altitude 300 km; 50 flights a year. Centaure, Nike-Apache, and larger rockets. Geophysical observatory in auroral region.

United Kingdom

See Australia, Woomera (31°S, 137°E).

United States

White Sands, New Mexico (32.5°N, 106.5°W). Major range, established 1945, Nike-Cajun, Nike-Apache, Aerobee 150, Aerobee 350. Recovery.

Cape Kennedy, Florida, ETR, (28.2°N, 80.6°W). Major range, mainly used for satellite and space-probe launchings.

Wallops Island, Virginia (37.8°N, 75.5°W). Major range. Nike-Cajun, Nike-Apache, Nike-Tomahawk, Aerobee, Aerobee-300, Aerobee-350, Javelin, Journeyman, Scout probe, and satellites. Water recovery; multiple range capability; 3-5 rail launchers.

Eglin AFB, Florida (30.4°N, 86.7°W). Major range. Nike-Cajun, Nike-Apache, Aerobee.

Point Mugu, California, WTR, (34.1°N, 119.1°W). Major range. Nike-Cajun, Journeyman, Polar Orbiting Satellites.

Hawaii, Kauai (21.9°N, 159.6°W). Major range. Nike-Cajun, Nike-Apache, Nike- and Terrier-Tomahawk; water recovery; operational distance and angle-measuring equipment.

Kwajalein, Marshall Islands (08.8°N, 167.7°E). Nike-Cajun, Nike-Apache; radar and telemetry.

Tonopah, Nevada (38.0°N, 116°30'W). Major range, with limited altitude. Nike-Cajun, Nike-Apache, Nike-Tomahawk; operational distance and angle-measuring equipment; recovery.

McMurdo Sound, Antarctica (77.9°S, 166.6°E). Meteorological rocket range.

Ship Launching Facility; operational 1965 in Pacific near west coast of South America. 10 thousand tons, 150-meter flight deck; Nike-Cajun, Nike-Apache; radar.

Pt. Barrow, Alaska (71.3°N, 156.8°W). New limited range, 1964. Nike-Cajun, Nike-Apache.

Keweenaw Peninsula, Michigan (37.5°N, 87.7°W). New limited range. Nike-Cajun, Nike-Apache capability.

USSR

Mid-latitude site: (see Sharpe-Lowther survey). Geophysical rockets. Kapustin Yar (48.6°N, 45.8°E). Major range, established 1946. Tyuratam, Kazakhstan (45.6°N, 63.3°E). Major range.

Franz Joseph Island (80.5°N, 58°E). High Latitude Observatory; various sounding rockets.

Expedition ships. Meteorological rockets.

DISCUSSION

The main use of the sounding rocket continues to be for scientific research, in the investigation of aeronomical phenomena as a function of altitude in a region of space not accessible to satellites. For problems in astronomy and solar physics, the location of sounding-rocket launching sites is not very important, as the rocket extends the spectral region for observations of astronomical objects by carrying the instruments to a region free of atmospheric interference; a launching site in the Southern Hemisphere is, of course, necessary for astronomical observations over the southern portion of the celestial sphere.

Scientific problem areas requiring the use of sounding rockets include studies of the atmosphere, the ionosphere, and of energetic particles and solar radiations absorbed in the atmosphere. These investigations are concerned with measurements of the distributions of charged particles and of the ionic composition of the ionosphere, studies of airglow emissions, investigations of aurorae, measurements of atmospheric composition and its variability, and the study of structural and dynamical effects of the atmosphere. Many of the aeronomical parameters undergo extensive variations with geographical position and altitude. Over different locations, the variation of solar radiation and charged-particle bombardment further affects the dynamical conditions in the atmosphere. Most of these phenomena may be properly studied only by coordinated flights of instrumented sounding rockets from several launching sites.

Although there now exist about a score of launching sites, the distribution of these sites is not ideally suited for investigating a number of scientific problems. The early work with sounding rockets was carried out mainly from mid-latitude sites in the Northern Hemisphere; the addition of a site in the auroral zone—the Fort Churchill site, during the IGY—greatly enhanced the capability of studying both auroral effects and latitude variations. Auroral and high-latitude sites are now available for research at Andöya, Norway, and Kronogard, Sweden; the Kiruna, Sweden, launching site, as well as the limited use available at the Pt. Barrow site, should greatly improve auroral observations in the Northern Hemisphere.

New sites have also been constructed within the last 2 or 3 years in the equatorial region and in the Southern Hemisphere. The development of a major site in India, at Thumba, at the magnetic equator, is a desirable addition for investigation of the equatorial region. Other equatorial sites are fast becoming available, with a second magnetic-equator location in Brazil and sites in French Guiana, Surinam, and Kwajalein, as well as the mobile sites on the San Marco Platform and on board ships. A major site at Chamical, Argentina, should prove very useful in the study of latitude- as well as longitude-dependent effects. Woomera and Chamical are now the only sites available in the Southern Hemisphere, except for the small meteorological-rocket site in New Zealand; there is also a potential for extending the use of the Antarctic meteorological-rocket site at McMurdo Sound to higher-altitude research.

RECOMMENDATIONS

1. Arctic Studies. Coordinated flights from the three or more launch sites at different geographical locations in the northern auroral zone and Arctic polar region appear worthwhile for the investigation of particle bombardment, ionospheric characteristics, atmospheric structure, composition, and airglow.

2. Auroral Studies. Conjugate investigations of the aurorae, particularly during the next solar maximum, could be considerably improved with a site in the Antarctic auroral zone.

3. Southern Hemisphere—mid-latitude sites. Zonal studies coordinated with measurements from Australia and South America and meridional investigations in conjunction with European launches suggest the need for an additional site in the Southern Hemisphere, in Africa.

4. Western Hemisphere. The distribution of sites in Canada, the United States, and Argentina suggests the desirability of constructing an equatorial site on about the same meridian. Mid-latitude and equatorial sites in South America should probably be developed into major launching facilities. The new Surinam, Brazil, and French Guiana sites, in conjunction with Chamical, support this suggestion.

5. Shipboard Site. Mobile launch sites using large ships are recommended for latitude surveys, auroral-conjugate studies (where the conjugate launching site is over water) and special investigations (i.e., observations of solar eclipses).

6. Coordination. Standards for synoptic investigations should be set for each scientific problem being investigated. The elements of each coordinated experiment should include:

(a) the acceptable instrumentation and the parameters to be investigated;
(b) the geographic locations of interest;
(c) the number of sounding-rocket flights required; and
(d) a communication and control system for organizing the investigation.

Additional improvements in existing launching facilities should be considered to support such coordinated flights.

VIII
Space Research and the University

INTRODUCTION

The Working Group on NASA-University Relations (see List of Participants, Appendix 1) met formally on June 28 and 29, 1965. Several informal discussions were also held during the course of the summer study, and, more importantly, each of the other working groups was asked to consider the topic and was provided with the working papers noted below. The recommendations in this report represent a consensus of the 125 participants in the Woods Hole study.

The general background for the Working Group's discussion was twofold: first, the Iowa summer study recommendations of 1962 had been made available in advance of the present study; second, a recent report by the Space Science Board was at hand, affording a specific point of departure for the Woods Hole study. This examination, conducted by an *ad hoc* committee of the Space Science Board in the Spring of 1965 under the chairmanship of Professor John A. Simpson, included (a) a study of NASA's Sustaining University Program, reproduced here as Appendix 2, and (b) a series of questions on the general subject of relations between NASA and the academic community. In addition, NASA made available detailed information on its Sustaining University Program; these were supplemented by oral reports by and discussions with invited agency officials. The Working Group addressed itself to two major topics: (1) the Sustaining University Program and (2) NASA-university relations.

THE SUSTAINING UNIVERSITY PROGRAM

The opening statement on this subject of the Iowa study in 1962 read:

> An examination of the pertinent excerpts from the National Aeronautics and Space Act of 1958, as amended in 1961, leads to the conclusion not only that NASA possesses adequate authority to develop a program of university participation in space science and technology which is of broad scope and substantial magnitude, but, indeed, that it is directed to do so. In August, 1961, several working groups organized by NASA met to consider the role of the universities and colleges in the nation's space program, and NASA's relationship to them. It was concluded that NASA must turn to the universities not only for the trained manpower it needs but also for the basic research undergirding NASA's activities. For these reasons, it seemed clear, the universities are vital—probably decisive—for the future of space science and exploration.*

The Working Group on NASA-University Relations believes that these statements remain valid. It concurs in the analysis of the Simpson committee (Appendix 2) which shows the fractional role allotted to university participation, and finds it unnecessary to repeat its own almost identical findings here. The following recommendation was therefore adopted:

Recommendation 1. The Working Group, having reviewed and deliberated upon the Simpson Report, unanimously adopts the Report as an integral part of the current Summer Study and urges that the government continue suitably the imaginative and effective Sustaining University Program of the NASA.

For convenience and emphasis and because the Woods Hole Summer Study amplified upon the four key recommendations of the Simpson committee Report, they are restated here:

Recommendation 2. The Working Group recommends that the budget for training be increased from its present level of about $20,000,000 to $30,000,000 per year, and notes with approval that NASA has requested authorization to put $25,000,000 next year into training grants.

Recommendation 3. The Working Group recommends that the budget for facilities grants, which has been shrinking at the rate of $2,000,000 per year over the past two years to where it is now at the $8,000,000 level, be increased to approximately $15,000,000 or $20,000,000.

* "NASA-University Relationships," Chapter 12, p. 12-1, A Review of Space Research, National Academy of Sciences—National Research Council Publ. 1079 (1962).

Recommendation 4. The Working Group recommends that sustaining research grant funds be sharply increased, by approximately a factor of two.

Recommendation 5. The Working Group recommends that a reasonable rate of growth for all these components of the Sustaining University Program be re-established for the years ahead.

Congress explicity recognized the role of the scientific academic community in its legislation establishing NASA. NASA in turn, has recognized this role in its policies and programs. These congressional and executive assessments rest on several considerations: although scientific research represents only a small part of NASA's current budget, the total national space effort is linked to and dependent upon science and engineering. Manned space activities depend upon a host of scientific and research investigations; ultimately history will judge such enterprises by their results: among them, we believe, new knowledge will rank high. Moreover, the space effort, directly or indirectly, draws upon the academic community for experts to serve in government and industry facilities: it is reasonable that this source be sustained.

Aside from the conduct of significant research, universities provide the sole mechanism of propagation of scientists. In discussions of the public benefits accruing from the space effort, much attention has been given to what is called "spin-off." Without in any way minimizing technological spin-off, which we believe to be significant—particularly in such space applications as communications, weather operations, and geodesy, but also in a variety of technological-industrial processes and applications—we suspect that in the long run the academic spin-off may be the greatest. The Sustaining University Program makes possible the production of specialists; some stay in the universities, engaging in research and the training of another generation; some enter government operations; some go to industry. They will shape the national future in many aspects of science and technology. Any specific usefulness produced now will be increased manifold by the persons trained in the Sustaining University Program.

NASA-UNIVERSITY RELATIONS

The Simpson committee commended NASA on its Sustaining University Program in the following terms: "NASA has made an admirable and bold beginning in the discharge of its responsibilities for strengthening institutions of higher education—responsibilities that derive from the terms of its congressional charter. The programs for construction of laboratory facilities, training graduates, and providing research grants have all proved to be essential for effective participation by the universities in the space sciences and technology." The Working Group on NASA-University

Relations concurs. Both groups recognized, however, the existence of problems in the relations of NASA and universities. Some of these relate to the Sustaining University Program, but the major problems relate to university participation in flight programs.

The Simpson committee, in the course of its study, noted a number of areas of mutual concern where further attention by both the universities and NASA would be beneficial. Thus, the nature and function of universities, which determine how academic institutions operate, merit better understanding by some NASA field units. On the other hand, many universities fail to appreciate the nature and function of NASA as stipulated in its legislative charter. It is encouraging that progress has been made: thus, NASA Headquarters' appreciation of university requirements has led to the imaginative Sustaining University Program and to constructive guidelines and policies. (In particular, the Goddard Space Flight Center has shown awareness of the problems of university experimenters.) In turn, university scientists have increasingly recognized NASA's desire to strengthen the universities. Specific problem areas are being looked at by both NASA and the universities. Some may not have simple solutions either because of their natures or because of the basic structure of NASA or of the universities; some will require give and take on both sides; all will benefit from closer relations and discussions, conducted on a continuing basis.

Specific topics and areas noted by the Simpson committee included: the over-all question of mutual understanding of functions; the desirability of uniform application of NASA Headquarters policies; grants versus contracts; overhead; the growing complexity of flight experiments; short-range versus long-range research problems; and the relationship of the nature and level of in-house research with that carried out under grants and contracts.

Such questions were referred to the Woods Hole Summer Study. Other topics emerged in the course of discussions held by the working groups. These may be grouped in four categories: administrative procedures, university responsibilities, university participation in flight experiments, and some aspects of good research.

Administrative Procedures

NASA Headquarters has instituted policies intended to strengthen the universities. These policies recognize the differences, for example, between universities and industry. Their uniform application by all NASA field centers (as is so well done by the Goddard Space Flight Center) and by subcontractors calls for continued attention by NASA Headquarters. Basically, the carrying out of Headquarter's policies depends upon a kind of education of all groups involved, and, because the composition of groups changes with time, periodic "refresher courses" are necessary.

NASA has available to it, and uses, both grants and contracts in the support of university work. Both forms appear useful, and the present degree of flexibility in their use is commended. It is desirable that NASA be responsive to university wishes as to one or the other of these instruments for research support. Nonetheless, the overhead problem for grants is a real one, and it is suggested that this matter be explored by NASA along the lines of the recommendations in the "Kistiakowsky Report."*

NASA's present responsibility for, and control over, the use of funds awarded to universities was not only recognized as appropriate but was commended. For some research programs, universities believe that NASA should select the subtasks within broadly funded sustaining university grants; for others, universities are anxious to have unencumbered funds for support of research. Flexibility is therefore necessary. Moreover, some care is needed so that mission orientation or the desires of mission-oriented groups for short-term results do not conflict with NASA's goals in its long-range sustaining research grant program. These goals are important to NASA and are crucial to the most imaginative participation by universities in the space effort. No changes are called for at present in the general method of awarding grants and contracts in the Sustaining University Program.

Earlier, implicit reference was made to the difficulties encountered by universities if they are regarded as organizations similar to industrial enterprises. NASA should make every effort to simplify procedures and requirements: in general, the energies of faculty members and graduate students are best utilized in substantive scientific work, and such persons should be occupied as little as possible with administrative details. Thus, to cite a small example, restrictions on university staff in access to their experiments at spacecraft facilities should be minimized.

University Responsibilities

There are several particular responsibilites that, in general, merit further attention on the part of academic institutions. One of these relates to university mechanisms for dealing with NASA. Even within a single university, the lines of authority are often vague; moreover, organizational structure varies widely from university to university. NASA representatives often find it difficult to identify those with whom to discuss the various aspects involved in a collaborative endeavor. Universities might well give thought to this administrative problem, seeking to develop an effective mechanism for their relations with NASA.

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* Federal Support of Basic Research in Institutions of Higher Learning, National Academy of Sciences—National Research Council Publ. 1185 (1964).

Another responsibility is in reporting to NASA on work in progress or completed. From time to time, unreasonable delays characterize university reporting.

Customary care in being responsive to agreements needs little comment. But one topic calls for explicit attention: the facilities grants of the Sustaining University Program. Under the NASA charter, this portion of the university support program can only be justified in terms of active space-related research programs and their facility requirements at a given university; in large measure, support for the operation of such facilities will have to be generated by the universities themselves. Justifiable demand has to be created first, and the decrement in this portion of NASA's program observed over the past 2 years does not wholly represent an arbitrary decision to cut back. It is also noted that under NASA's regulations, in return for whole-dollar support for facilities, a university is required, as its contribution, "to seek ways in which both the direct and indirect benefits of such [NASA-supported] research can contribute to the economic, social, and general well-being of the nation."* In some measure, the universities may not have responded explicity to this obligation, although it must be recognized that such an objective cannot be attained quickly. Moreover, while the universities must seriously consider this contractual provision, NASA must recognize that insofar as universities carry out their primary roles—in the promotion of learning, in the seeking and acquisition of new knowledge, in its dissemination, and in scholarly study of its implications—they are, to a real degree, responsive to this obligation.

University Participation in Flight Experiments

The Simpson report (Appendix 2) is addressed to a specific appraisal of the NASA Sustaining University Program. To a large measure, the present study has also been concerned with the Sustaining University Program. In this section, however, the interest is broader: here we are concerned with the active participation of the universities in research using space tools. Such research embraces almost every discipline of science and ranges from space-oriented ground studies to manned and unmanned investigations of the nearer planets.

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* To cite more fully from the Memorandum of Understanding signed by NASA and the university when a facilities grant is made, "the National Aeronautics and Space Administration is particularly desirous that the environment in which space research is conducted and its full benefits realized will be characterized by a multidisciplinary effort which draws upon creative minds from various branches of the sciences, technology, commerce, and the arts. The desires of the university are in conformity with this policy, and the institution tends to foster and conduct research in all areas of space-related sciences, bring to bear on this research the efforts characteristic of a major university, and seek ways in which both the direct and indirect benefits of such research can contribute to the economic, social, and general well-being of the nation."

The participation of universities in space research can best be facilitated by taking into account those conditions that characterize academic work. Thus, the most imaginative scientists in our universities can be attracted to space research if there is reasonable continuity to their work and if successful flights of experiments are reasonably certain.

The ratio of graduate students to senior university investigators in space-flight experiments is relatively low. The cause lies largely in the scheduling of these experiments, which would somehow have to match the schedule of graduate training if graduate students are to contribute. Even at the professional and postgraduate levels, however, the long lead times of space work are a problem. It is reasonable to expect that as conventional launchings increase and become more routine, their lead times will become shorter and more flexible. Coupled to suitably supported ground, balloon, and rocket research of direct space interest, and assisted by a vigorous and growing Sustaining University Program, a varied activity that will embrace continuity and timely flight opportunities should become available for even graduate participation. To achieve this goal will nevertheless require energetic and imaginative steps within NASA.

There is, however, a related problem now facing the scientific community and NASA that appears far less amenable to solution: as more powerful spacecraft become available late in this decade, and as planetary investigations begin, lead times and support will become more difficult problems. Two to five years between the "freezing" of a payload and the actual mission will be common. Such lead times may discourage imaginative scientists from submitting experimental proposals and may make difficult continuity of work, particularly on the part of younger men. Moreover, the funding of experiments itself may be more complicated, in part to ensure sustained work on a problem during a flight-waiting period, in part because ground-based work in the waiting period may outmode a given experiment or reveal modifications not easily added, in part because scientific advances may yield experiments of importance that cannot be accomodated in a reasonable time when few missions to, say, a given planet in a decade, are in the offing. Because this problem requires further analysis, no recommendation is being submitted, but the Working Group requests that the Space Science Board undertake an appropriate study of the problem as soon as possible.

Two additional areas merit comment in terms of effective participation by university scientists. One has to do with advance information on prospective flights; the other relates to engineering support.

The nature of university departmental organization differs in many ways from the organization of an industrial enterprise or a government laboratory. For instance, few science departments have any significant complement of engineers and administrative staff. This means that response to notice of impending flights rests upon a faculty member already engrossed in research and teaching. Very often, notice of flight plans arrives with insufficient time for consideration and response. Early release of flight plans would be helpful.

Again, with few exceptions, colleges and universities do not have the staff to back up an experimenter with engineering advice and services. Universities thus suffer a serious handicap as against industry and government facilities. Here, too, there is every reason to expect satisfactory resolution. Some steps that would be helpful are listed below:

1. Individuals and groups at specific NASA centers could be explicitly designated as responsible for making engineering advice available.
2. In some cases, the practice of taking specifications and/or prototypes and then having fabrication done by or under contractual supervision of a NASA center would be helpful. It would, of course, be necessary that the scientist retain responsibility for the experiment; and that open communication be maintained among all three collaborators—the experimenter, NASA, and the industrial fabricator. The modifier "some" is emphasized because in most projects a single channel of authority and decision is preferable; two channels generally lead to confusion, delays, and friction. Thus, care by both universities and NASA is required in determining when deviation from single-channel authority might succeed.
3. Because in some circumstances the full utilization of spacecraft capabilities requires a research team, both NASA and the universities should encourage the formation of such teams.

The increasing complexity of large space systems (now emphasized even more by the capabilities of the Apollo and Apollo Extended Systems) will tend to make the problem of engineering back-up more difficult than it has been and is. NASA might well give consideration to the establishment of a "university services organization" to assume a variety of industrial, contractual, and engineering functions on behalf of universities. Goddard, it is noted, has been effective in doing some of this, particularly in rocketry, but first claim on its attention must probably be that of its own scientific staff. It can be argued that just as the new NASA Boston electronics establishment serves, in a sense, as an "industry service center," a "university service organization" merits consideration. It is even possible that several such centers, geographically distributed for convenience, may provide the desired solution, as suggested some years ago by the Space Science Board.

Some Aspects of Good Research

The engagement of creative human resources and funds in any activity calls for a care that needs no amplification here. Nor is it necessary to discuss the added burden of responsibility that the use of public funds entails. It is worth stressing, however obvious the point, that the very large costs of space systems, tracking, telemetry, and data processing warrant the most critical assessment of what we do and how we do it in space. Our concern has been restricted to space research, which is in actuality a very small (though substantial) part of the entire national space effort. An undercurrent throughout the discussions of the Working Group, and of the other working groups (e.g., in the Iowa study and in sessions of the Space Science Board), has been the question of good research. That important scientific problems lie before us has been amply documented, here and elsewhere. The great concern relates to identifying prospectively excellent work. In creative activities, the achievement of excellence lies in the individual. Neither science nor the nation can do better than what can be done by its population of talented individuals, in being or in development. Science and the nation can attract and make possible the participation of such men. This report is implicitly or explicitly concerned in large measure with the conditions that serve this goal.

To this end, also, explicit recognition of the variety of methods and tools that can contribute to space research is required. For example, the investigation of the planets requires far more work from ground-based optical, radio, and radar telescopes. Such work will yield results of intrinsic value, and will diminish the demand for flight experiments of certain kinds. The results of such work may well be important to further research and crucial to major (manned or unmanned) missions to the Moon and planets. Similar arguments can be made in support of expanding balloon and rocket studies.

Moreover, a proper effort on ground, with balloons, and with rockets, would serve not only to yield significant space findings, to eliminate some kinds of more costly flights, and to provide environmental data important to certain space missions; it would also serve the ends of good research. It would contribute to the sustained involvement of imaginative minds. It would do this by affording, as a by-product of a sound scientific effort, continuity in university research efforts, and by bridging any long gaps between space-flight experiments. And it would permit graduate students to tackle significant problems on a practical schedule. Accordingly,

Recommendation 6. The Working Group recommends that suitably space-oriented ground-based, balloon, and rocket programs be expanded as promptly as possible. The present rocket program should attain a level between two and three times that now in being as soon as possible.

The phrase "between two and three times" is not ambiguous: other working groups have been specific as to the level in their disciplines.

More directly relevant to the problem of judging prospectively good research is the question of how to evaluate proposals. Competition among proposals is, and should continue to be, encouraged. The screening of proposals by competent experts is crucial. One set of procedures should prevail. Accordingly, Recommendation 7. The Working Group recommends that formal panels of outside consultants, for the scientific screening of all specific research grants and all in-flight experiments, consisting of experts chosen on the basis of their experience and reputation, be established in those fields of science where appropriate.

The qualification "in those fields of science where appropriate" is included only because there are some areas (e.g., planetary investigations) in which a pooling of scientific interests has been effective. The problem tends to arise in scientific areas where (i) there are appreciable numbers of competing scientists within the government and in the universities and (ii) the experiments are largely performed by a single experimenter (or by a small group in one laboratory); moreover, the availability of engineering and administrative services, along with current planning information, to some groups makes it more difficult for other groups to compete fully.

Finally, the Working Group considers that the value of the NASA Sustaining University Program merits consideration of a broader approach. This program, as indicated earlier and in the Simpson report, has been effective in science. Might not its extension to engineering be as rewarding to the nation? [NASA is now supporting a Stanford University study of this question.] The Working Group suggests that the subject deserves attention especially in two areas: first, in creative engineering and, second, in the area of team approach to major problems and missions in which groups of scientists and engineers work together. The Working Group also suggests that this subject be studied by a committee of the Space Science Board.

APPENDIX 1: LIST OF PARTICIPANTS

WORKING GROUP ON NASA-UNIVERSITY RELATIONS

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APPENDIX 2: UNIVERSITY RESEARCH, GRADUATE TRAINING AND THE SPACE PROGRAM:

REPORT OF AN AD HOC COMMITTEE OF THE SPACE SCIENCE BOARD*
APRIL 21 1965

INTRODUCTION

This report is concerned with the programs and policies of the National Aeronautics and Space Administration that will have a long-term effect on the advancement of the sciences. Since the foundations of research and graduate education rest within the universities and similar institutions of higher learning, our inquiry has been specifically directed to those of NASA's policies and procedures that sustain these institutions. Our principal findings are summarized below.

First, we find that the NASA has made an admirable and bold beginning in the discharge of its responsibilities for strengthening institutions of higher education—responsibilities that derive from the terms of its congressional charter. The programs for construction of laboratory facilities, training graduates, and providing research grants have all proved to be essential for effective participation by the universities in the space sciences and technology.

Second, we note that while these forms of support have enjoyed a modest rate of growth over the past few years, this growth has now ceased, apparently because of budgetary limitations. The consequences of this cessation in growth, if continued, will be extremely detrimental, especially in view of the prevailing reliance on science and technology both by the agency and the country as a whole. In fact, these limitations in the long run may prevent NASA from achieving its stated goals. The fraction of the NASA budget devoted to support of university research, laboratory facilities, and student training is, even at present, less than 1 percent. We are concerned over the possibility that this budgetary limitation may not be simply a quantitative matter but rather imply a misunderstanding of the requirements for maintaining the quality of scientific and technological contributions to the space program throughout the years ahead. We shall attempt to place this problem in perspective and to propose some recommendations.

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* J.A. Simpson, Chairman; Kenneth Clark, R.B. Leighton, L.I. Schiff, Charles H. Townes, J.A. Van Allen; invited participants: T.L.K. Smull (NASA), J.T. Holloway (NASA); Secretariat: J.P.T. Pearman.

We have addressed ourselves principally to consideration of the programs that most directly support fundamental research and graduate training in the universities. As noted above, they are relatively small. However, the involvement of the universities in the current activities of "project-oriented" space research and technology is of a different character, even though it results in their benefitting indirectly from a significant fraction of the whole national expenditure on the space program. The following analysis may give some measure of the magnitude and special character of these activities.

In 1964, for example, the total NASA budget was about $5.1 billion. Excluding expenditures for manned space flight and for applied programs (e.g., meteorological and communications satellites), about $650 million was applied to purely scientific and technological activities. To this sum it is appropriate to add the cost of providing facilities for tracking and data acquisition ($270 million) and the total thus derived—$920 million—is then a rough measure of the scale of effort on the scientific side of the space program. This total includes the cost of providing the essential services, launching facilities, and rockets in addition to the expenditures on scientific research, instruments, and spacecraft for the various scientific missions. It has been fairly typical that about half the scientific experiments which these programs have made possible have involved university scientists. On this basis it can be construed that half of $920 million or somewhat less than 10 percent of the total NASA funds were expended in 1964 on scientific space programs that involve the university community directly or indirectly. Although these sums indicate the magnitude of the scientific space effort, the programs they support draw mainly on the competence of established scientists and laboratories and are not, in general, of a kind that would provide systematically for the training of new scientific workers nor support long-term research.

Many scholars and scientists within the universities are actively involved in the day-to-day pursuit of knowledge, through participation in one of NASA's space missions. This participation, while it enhances the scope of current scientific activities on the campus, usually does not augment the production of newly trained workers. Moreover, such activities often do not remain long on the university campus and, while large amounts of money may be involved, a substantial portion of these funds is ultimately devoted to the procurement from industry of the specialized apparatus and services that are required for space research. The magnitude of this more direct participation by the universities in the space program may be estimated from the actual obligations to universities for 1964 tabulated in Table 1. These show a total of about $109 million, or about 2 percent of the whole budget. Of this, some $36 million, or 0.7 percent of the whole, is devoted to the Sustaining University Program. The special significance of this small but important segment is discussed below.

Table 1

NASA FISCAL YEAR 1964 OBLIGATIONS TO UNIVERSITIES

	Headquarters	Centers	Headquarters + Centers
Research support	$ 38,450,353 (7,156,489)[a]	$ 10,776,230	$ 49,226,583 (7,156,489)[a]
Satellite instrumentation[b]	1,086,934	9,358,822	10,445,756
Tracking and data acquisition[c]	-	1,967,525	1,967,525
Research facilities	9,142,760[a]	-	9,142,760[a]
Training in space science and technology	19,815,471[a]	-	19,815,471[a]
NASA career employee training[d]	55,000	1,520,000	1,575,000
Apollo guidance[e]	-	16,286,000	16,286,000
Miscellaneous	304,382	147,176	451,558
Total	$ 68,854,900 ($ 36,114,720)[a]	$ 40,055,753	$108,910,653 ($ 36,114,720)[a]

[a] Sustaining University Program.

[b] Satellite instrumentation funds are listed separately because the rigorous environment to which such apparatus is subjected makes its construction and testing an extremely complex and difficult task. Its design and fabrication is often beyond the technological capability of a university, yet the scientist must be intimately involved in the development of his instrumentation. Consequently, this phase is contracted with the university; however, in many instances it is, in turn, subcontracted by the university to specialized industries.

[c] Tracking and data acquisition is largely a service type of activity provided by a few universities.

[d] NASA career employee training represents the university program entered into by NASA to provide for continued professional development of NASA staff. This program primarily involves working agreements that are established directly between the universities and the NASA centers.

[e] Apollo guidance is a separate entry owing to the size of the effort and because it is largely the activity of one university, Massachusetts Institute of Technology.

(From The Nature and Scope of the NASA-University Program, by T.L.K. Smull, NASA SP-73, 1965, pp. 7-9).

In contrast to the "project-oriented" space research activities mentioned above, the programs that are intended to provide for continued vitality and creative activity within the universities receive less than 1 percent of the total NASA budget. These programs are collectively identified as the Sustaining University Program; they include provision for graduate training, construction of research facilities, and the granting of funds for a wide range of research. In a fundamental sense, these contributions to university activity may be regarded as the sole means for maintaining or augmenting the intellectual and creative resources on which the whole superstructure of the space program must ultimately rely. In the long run, the quality of the entire effort, whether scientific or technological, pure or applied, depends on the creative and imaginative qualities of those who are impelled to take up the challenge.

Figure 1 shows the growth of the components of these programs, including a projection to the levels proposed by the Bureau of the Budget. We note that their growth has not reflected the importance attached to them both by the Space Science Board[1] and the ad hoc committee convened by the Administrator of NASA in 1961.[2] In spite of these difficulties the programs in this area have been generally excellent and have, indeed, had a very beneficial effect on university programs. A brief review of the Sustaining University Program is given in the Supplement. Today, almost 2000 three-year traineeships have been establsihed under grants to 131 institutions throughout the nation and the quality of students attracted to the program has been high. Facilities grants, which enable universities to establish laboratory and working space for the many evolving interdisciplinary areas of the space sciences, have been made to 27 institutions. These laboratories are essential for entry into, or continuation of, space research in universities; funds for them could not be obtained elsewhere. Without these facilities grants the goals set forth in the Seaborg Report[3] and the Gilliland Report[4] to the President could not be achieved. Sustaining research grants have played an important role in providing support for young scholars and scientists and for rapidly striking out in new directions of research. Grants have not been entirely limited to the sciences but have included also economics and the law—subjects of undoubted relevance to the long-term effects of the space program on industry and the economy.

While the present trend in the NASA budget as a whole is toward stability it also shows a progressive decline in major expenditures for the construction of, for example, new centers and launch facilities, since most of these are now coming to completion. At the same time, there is a corresponding increase in that fraction of the budget that is devoted to scientific and technological activities. This can be expected to produce further demands on the supply of scientists and engineers and on the facilities for research.

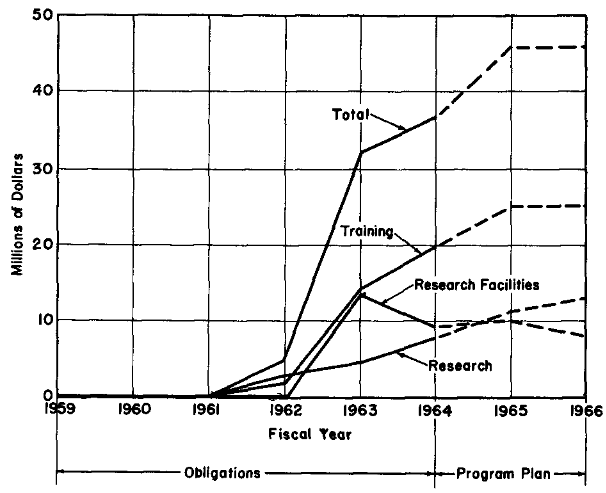

Figure 1. The sustaining university program fiscal history.

In these circumstances it is disturbing to find that the rate of growth of the Sustaining University Program is not to be maintained. We believe that there is serious doubt whether NASA will be able to achieve the aims of its charter over the next decade if these budget limitations prevail.

We re-emphasize that these university-supporting funds yield long-term results in much greater proportion than their magnitude: they sustain new sources of ideas and new generations of investigators, and contribute to improving the quality of "mission-oriented" research.

In view of the above conclusions we make the following recommendations:

Recommendation 1. The budget for training should be brought up to approximately $30 million per annum and the terms of predoctoral traineeships adjusted to approximate those of the new NSF traineeships.*

Recommendation 2. Funds for facilities grants should be increased to approximately the $15 million to $20 million level.

Recommendation 3. Sustaining research grant funding should be sharply increased by approximately a factor of 2.

Recommendation 4. A reasonable rate of growth for these components of the Sustaining University Program should be re-established for the years ahead.

The NASA is to be commended for having initiated this constructive effort and we strongly support the main elements of its program for the universities, especially noting the introduction of traineeships and facilities grants.

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* The terms of the NSF traineeships are now more attractive financially than those of NASA.

References

1. A Review of Space Research, Chapter 12. National Academy of Sciences—National Research Council Publication 1079 (1962).

2. Discussions of NASA-University Relationships, July 17 and August 14-18, 1961. (Reports of Meetings at NASA Headquarters, unpublished)

3. Scientific Progress, the Universities, and the Federal Government (Seaborg Report), Nov. 16, 1960. Government Printing Office.

4. Meeting Manpower Needs in Science and Technology (Gilliland Report), Dec. 1962. Government Printing Office.

SUPPLEMENT: THE SUSTAINING UNIVERSITY PROGRAM OF THE NATIONAL AERONAUTICS AND SPACE ADMINISTRATION

A SUMMARY COMPILED FROM OFFICIAL SOURCES

1. INTRODUCTION: PROGRAM AND HISTORY

(a) Purpose: Assure supply of people trained in aeronautical and space science and technology; alleviate shortage of research facilities; encourage broader participation of universities in NASA research programs.

(b) Administration: By Office of Grants and Research Contracts, OSSA, which is also responsible for all other dealings by NASA with universities.

(c) Components of Program:

(i) Training: mainly predoctoral; small amount of support to postdoctoral, summer institute and seminar activities.

(ii) Research facilities: building research laboratories in universities heavily engaged in work for space program.

(iii) Research: support to augment universities' ability to participate in aeronautical and space research, especially on interdisciplinary problems.

(d) History:

(i) Fiscal Summary (Millions of Dollars)

	Obligations			Budget	
Fiscal Year	1962*	1963*	1964	1965	1966
Training	1.9	14.1	19.8	25.0	25.0
Research facilities	0	13.4	9.1	10.0	8.0
Research	2.7	4.4	7.7	11.0	13.0
	4.6	31.9	36.6	46.0	46.0

* Note: There was no "Sustaining University Program" line item in the budget for these years.

(ii) Numerical—Universities participating

Fiscal Year	1962	1963	1964	1965
Training	10	88	131	142
Research facilities	6	9	12	6
Research	9	24	34	51

(e) Comparison with other NASA Obligations to Universities:

	Millions of Dollars			
	1962	1963	1964	1965 (est)
Sustaining university program	4.6	31.9	36.6	50+
Total obligations to universities		88.1	109.0	150
Ratio		0.36	0.37	0.33+

2. TRAINING

(a) Main Purpose: Production of about 1000 PhD's per annum.

(b) Procedures: Universities must apply to enter program. Selection criteria include: accreditation ratings, resources competence, suitability of approved doctoral programs, location and regional relationships. Grants are to the universities, which are responsible for administration and selection of candidates. Term of grants is 3 years: basic stipend $2400 per annum (12 months); supplementary allowance to $1000 per annum; in 1964 additional allowance to university averaged $2631 per student per annum.

(c) Scale of Program:

	1962	1963	1964	1965
Candidates Entering	100	786	1071	1275
Universities	10	88	131	142

(See Table 2 for details)

Disciplines Studied, Totals 1962–1964

	No. Trainees	Percent
Physical Sciences	992	50.6
Engineering	731	37.3
Life Sciences	157	8.0
Behavioral Sciences	70	3.8
Other	7	.3
	1957	100.0

(d) Results (to January 1965):

PhD degrees awarded

Physics	17
Engineering	9
Mathematics	8
Biology	3
Other	1
Total	38

Fate of students since graduation	
University research or teaching	24
Postdoctoral studies	5
Government labs	3
Industrial	6

(e) Postdoctoral and Related Support: Examples in fiscal year 1965: Summer fellowships, conferences and institutes at about 10 universities; post-MD training. International Fellowship Program for foreign nationals in U.S. universities; postdoctoral resident research associateships at NASA centers. Total funds $<$\$0.7 million (3 percent of the training budget).

3. RESEARCH FACILITIES

(a) Purpose: provide adequate working space at universities heavily engaged in scientific and technical activities for the space program.

(b) Criteria for awards: relevance to space program of research for which facilities are required; urgency of need; competence; universities' commitment to space research; quality of supporting facilities and staff.

(c) Scale of Program:

Fiscal Year	1962-1963	1964	1965
Grants awarded	15	12	6
Obligations (millions of dollars)	13.4	9.1	15 (est)

(See Table 2 for details)

(d) Results: Completed to date:

(i) Harvard University: Biochemical annex to cyclotron. Biological effects of ionizing radiation; Apollo studies (4500 ft^2).

(ii) Lowell Observatory: Planetary research center and Western Hemisphere repository of lunar and planetary photographs. (8600 ft^2).

(iii) Princeton University: Facilities for research on chemical, nuclear, and ion rocket propulsion (26,300 ft^2).

(iv) Rensselaer Polytechnic Institute: Materials Research Center (59800 ft^2). Joint funding with NSF and others made possible a total of 108,000 ft^2.

(v) University of Chicago: Laboratory for Space Sciences. Preparation of flight experiments; analysis of results—wide range of participation in unmanned space research program (45000 ft^2).

(vi) University of Minnesota: Addition to Physics Laboratory. Atmospheric, solar, cosmic-ray physics (17400 ft^2).

4. RESEARCH

(a) Purpose: long-term support of broad programs in research related to aeronautics and space science and especially to encourage interdisciplinary research. It is intended to complement but not compete with the project-oriented research supported by NASA.

(b) Procedures: proposals are selected for support on basis of quality and scientific merit of program, NASA's long-range scientific goals, other research in the university, secondary effects (e.g., training), effects on the university's research capabilities, the university's intentions to support the program and research staff. Principal investigator selects specific tasks. Fiscal support is normally spread over several years.

(c) Scale of Program:

Fiscal Year	1962	1963	1964	1965
Universities participating	9	24	34	51
Funds (millions of dollars)	3.5	5.6	7.2	11.0

Some details of the program for fiscal year 1962 and 1963 are given in Table 4.

(d) Results: The following excerpt (Budget Estimates 1966, Vol. II) mentions some of the achievements of the program.

> At the larger institutions, grants under this program have provided a broad base of support to NASA-sponsored research projects, lending additional strength and breadth to these project efforts and permitting better long-range planning on the part of the institution. At UCLA this program provided support for the development of a project for the collection of meteoroids in space, and has supported the design and fabrication of a unique testing chamber for spacecraft magnetometers. At the California Institute of Technology, the Graduate Research Center of the Southwest, and the University of California at Berkeley, grants have supported fundamental research which has resulted in the development of new flight experiment designs and new opportunities for the interpretation of data acquired from flight experiments. The University of Maryland Computer Center has developed new techniques in programming space science information. The University of Pennsylvania, through support provided by the Sustaining University Program, has created a unique capability in the development of unconventional power sources; this multidisciplinary effort draws upon the talents and skills of the many disciplines within the university. At the University of Washington (Seattle), a broad multidisciplinary program of ceramics research has been initiated.
>
> In several instances the impact of these grants upon the university has been extensive. At William and Mary the grant was instrumental in supporting a new PhD program in physics, while at several other institutions participation has resulted in large matching contributions from the university and private donors. These are examples of how this broad, flexible, and long-range form of support can stimulate the strengthening and development of selected institutions.
>
> At universities where prior involvement in space research has been minimal, several projects have developed to a point where they have succeeded in obtaining

support on their individual merits, either from other NASA program offices or other agencies. Thus, these grants have served to provide a basis for new researchers and have provided them with an opportunity to develop their work to a point where it can attract sufficient attention to be recognized and independently supported. In addition, by virtue of the stabilized, long-range funding provided under the grant, these schools have been able to hold and attract staff members who might have moved to other institutions more capable of providing research opportunities.

Reviews of the publications and progress reports by interested NASA program offices and the scientific community indicate that the smaller schools can be counted upon to make outstanding contributions to the space program if given the opportunity to participate. Examples of such work are materials research at the University of Denver, astrophysics at Montana State College, and studies of the planet Jupiter at the University of Florida. Engineering research in structures at Texas A&M has attracted considerable attention, and at William and Mary significant progress has been made toward the eventual utilization of the 600 MeV synchrocyclotron under construction at the Langley Research Center for fundamental research in high-energy physics.

Table 2. Summary of Training Program

	No. of Trainees			Dollars		
	1962	1963	1964	1962	1963	1964
ALABAMA						
Alabama, University of		10	10		192,000	192,000
Auburn University		10	10		147,600	176,900
ALASKA						
Alaska, University of			3			57,600
ARIZONA						
Arizona State University		6	6		79,200	106,200
Arizona, University of		10	10		177,000	177,000
ARKANSAS						
Arkansas, University of		8	8		144,000	144,000
CALIFORNIA						
California Institute of Technology		15	15		268,900	268,900
California, University of Berkeley			15			237,000
California, University of Los Angeles	10	10	15	132,000	128,700	288,000
California, University of Riverside			2			35,400
California, University of San Diego			6			96,500
Southern California, University of		8	10		152,400	193,000
Stanford University		12	15		230,800	310,500
COLORADO						
Colorado School of Mines			3			39,900
Colorado State University		6	6		91,800	86,500
Colorado, University of		10	10		176,600	182,200
Denver, University of		6	6		100,800	100,800

Table 2 (cont.)

	No. of Trainees			Dollars		
	1962	1963	1964	1962	1963	1964
CONNECTICUT						
Connecticut, University of		6	6		106,900	116,800
Yale University		10	12		205,500	248,400
DELAWARE						
Delaware, University of		6	6		109,600	109,600
DISTRICT OF COLUMBIA						
Catholic University of America		10	10		165,900	192,800
George Washington University		6	6		109,800	109,800
Georgetown University			6			106,200
Howard University			4			81,600
FLORIDA						
Florida State University		8	8		131,200	128,700
Florida, University of		10	10		177,000	177,000
Miami, University of			4			70,800
GEORGIA						
Emory University			2			38,600
Georgia Institute of Technology	10	12	15	192,000	230,400	288,000
Georgia, University of			8			153,600
HAWAII						
Hawaii, University of			3			53,100
ILLINOIS						
Chicago, University of	10	15	15	222,000	310,500	310,500
Illinois Institute of Technology		10	10		188,700	191,800
Illinois, University of		12	15		212,400	265,500
Northwestern University		10	12		187,200	206,000
INDIANA						
Indiana University		10	10		143,100	192,400
Notre Dame, University of		8	8		153,600	153,600
Purdue University		12	15		223,200	279,000
IOWA						
Iowa, University of	10	10	15	169,500	175,500	264,200
Iowa State University		10	12		171,000	205,200
KANSAS						
Kansas State University		8	8		142,200	142,200
Kansas, University of		8	8		143,300	143,000
KENTUCKY						
Kentucky, University of			8			153,600
LOUISIANA						
Louisiana State University		8	8		131,500	127,700
Tulane University		8	8		158,400	158,400
MAINE						
Maine, University of			4			76,800
MARYLAND						
Johns Hopkins University		8	10		156,000	195,000
Maryland, University of	10	10	10	192,000	192,000	192,000
MASSACHUSETTS						
Boston College			3			61,200

	No. of Trainees			Dollars		
	1962	1963	1964	1962	1963	1964
Boston University			6			106,200
Brandeis University			6			106,200
Clark University			2			29,200
Massachusetts Institute of Technology		15	15		310,500	310,500
Northeastern University		3	4		50,900	68,500
Tufts University			3			58,800
MICHIGAN						
Michigan State University		8	10		144,000	192,000
Michigan, University of	10	15	15	177,000	265,500	279,000
Wayne State University			5			80,000
MINNESOTA						
Minnesota, University of	10	15	15	192,000	288,000	288,000
MISSISSIPPI						
Mississippi State University			4			65,300
Mississippi, University of			4			70,800
MISSOURI						
Missouri, University of		8	10		152,900	191,000
Missouri School of Mines and Metallurgy		4	5		76,400	95,500
Saint Louis University		8	10		140,400	175,500
Washington University		8	10		154,800	193,500
MONTANA						
Montana State College			4			76,500
Montana State University			3			57,600
NEBRASKA						
Nebraska, University of			6			104,400
NEVADA						
Nevada, University of		3	4		40,600	50,200
NEW HAMPSHIRE						
Dartmouth College			6			109,800
New Hampshire, University of			4			77,000
NEW JERSEY						
Princeton University		12	12		230,400	196,600
Rutgers—The State University			10			162,600
Stevens Institute of Technology		6	6		108,000	108,000
NEW MEXICO						
New Mexico State University			8			158,400
New Mexico, University of		6	8		113,900	153,600
NEW YORK						
Adelphi University			3			57,000
Alfred University			2			26,400
Brooklyn, Polytechnic Institute of		12	15		216,000	283,500
Clarkson College of Technology			3			62,100
Columbia University		12	15		228,000	275,500
Cornell University		10	12		177,000	212,400
Fordham University			3			53,100
New York, The City University of			4			76,800

Table 2 (cont.)

	No. of Trainees			Dollars		
	1962	1963	1964	1962	1963	1964
New York, State University of,						
at Stony Brook			2			31,100
New York University		12	15		220,600	258,800
Rensselaer Polytechnic Institute	10	10	15	177,000	192,000	302,200
Rochester, University of		10	10		183,000	186,000
Syracuse University		8	8		141,600	141,600
NORTH CAROLINA						
Duke University		10	10		177,000	177,000
North Carolina State College		10	10		142,000	147,000
North Carolina, University of		10	10		132,000	186,300
NORTH DAKOTA						
North Dakota State University			2			37,900
OHIO						
Case Institute of Technology		10	10		203,700	206,700
Cincinnati, University of		8	8		141,100	117,600
Kent State University		2	3		25,500	54,100
Ohio State University		8	8		112,000	120,000
Ohio University			2			34,100
Toledo, University of			4			60,000
Western Reserve University		8	8		141,600	141,600
OKLAHOMA						
Oklahoma State University		10	10		120,000	162,000
Oklahoma, University of		10	10		177,000	177,000
OREGON						
Oregon State University		8	8		149,400	149,500
PENNSYLVANIA						
Carnegie Institute of Technology		10	12		169,500	225,100
Lehigh University		8	8		153,600	153,600
Pennsylvania State University		10	12		177,000	212,400
Pennsylvania, University of		10	12		192,000	230,400
Pittsburgh, University of		10	12		180,000	217,600
RHODE ISLAND						
Brown University			10			180,000
Rhode Island, University of		4	4		70,800	70,800
SOUTH CAROLINA						
Clemson College		4	5		75,600	95,300
South Carolina, University of			5			97,100
TENNESSEE						
Tennessee, University of		8	10		141,600	177,000
Vanderbilt University		8	10		153,600	179,300
TEXAS						
Houston, University of		10	10		177,000	177,000
Rice University	10	10	15	192,000	192,000	288,000
Southern Methodist University			3			54,000
Texas A&M University	10	10	12	220,000	192,000	230,400
Texas Christian University			3			51,900
Texas Technological College		6	6		101,600	101,600
Texas, University of		10	12		132,000	158,400

Table 2 (cont.)

	No. of Trainees			Dollars		
	1962	1963	1964	1962	1963	1964
UTAH						
Brigham Young University			4			40,800
Utah State University		4	4		56,000	73,200
Utah, University of		8	8		141,600	141,600
VERMONT						
Vermont, University of		3	4		48,700	67,800
VIRGINIA						
Virginia Polytechnic Institute		10	10		177,000	177,000
Virginia, University of		10	10		153,400	163,700
WASHINGTON						
Washington State University			6			100,700
Washington, University of		10	10		150,200	148,800
WEST VIRGINIA						
West Virginia University		8	8		143,100	151,800
WISCONSIN						
Wisconsin, University of		10	12		183,00	223,200

Table 3. Summary of Research Facilities
(March 1, 1965)

Institution	Investigator/Topic	Area (sq.ft.)	Amount
Fiscal Year 1962			
RPI	Wiberley/Materials research	59,800	$ 1,500,000
Stanford	Lederberg/Exiobiology	14,500	535,000
Chicago	Simpson/Space sciences	45,000	1,775,000
Iowa	Van Allen/Physics and Astronomy	24,000	610,000
California (Berkeley)	Silver/Space sciences	44,100	1,990,000
Harvard	Sweet/Biomedicine	4,500	182,685
Fiscal Year 1963			
Minnesota	Nier/Physics	17,400	704,000
MIT	Harrington/Space sciences	75,000	3,000,000
Colorado	Rense/Astrophysics	31,800	792,000
UCLA	Libby/Space sciences	68,500	2,000,000
Wisconsin	Hirschfedler/Theoretical Chemistry	12,000	442,760
Michigan	Norman/Space sciences	56,000	1,750,000
Pittsburgh	Halliday/Space sciences	47,300	1,500,000
Princeton	Layton/Propulsion sciences	26,300	625,000
Lowell Observatory	Hall/Planetary sciences	8,600	236,520

Table 3 (cont.)

Institution	Investigation/Topic	Area (sq.ft.)	Amount
Fiscal Year 1964			
Texas A&M	Wainerdi/Space sciences	34,000	1,000,000
Maryland	Martin/Space sciences	70,000	1,500,000
USC	Meehan/Human Centrifuge	4,000	160,000
Cornell	Gold/Space sciences	38,000	1,350,000
Rice	Dessler/Space sciences	68,000	1,600,000
Purdue	Zucrow/Propulsion sciences	5,000	840,000
Washington (St. Louis)	Norberg/Space sciences	24,600	600,000
New York	Ferri/Aeronautics	21,000	582,000
Georgia Tech	Picha/Space Science and Technology	50,000	1,000,000
Arizona	Kuiper/Space sciences	50,000	1,200,000
Illinois	Alpert/Space sciences	51,000	1,125,000
PIB	Bloom/Aerospace sciences	16,000	632,000
TOTAL		966,400	$29,231,965

Table 4. Sustaining University Program
Research Program

Institution/Project	Amount/ Duration	Annual Level of Effort
FISCAL YEAR 1962		
California (Berkeley)/NsG-243 Multidisciplinary Space Sciences	500,000/3 yr	250,000
California (L.A.) NsG-237 Multidisciplinary Space Sciences	500,000/3 yr	500,000
Columbia/NsG-294 Materials Research	100,000/2 yr	60,000
G.R.C. of S.W./NsG-269 Earth and Planetary Sciences	923,000/3 yr	500,000
Kansas/NsG-298 Development of Space Science Research	100,000/3 yr	50,000
National Academy of Sciences/NsG-252 Space Science Summer Study Program at State University of Iowa	154,000/1 yr	154,000
National Science Foundation/R-62 13th Assembly of the International Union of Geodesy and Geophysics	40,000/1 yr	40,000

Table 4 (cont.)

Institution/Project	Amount/ Duration	Annual Level of Effort
Ohio State/NsG-295 Biological effects of gases low in nitrogen	58,000/3 yr	25,000
Pennsylvania/NsG-298 Unconventional Techniques of Energy Conversion	250,000/3 yr	75,000
Texas A&M/NsG-239 Development of Space Sciences Research	100,000/3 yr	50,000
Wisconsin/NsG-275 Theoretical Quantum Chemistry	700,000/3 yr	400,000
	$3,424,000	$1,825,000

FISCAL YEAR 1963

Institution/Project	Amount/ Duration	Annual Level of Effort
Adelphi/NsG-394 Development of Research in Space Sciences	76,920/1 yr	76,000
Alabama/NsG-381 Aerospace Sciences	600,000/3 yr	300,000
*California (Berkeley)/NsG-243 Multidisciplinary Space Sciences	140,000 (SC) 400,000 (AFE) 540,000 total/3 yr	400,000
*California (L.A.)/NsG-237 Multidisciplinary Space Sciences	400,000/3 yr	200,000
Cal Tech/NsG-426 Space Physics, Lunar and Planetary	260,000 (SG) 620,000 (SL&SG) 880,000 total/3 yr	400,000
Catholic/NsG-411 Conference in Space Plasma Physics	6,850/1 wk	6,850
Cornell/NsG-382 Atmospheric and Planetary Sciences	200,000 (SC) 200,000 (SL&SG) 400,000 total/3 yr	200,000
CUCOSS/NAS-7100 NASA/CUCOSS Liaison Office	60,000/1 yr	60,000
*G.R.C. of S.W./NsG-269 Multidisciplinary Space Science Research	425,000/3 yr	500,000
*Kansas/NsG-298 Development of Space Science Research	150,000/3 yr	100,000

Table 4 (cont.)

Maine/NsG-338 Development of Space Science Research	61,250/3 yr	30,000
Maryland/NsG-398 Computer Sciences	700,000/3 yr	350,000
Maryland/NsG-482 Conference in Space Communication	6,036/1 wk	6,036
MIT/NsG-496 Multidisciplinary Research Space Sciences and Engineering	1,512,000 (SC) 488,000 (SB&RP) 2,000,000 total/3 yr	500,000
Mississippi State/NsG-80 Biology of Closed Ecological Systems	15,000/1 yr	15,000
Montana State College/NsG-430 Space Physics Research	70,000/3 yr	35,000
NYU/NASr-167 NASA-University Liaison Activities	33,000/1 yr	33,000
*Pennsylvania/NsG-316 Unconventional Techniques of Energy Conversion	75,000 (SC) 50,000 (RP) 125,000 total/3 yr	75,000
Pittsburgh/NsG-416 Multidisciplinary Space Sciences	500,000/3 yr	250,000
Rice/NsG-6 Materials Research	50,000 (SC) 300,000 (RR) 350,000 total/3 yr	300,000
Syracuse/NsG-159 Materials Research	10,464/1 yr	10,464
*Texas A&M/NsG-239 Development of Space Sciences Research	25,000/3 yr	50,000
Washington (Seattle)/NsG-484 Multidisciplinary Ceramics Research	340,000 (SC) 60,000 (RR) 400,000 total/3 yr	100,000
Wisconsin/NsG-275 Theoretical Quantum Chemistry	370,000/3 yr	400,000
Xaviar/NsG-315 Conference on Quantum Mechanics	7,000/1 wk	7,000
	$6,093,520 (SC)	

* Continuation of fiscal year 1962 research grants.

Note: Letters in parentheses indicate joint funding with other NASA program activities.

APPENDIX 3: NASA POLICIES WITH RESPECT TO EDUCATIONAL INSTITUTIONS*

T.L.K. SMULL

There are two basic features of NASA policy with respect to its dealings with educational institutions that deserve mention. First, in the development and conduct of the NASA-University program, the one basic principle underlying all NASA policy regarding its relationships with universities is that NASA wishes to work within the structure of the universities in a manner that will strengthen the universities and at the same time make it possible for NASA to accomplish its mission. While we are anxious to reap the benefits to be gained by developing research potential in the universities, we want to support research in the traditional atmosphere of instruction and learning from research that results from keeping the research activities surrounded by students. We are keenly aware of the need for an ever-increasing supply of highly trained personnel if we in NASA, and in fact the Nation as a whole, are to carry out our goals successfully and reap the maximum benefits of the Nation's space program. We are not interested in the creation of institutes that tend to draw university faculty away from the educational aspects of their research. The university is the only segment of the team undertaking this space program that produces manpower. The other two partners in this enterprise—industry and government—only consume manpower. It is for this reason that NASA hopes to conduct its joint activities in a manner that will preserve and strengthen the universities' educational role. This basic policy is interwoven in the policies and procedures of NASA's support of training, research, and research facilities.

The other basic policy is that of striving, wherever possible, to assure the long-term funding that is so essential to the successful conduct of research. We have pioneered, within NASA, the use of a funding mechanism which has become known as either step funding or forward funding in a manner that is intended to give stability to those university programs that are known to be of several years' duration. The pattern of this type of funding is shown in Figure 2. Under this arrangement funds in the amount of 100 percent of the agreed level of effort are made available during the first year. Funds in the amount of two thirds of the agreed level of effort are programmed to be paid during the second year and one third of the agreed level of effort would be paid during the third year. When the initial grant is made, these funds are all set aside by NASA and are paid to the university on demand from the university on a quarterly

* Excerpted from, The Nature and Scope of the NASA-University Program, by T.L.K. Smull, Director, Office of Grants and Research Contracts (NASA SP-73, 1965), pp. 10-12.

basis. During the course of the investigation, based upon a semiannual review, NASA will supplement the grant annually with a grant of funds in the amount of the agreed-upon level of effort. These supplements are scheduled to be paid in accordance with the university's demand over a 3-year period, as indicated in the figure. In this manner, the university always has funds coming in for two additional years, at a reduced rate, should NASA decide to withdraw its support or Congress fail to appropriate funds for this purpose. This permits the university to dissipate any obligations which it may have incurred in an orderly manner over a 2-year period. Although this type of funding is not appropriate for all research, it is desirable for the greater part of research activities that NASA supports because it creates stability and thereby increases research productivity. Every effort is made, when appropriate, to use this funding technique.

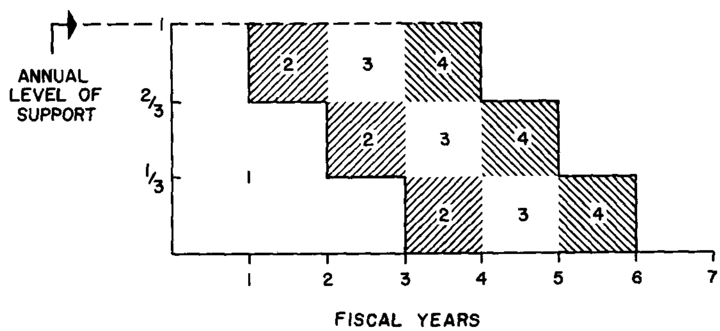

Figure 2. Step funding of research support.

IX

Biology

INTRODUCTION, FINDINGS, AND RECOMMENDATIONS

The Working Group on Biology considered the following subject areas to be within the context of their Summer Study objectives:

(1) Environmental biology
(2) Exobiology
(3) The role of man in space in broad programs of environmental space biology and planetary exploration
(4) Specific NASA structures, practices, and policies that pertain to support of fundamental biological objectives.

The Group confined its attention primarily to these subjects, avoiding overlap with the consideration of other Working Groups except where such overlap seemed desirable in order to appraise topics of common concern.

Significant findings and appropriate recommendations were made by the Working Group for each of the subject areas, as follows:

Environmental Biology

Recommendation 1. In-flight studies of biological effects of space radiation are of fundamental scientific importance insofar as they test for synergistic or antagonistic effects between weightlessness and radiation. The Working Group therefore recommends that experimental studies on such synergistic or antagonistic effects should be supported.

Recommendation 2. Biorhythm studies in space flight are desirable on an exploratory basis. Experiments should be carried out to determine whether biorhythms are altered in near-Earth orbit and in deep space probes beyond the Earth's magnetosphere.

Recommendation 3. Broad exploratory studies on effects of weightlessness on growth and development are scientifically desirable. The Working Group thus recommends that such studies be undertaken with a high priority and be carefully chosen so as to form an effective basis for subsequent definitive studies to learn (a) how organisms perceive gravity and (b) how they use information on gravitational field strength and direction in determining processes of growth and development.

Recommendation 4. The NASA Biosatellite Program is essential to the proper study of space biology. Hence, the Working Group recommends that the NASA Biosatellite Program be continued beyond the currently planned series. Follow-on biosatellite programs should be based on the use of a launch vehicle capable of boosting a several hundred pound scientific payload into orbit in a manner that avoids the undesirable constraints of the current program. A centrifuge should be included in at least some future biosatellite flights.

Recommendation 5. The design of in-flight biological experiments requires effective communication between experimenters and space equipment design engineers. The Working Group recommends therefore that biologists should have access to ample NASA engineering advice and aid in developing their experiments through the prototype, flight equipment, and engineering test stages. To achieve this, the personnel complement at the Ames Research Center should be increased.

Recommendation 6. The Working Group recommends that NASA should take immediate steps to develop a program to explore the biological effects of vibration. Possible biological effects of vibration during launch of a biosatellite may be avoided if organisms are sent aloft in a dormant or otherwise insensitive condition. Alternatively, breeding the test organisms during space flight would avoid the problem.

Recommendation 7. Routine tolerance testing of higher animals to space flight conditions with only "before and after" measurements has little scientific importance. Thus, the Working Group recommends that, if animals are placed in orbit to determine their tolerance to conditions of prolonged space flight, every effort should be made to acquire in-flight scientific data as well.

Exobiology

Recommendation 8. In the forthcoming unmanned and manned exploration of Mars, attention should be directed to the possibility that no extraterrestrial life exists there, in which case it will be of great scientific interest to characterize any prebiotic organic chemistry that may be present on the planet. Thus, the Working Group recommends that exobiology not be thought of as confined to the search for living organisms, but should admit for its scrutiny all stages in the evolutionary development of life on a planet, with the prebiotic phase holding special interest.

Recommendation 9. The untimely contamination of Mars with terrestrial organisms would compromise the interpretation of biological studies of Mars. Accordingly, the Working Group recommends that sterility precautions be strictly enforced in the early stages of Martian exploration, and that NASA maintain strict sterility enforcement as an announced national policy until accumulated evidence indicates that relaxation of standards will not degrade biological objectives of Martian exploration or lead to the destruction of an important resource.

Recommendation 10. It is considered essential to preserve samples of virgin Martian surface and subsurface. The Working Group recommends therefore that, for biological as well as for other purposes, NASA develop a device to encapsulate, sterilely and hermetically, samples of the Martian surface and subsurface. Such devices should be sent with an early Martian lander to collect samples for retrieval by later manned missions.

Recommendation 11. Additional data on Mars are necessary to the efficient design of biological experiments. Thus, the Working Group recommends that reconnaissance by optical images be made in early as well as advanced phases of the unmanned exploration of Mars. NASA should make a major effort to remove critical limitations on communication of data over the Earth-Mars distance.

Recommendation 12. A Mars lander, large enough to accommodate an array of integrated automated experimental sequences, is necessary to the biological exploration of Mars. Therefore, the Working Group recommends that NASA implement, as an item of great urgency, the development of an Automated Biological Laboratory (ABL), recommended by the 1964 Exobiology Summer Study. In this connection, NASA should re-examine the possible advantages, and the feasibility, of using the Saturn V vehicle for Voyager missions.

Recommendation 13. The breadth of the problems, and the cost of planetary exploration, place immense demands on science and on the nation's resources. The Working Group therefore recommends that a continuing committee, advisory to NASA but preferably outside the NASA organization, be established to monitor and to advise on the scientific program of planetary exploration.

Recommendation 14. Complete biological and other investigations ultimately will require that scientists themselves travel to Mars, after unmanned observations have been well advanced. In view of this anticipated need, the Working Group recommends that NASA continue to support research on bioregenerative life-support systems.

Recommendation 15. Since the hazard of back contamination, while probably very small is potentially catastrophic, the Working Group recommends that safeguards be incorporated into the planning of Voyager and Apollo missions. Nevertheless, quarantine regulations should be designed to be compatible with the scientific goals of the missions. Samples to be tested for biological activity must not be treated so drastically as to destroy that activity for research purposes, and provision must be made to give experimenters access to the samples as soon as possible.

The Role of Man in Space

Recommendation 16. Scientific studies of space require the participation of highly competent scientists. The Working Group thus recommends that the philosophy and the program of the Office of Manned Space Flight be broadened immediately to include trained scientists of exceptional abilities as scientist-passengers, to serve as observers and experimenters, having minimal involvement with piloting responsibilities. The program should include such observers as early as possible in the exploration of the Moon and of Mars. In the selection of scientist-passengers, NASA should emphasize breadth of knowledge and interest, general intelligence, Ph.D. or M.D. level of training, and psychological adaptability; physical qualifications (e.g., age and vision) set for pilot astronauts should not be as stringent for scientist-passengers.

NASA Procedures

Recommendation 17. The Working Group recommends stronger research support to a NASA research center in order to meet the biological needs of the space program and to (i) carry on in-house research in space-related fundamental biology, (ii) carry on program-oriented research, (iii) assign personnel able to provide engineering consultation to biologists working on space-related biological problems under NASA grants or contracts, and (iv) develop and test prototype and flight equipment in collaboration with experimenters who require engineering assistance.

Recommendation 18. The system for the review of proposals for research and for in-flight experiments is in need of formalization. Hence, the Working Group recommends that formal panels of non-NASA scientists be established to assign merit ratings, exclusively on scientific grounds, to such proposals.

ENVIRONMENTAL BIOLOGY

Three fields of basic biological research can benefit substantially by the exploitation of conditions and facilities for experimentation that are available only in space. These fields are: (1) radiation biology, (2) studies of circadian rhythms, and (3) investigations of gravitational influences in the range of G values between zero and 1.

Radiation

It is possible to simulate in ground-based laboratories most of the kinds of radiation that can be encountered in space. (Certain highly energetic heavy particles are exceptions.) Thus, the direct study of radiation biology in space does not appear to be a likely means of obtaining new biological data of fundamental interest. However, the effects of radiation on organisms do depend in some measure on interactions with other environmental factors. For example, do animals or plants exposed, under conditions of weightlessness, for long periods in the space environment respond to radiation in the same manner as they do on the Earth? Possibly not. If we extrapolate to the space environment our extensive but Earth-bound knowledge of radiation effects, it is important to ascertain the validity of the extrapolation.

Significant synergistic or antagonistic effects from simultaneously imposed stresses of radiation and weightlessness may not be expected, perhaps, but they are quite possible, particularly with large organisms like mammals or higher plants. We know that radio-sensitivity is affected by metabolism, which in turn may be sensitive to weightlessness. With microorganisms, on the other hand, the discovery of such effects would be surprising as we know of no mechanism whereby graviperception can be achieved by organisms smaller than a few microns. Even this possibility cannot be eliminated rigorously, however. Also, it has been reported that vibration can affect mitotic spindles and that weightlessness can cause chromosome breaks; accordingly, an apparent synergism between space flight effects and radiation may accrue.

While the practical effects of radiation on manned space flight may be the principal motivation for conducting biological radiation tests in space, any pronounced synergistic effect would be likely to give rise to an intrinsically interesting scientific study. From both viewpoints, a demonstration of pronounced interaction between radiation and weightlessness, which could result from the current Biosatellite Program, would be of enormous interest and would call for re-evaluation of the importance and scope of space-radiation biology.

Biorhythms

Circadian rhythms, by now observed in so many varied organisms that they are recognized to be among the most general of biological phenomena, are understood only in the descriptive sense. We do not know by what mechanisms these rhythms are maintained, entrained, or modulated. We cannot explain satisfactorily their relative independence of temperature, how synchrony is achieved, or the causes of desynchrony when that occurs. However, we have a wealth of empirical knowledge about these rhythms and how they perform important roles in the organisms that display them. Existing formal models of biorhythms describe many of their dynamic features, but in spite of the high predictive value of these models, the effort to expand further our understanding of circadian rhythms constitutes one of the most exciting challenges of present-day biology. The nearly ubiquitous character of these rhythms strongly suggests that they play important fundamental roles in the lives of most organisms. New approaches to the study of such rhythms may provide keys to their secrets.

Two factors suggest that studies of circadian rhythms in the space environment may be especially fruitful. The first stems from the observation that of all of the organisms surveyed, only among the microbes were circadian rhythms generally undetected. Is it only a coincidence that these organisms also appear to be incapable of sensing gravity, or have multicellular organisms, evolving in a 1-G environment, built into their rhythm-control mechanisms some dependence on the gravitational field that has not yet been revealed by all of our experimenting in Earth-based (1-G) laboratories? It is meaningful, therefore, to ask even the simplest question one can pose in a space experiment on this topic: Do rhythms persist in a state of weightlessness?

A different course of reasoning provides the second major justification for conducting circadian-rhythm experiments in space. One school of thought postulates that some factor associated with the Earth's rotation exerts a determining influence on biorhythms. Perhaps the only unambiguous test of that hypothesis will be to study organisms in space, where they will be removed from possible terrestrial influences. If familiar characteristics of the rhythms are exhibited in the space environment, the postulated dependence on continued terrestrial inputs would seem unlikely. Definitive answers are more apt to come from biological studies using deep space probes than from biosatellites in near-Earth orbit.

Variable G Effects

Probably the most fundamentally interesting biological experiments to be performed in space will be those involving weightlessness or effective acceleration fields of less than 1 G. There can be no doubt that abnormal G values will produce biological effects, but it remains uncertain how these effects will be displayed. This area of study should be accorded the highest priority among those proposed for satellite investigation.

At this preliminary stage, the basic questions can be stated only in general terms. How do organisms perceive gravitational-field strength and direction? How do they use G information to control their own growth and development? Initially, we may expect to measure the effects of weightlessness on growth, development, and organization of both plant and animal material. The first studies will necessarily be largely exploratory. As we learn what effects to expect, more specific experiments can be designed to investigate in depth those directions that prove to be of greatest interest.

The scientific justification for such studies is our awareness, on the one hand, that multicellular organisms sense gravity and use it to influence growth and development, and, on the other, that our understanding of the biological mechanisms involved is distressingly incomplete. This has long been a challenge of fundamental interest to biologists. The space environment offers a new laboratory for research on problems of graviperception and development. Studies of growth and differentiation of seedlings seem most promising in the investigation of G effects on plants, while studies of fluid distribution and of central nervous systems deprived of gravitational sensory input are areas of interest with respect to animals.

Biosatellites

The Working Group also reviewed the current NASA Biosatellite Program, a follow-on program proposed by NASA, and a somewhat similar program proposed by the U.S. Air Force. Detailed critical review of the current Biosatellite Program was attempted because it was believed that the identification of any weaknesses or strengths would be helpful to possible future biosatellite efforts.

A serious drawback relating to the participation of life scientists in biosatellite experimentation has been the time lag between submission of a proposal and execution of the experiment. This has discouraged many biologists who might otherwise have submitted experimental proposals.

The Working Group believes that the present program emphasizes those areas of research that are scientifically important. More experiments have been proposed and placed in scientific-priority Category I (selected for flight) than can be accommodated by the present flight program. The Group was unanimous in recommending that a follow-on biosatellite program be carried out not only to fly the remaining experiments but also because the Group was convinced that interest in and support of environmental biology in space research will continue to increase. Further biosatellite effort by NASA is scientifically justified.

Some problems in experimental design are evident in the present Biosatellite Program. Criticisms apply mainly to inadequate controls for on-board experiments. In the first series of biosatellite flights, for example, the investigations are intended to explore effects of weightlessness either individually or in combination with other factors. However, to be able to attribute any observed effect to weightlessness, it is important to rule out possible influences of acceleration or vibration; a way to do this adequately has not yet been developed.

Sensitivity of organisms to vibration has not been of much general biological interest in the past, and little knowledge of its effects has been accumulated. For example, radiation sensitivity, relating both to damage and repair events, could be altered by vibration, acceleration, or deceleration during launch and re-entry. In the future, we must be even more concerned with experimental designs that take into consideration the variables necessary to obtain valid control data. It may seem feasible to simulate acceleration and vibration profiles on the ground or, as is being done in the radiation tests, to locate some of the control organisms behind a radiation shield in the spacecraft itself. In fact, neither control is ideal. It may not be possible to simulate the vibration profile as precisely as necessary if the organism under test proves to be highly sensitive to vibration. Even in the same spacecraft, vibration in different locations can vary in amplitude at different frequencies.

If acceleration, deceleration, and vibration cannot be well enough controlled, and if test organisms prove to be hypersensitive, the experimental organisms might be bred entirely in space or they might be sent aloft in a dormant or hibernating state, in which they are presumably less sensitive, and activated after orbit is achieved.

Another shortcoming in the present design of biosatellite experiments is the absence of an on-board control at 1 G. This deficiency could be remedied with a centrifuge, at least for small test organisms. Moreover, only by means of an on-board centrifuge (or by spinning the entire vehicle) can G values between zero and 1 be attained. Thus, it is recommended that an on-board centrifuge be incorporated in future biosatellite designs, both as a control device and as a means of extending the usefulness of the biosatellite by permitting the creation of a fully variable G environment.

Some General Requirements for a Follow-on Unmanned Biosatellite Program

The Working Group agreed that the general objectives for the follow-on program should remain unaltered; specific future objectives will depend in part on findings obtained by the current Biosatellite Program. For example, if synergistic or antagonistic effects between weightlessness and radiation are revealed, they should be validated and studied further. Unless such effects are found, radiation studies should not be accorded a high priority.

If space flight is found to affect circadian rhythms, this too should be confirmed, and further experimentation in this area will warrant a high priority. If space flight does not affect circadian rhythms, studies of rhythms on low Earth orbital flights should be considered less urgent.

Principal objectives of follow-on programs should be the resolution of two fundamental questions already mentioned; in general terms these are:

(1) How do organisms perceive gravitational strength and direction?

(2) How do organisms use gravitational information to control growth and development?

The Working Group considered that an optimal follow-on program to investigate biological effects of the space environment should include: (1) a series of three or four missions (six or eight spacecraft including backups) as a probable minimum; (2) capability for 60 days in orbit, although this is probably not required for most missions; (3) recovery of most payloads (except for some missions that may be designed so as not to require recovery); (4) orbits designed to avoid the Van Allen radiation belts, with their introduction of undue complications into evaluation of experimental results; (5) a centrifuge to provide on-board control for experiments; (6) provision for on-board chemical and other analysis and for data processing prior to storage or Earth readout; and (7) a scientific payload weight of about 200 lb. Smaller payloads, such as those in the current biosatellites series, tend to be too restrictive, while larger payloads introduce increasingly serious problems of mutual incompatability among different experiments.

Tolerance Testing

As part of the NASA effort to extend the duration of man's operations in space, systematic incremental increases of exposure have been carried out. This is sound policy in environmental medicine. Without attempting a medical judgment, the Working Group noted that mere tolerance testing, whether or not it is required for practical reasons, is unlikely to provide results of basic biological interest.

How much use to make of test animals exposed, before man, to environmental stresses of prolonged space flight has been a matter of dispute in the areas of physiology and medicine. If primates are used for tolerance testing, it would be desirable and economical to obtain in-flight data of scientific value. Conversely, if experimental organisms, including primates, are flown for scientific purposes, such flights can be made to serve a program of tolerance testing as well.

The Working Group recognized that in-flight research on animals is often thought to be justifiable chiefly to provide back-up information for the man-in-space effort. This relationship, however, while important, is probably not as direct as one might think. It often has proved efficacious in environmental health research first to identify a medical problem, then to employ experimental animals best suited for a thorough investigation of this problem in a program of study aimed at discovering possible solutions that ultimately may be applied to man. Fundamental to the usefulness of this approach is the compilation, within the framework of the problem area, of techniques for the use of various laboratory animals and experiences with these animals. Thus, NASA is justified in supporting such research for its fundamental scientific interest, both as a means of providing a research technology to be used when unforeseen problems arise and as part of a program of tolerance testing if such tests are deemed necessary for practical reasons.

From this standpoint, it seems that the medical and physiological objectives of both the NASA and the USAF man-in-space programs should coincide rather well and that therefore the interests of both agencies could be served by a joint follow-on biosatellite effort.

EXOBIOLOGY

The Working Group agreed that the investigation of possible extraterrestrial life, especially on Mars, is a scientific enterprise of historic importance. It agreed with the opinions developed and documented in the 1962 Summer Study (A Review of Space Research, National Academy of Sciences—National Research Council Publ. 1079, pp. 9-6 to 9-11) and in the 1964 Exobiology Summer Study (Biology and the Exploration of Mars, National Academy of Sciences—National Research Council Publ. 1296). The question of the significance of extraterrestrial life to fundamental biology is complemented by its philosophical importance and by its broad human appeal; our scientific conception of life, our conception of the physical universe, and our thinking about our own place in the universe all must be affected by the outcome of our exploration. It is especially urgent that basic information about the indigenous life of Mars be obtained before the planet is contaminated. The Working Group believes that the exploration of Mars, with initial emphasis on the detection and characterization of possible Martian life, should constitute the major scientific goal of the United States space program in the period following the manned lunar landing.*

*One member of the Working Group felt that certain aspects of environmental biology that can be investigated using space vehicles would have biological importance at least equal to that of exobiological studies.

In planning the biological exploration of other planets, the question, "Is life present?" should be broadened to the question, "Is any stage of biological evolution observable?" This question covers the search for evidence of early stages in the evolution toward organisms (the prebiotic stage) as well as for living organisms and for the remains of extinct life. According to the prevailing views of the origin of life on Earth—views that can only be tested by planetary research—the prebiotic stage includes: the appearance of organic compounds through nonbiological processes in primitive planetary atmospheres; the amplification and diversification of this "primordial soup" toward the biochemical level by the appearance of large molecules; the appearance of specificity and catalyctic powers among large molecules; and the evolution of self-replicating macromolecules. Somewhere along the line, such biochemical systems are thought to associate into discrete systems contained in membranes. When such bodies can reproduce themselves more or less perfectly and can sustain their reproduction, they may be regarded as living cells.

One of the most interesting and challenging possibilities at this level is the appearance of living systems based on biochemistries different from that of Earth organisms. At present, we cannot know what aspects of terrestrial biochemistry are mandatory for life in its most general sense, because the process of natural selection under the particular conditions of this planet would have eliminated alternatives that were even slightly inferior. Under different conditions on another planet, the outcome of natural selection in the early phases of organic evolution could have been quite different. Even the possibility of a noncarbon biochemistry must be considered, though it is generally considered improbable. On the other hand, evolution on two somewhat similar planets in the same solar system, such as Earth and Mars, could be quite similar. Recent investigations stimulated by the prospects of Martian exploration have already demonstrated that some organisms that have evolved on Earth are capable of living under the stresses of a simulated Martian environment. Beyond the original microbial level, evolution probably proceeds by the biochemical diversification of microorganisms, the most critical event being the appearance of photosynthesis or other processes capable of supporting life after the "primordial soup" has been exhausted. The other major step to be sought is evolution toward large multicellular organisms characterized by increasingly effective specialization of function and of adaptive behavior.

The evidence of evolution prepares us also for the possibility that all life on Mars may be extinct; there is no assurance that evolutionary adaptation can keep pace with changed stresses in the environment. In our venture into exobiology we must be prepared for the possibility that the biology of a planet can be learned only through its paleontology.

Prevailing views of the evolution of life on Earth imply a fairly strong prediction that traces of some stage of organic evolution, be it only the presence of simple organic molecules, will be found on another planet, such as Mars perhaps, that is in some ways similar to ours. Totally and unambiguously negative findings on such a planet would have considerable impact on our thinking about the origin of life on Earth.

Considering the main variables of the survival of biological systems—temperature, atmospheric chemistry, surface properties—it is generally agreed that Mars is the most promising objective for efforts to detect extraterrestrial life. In many respects, our knowledge of planetary environments is not nearly sufficient to support categorical positions, however, and the Working Group believes that inadequate consideration may have been given to other planets as possible abodes of life. A search for life on Mercury or Jupiter, for example, might be warranted, and the characteristics of Venus are such that this planet may be considered more likely even than Mars to harbor life. Although recent data can be interpreted—according to the atmospheric model currently favored—as demonstrating that Venus' surface is prohibitively hot, the temperature has been measured only remotely and indirectly and we are not yet certain that this model is correct. Thus, new observations at some future date may kindle an active interest in Venus as a site for exobiological research.

Sterilization

Because recent research suggests that physical and chemical conditions on Mars may permit the proliferation of at least some Earth organisms, the Working Group concurs emphatically with previous recommendations that strict sterilization of landing capsules is essential to the scientific goal of the first landing missions. The preservation of possible biological resources should outweigh all other objectives in the exploration of Mars until the existence of life or of prebiotic material on the planet is definitely ruled out, or until it is ascertained that terrestrial organisms would not compromise or destroy the evidence of Martian biology. In connection with the second condition, it should be noted that a prebiotic state—which is of particular scientific interest—might be more sensitive to destruction by contamination than microorganisms or higher organisms would be. Thus, the stringent enforcement of sterility should be applied to the earliest landings, and the precautions should be reassessed at later stages of exploration.

As an additional safeguard for the scientific goals, the Working Group urges the development of devices for collection and sterile encapsulation of Martian surface and subsurface samples. Such devices should be flown on early Mars landing probes to collect samples on impact and preserve them for retrieval by later manned missions. This scheme, which was recommended in the 1962 study, would provide some assurance of scientific access to virgin Martian material in case of inadvertent contamination.

Means of Observation

Biological exploration of Mars should begin with unmanned landers carrying devices capable of detecting and transmitting evidence of life and prelife phenomena. The detectors should test for organic molecules, for fundamental biochemical processes such as enzyme catalysis, and for growth and metabolic activities of microorganisms, and should include photographic devices to secure evidence of microscopic and macroscopic organic structure.

The Working Group puts considerable emphasis on the special importance of visual observation in the search for extraterrestrial evidence of life. Physical and chemical means are indispensable in reporting the existence of an organic chemistry and possibly of some aspects of a biochemistry. On the other hand, the requirements of biochemical reactions on Earth and, even more, the requirements of growth and metabolism of organisms, are so discriminating that most preconceived tests can be regarded as tests for organisms reasonably similar to those on Earth. These tests are incapable of detecting organisms whose processes or requirements are very different from those of Earth organisms. It is felt, therefore, that visual observations, and especially observations of developmental changes with time, would provide compelling evidence of the existence of organisms in the vicinity of the landing, whatever the biochemical basis or physiochemical requirements of those organisms. Obviously, detailed pictures of local structure, both large scale and microscopic, would have great scientific value and would be of great interest generally, even if they did not reveal organisms.

A major difficulty is encountered in connection with the acquisition of reliable samples in the detection of life. If detector devices should fail to register evidence of life, can we attribute this to unsuccessful or inappropriate sampling or to the actual absence of life? What should constitute a valid negative result from life-detector sampling? A camera, on the other hand, has a much less difficult sample-acquisition problem. If an optical image includes direct or indirect evidence of life, and if the picture is of at least average quality, recognition is highly likely. If Martian living forms are very different from those on Earth, a detector based on metabolic characteristics or on growth behavior, for example, may not be properly designed to achieve a positive indication. Photographic evidence, on the other hand, even of unimaginably strange creatures, is likely to identify items of interest.

The foregoing remarks pertain to macroscopic life. If Mars is inhabited only by microorganisms, pictures are not likely to detect them directly. But even then, optical surveillance of the sampling operation could be of tremendous aid in reducing possible ambiguity of results from life detectors designed to find microbes. Reconnaissance using optical images should therefore play a very important role during the early as well as the advanced stages of exploration of Mars. From the standpoint of life detection alone, macrophotography to microphotography of planetary surfaces is expected to equal or surpass in value any alternative method. Thus, photography is an important element of a multiple-technique approach to planetary biological exploration—a type of approach to which the Working Group feels strongly committed. It will therefore be especially important to insure that suitable optical images can be acquired and transmitted. In response to this need and with recognition of its immediate applicability to planning for early unmanned Martian landers, the Working Group recommends that prompt attention be given to overcoming communication difficulties and that Voyager Program planning incorporate the requirement for image transmission as a high-priority item.

Automated Biological Laboratory (ABL)

The development of the Automated Biological Laboratory (ABL) would be a major and mandatory step in the biological exploration of Mars: a step that could advance us from the stage of improved estimates of the probable existence of life to definitive answers. By its very nature, the question, "Does life at any stage of evolution exist on Mars?" is <u>not</u> answerable by some single reading of a dial. It <u>is</u> answerable by <u>a</u> logical sequence of procedures involving: (a) multiplicity of samples, (b) automated assays, tests, and experiments carried out according to prearranged sequences that may be overridden or reordered by command from the Earth, and (c) correlation of all physical, chemical, metabolic, and morphologic data. An essential feature of this approach—used with the ABL—is that each test or measurement would be selected in the light of the results of earlier tests, so that only a small fraction of all possible observations need be conducted. (For example, biochemical tests would be made on samples selected because a microscope indicated that those samples contained bodies suggesting organic form.)

The small initial "one-shot" experiments projected for early Martian landings have high potential value. They could elevate speculative inferences about life to genuine probabilities. This could be true even of nonbiological observations on the planetary environment. But positive results would leave unanswered the most interesting questions on the nature of Martian life, and negative results would be ambiguous. This limitation is inherent where samples are selected blindly and preconceived experiments are performed; it would not be overcome by repetitive experiments on the same small scale.

The ABL could be recommended as the first step in unmanned exploration if it were technically feasible at present, but unquestionably it represents the mandatory second step. The ABL contains not only the first capability for decisive answers to questions about extraterrestrial biology, but it can also give a high scientific yield even if life or traces of life are absent. Such an instrument is necessarily designed for the physical, chemical, and structural examination of surface samples in fine detail. That examination is bound to be profitable to studies of the nonbiological features of a planet, reinforcing studies of the more global aspects, and would thus be of immediate value for planetary research as a whole. The Working Group again affirms and underlines the arguments for a mission that will at least allow for some visual information, and for the ABL concept as an advanced means of performing satisfactory exobiological research.

Voyager Program

The Working Group endorsed the Voyager Program as a first step in unmanned biological exploration of Mars that seems accessible to present technology. The Group acknowledges the merits of the "walk before you run" approach, and agrees that it is possible to obtain some biologically significant information from the very small Voyager payload, while affirming again the arguments for attempting to acquire at least some visual information and for the ABL concept, as discussed above. Since successful exobiological exploration depends on the integration of results from a variety of physical, chemical, metabolic, and visual observations, the value of the integrated information will rise very steeply with the complexity of the payload. Thus, NASA should strive to enlarge the Voyager payload even for the first lander. Although it has been suggested that this payload could be launched with a Saturn-IB Centaur booster, it seems highly probable that the Voyager program eventually will require a booster of much larger capacity. The possibility of initial use of the Saturn V vehicle should therefore be carefully re-examined.

Orbiter Studies

There are good biological reasons for a survey of the planet by an orbiter capable of providing high-resolution pictures of the surface over a substantial part of the Martian year. Such a survey would not obtain critical evidence of the presence of organisms unless they were quite large, but could observe evidence of alterations of the habitat by biological processes and, if conducted early, might provide a basis for the selection of likely places in which to search with a future lander. It is not yet decided whether the early (1969) Martian mission will include a long-lived orbiter equipped for photographic survey. If, for engineering reasons, it turns out that more valuable scientific information (high-resolution photographs of surface features) is attainable from a fly-by than from an orbiter, and if the 1969 opportunity is used only for a fly-by mission, there will still remain the need for orbiter reconnaissance of the surface throughout the periods of the Martian seasonal changes.

Advisory Committee

The 1964 Exobiology Summer Study recommended the establishment of a committee outside of the NASA organization to focus on the scientific content of the total Martian exploration program, to maintain and to monitor continuing contact among experimenters and engineers involved with the program, and to advise the NASA on directly related scientific matters. The Working Group agreed that only a continuing committee will be able to keep well enough informed on the course of developments for its detailed advice to be useful. Such a committee is essential to preserve the scientific objectives of Martian exploration. It should be established and begin to function as soon as possible.

Manned Planetary Missions

The manned biological exploration of Mars is a scientifically valid goal. If unmanned landings give negative or ambiguous results, the issue of possible Martian biology would not be resolved; it would then be recognized as lying beyond the limits of the sampling and testing techniques used. Such sampling could miss sparse, very unusual, or fossil traces of life. If, on the other hand, the first results are positive, then the importance of the Martian biology could become as great and as enduring as that of the Earth. Ultimate synthesis of terrestrial and Martian biologies into one science could come about only through manned investigations.

Regenerative Life-Support Systems

The Working Group approved the continuation of a broadly based research program on regenerative life-support systems (chemical, plant, and bacterial) as it is now being carried out by NASA and the USAF. A satisfactory system must be developed in time for use on manned Martian missions. The Group noted, however, that none of the systems thus far proposed has reached even the prototype stage, and that it is not possible to predict whether any will be capable of producing acceptable food.

Hazards of Back-Contamination

The Working Group agreed with the recommendations of the 1964 study group (Space Science Board Conference on Potential Hazards of Back Contamination from the Planets, February 19, 1965). Like the previous study group, the 1965 Group believes that the hazards are small but the consequences of misjudgment are potentially catastrophic. The Group agrees that safeguards should be incorporated into the Apollo operation.

Quarantine regulations should be designed to be compatible with the scientific goals of extraterrestrial missions. Samples to be tested for biological activity must not be treated so drastically as to destroy that activity for research purposes. Provision must be made to give experimenters access to the samples as soon as possible. Screening procedures for biological study and pathogenicity should be confined to modest aliquots of the samples. Thus, stringent precautions can minimize the hazard of back contamination, yet remain compatible with scientific needs for access to samples. Close consultation between the NASA and the U.S. Public Health Service will be required.

THE ROLE OF MAN

The Biology Working Group limited its consideration of this topic to those factors that bear directly on the performance of fundamental biological research in space.

The limitations imposed by the space program in weight and volume require that decisions be made repeatedly as to the relative effectiveness of experiments or sampling performed by instruments and by man. The advantages of using man increase with the need for interpretive or value judgments during the course of a mission.

The basic roles of man in the space-research program are (1) pilot, (2) scientific technician, and (3) observer-experimenter.

(1) The need for highly trained pilots or astronauts is evident, and an important part of biomedical research is concerned with the well-being of astronauts in the space environment. However, fundamental biological studies are of a different order and type from those concerned with astronaut safety. In addition, experience has shown that pilot-astronauts cannot be expected to have much time for scientific observation.

(2) The title "scientific technician" implies the performance of preprogrammed observations. Many kinds of measurement can be made automatically and telemetered to Earth, and others can be stored for later recovery. To make other measurements it may be more economical in terms of volume and design to use a human observer in accordance with a specified program that may be established before flight or altered according to the measurements resulting during flight. A technician-astronaut must be carefully trained and he must have sufficient motivation.

(3) A third role of man for scientific observation in space is to

perform tasks that cannot be adequately anticipated or automated. By trying to preprogram all details, a rigidity can result that could limit the scientific yield of the mission.

In some missions, as in the exploration of the Moon or of Mars, observational skill will be paramount; in others, as in an extended Apollo or MOL program, experimental skill is more important. Ideally, the two competences can be combined, maintaining both at high levels in the same scientist-astronaut. He must also be trained to understand the role of the pilot-astronaut and, if only in a limited way, to be able to substitute for him in emergencies. Missions should be so designed however, that the scientist can devote his principal attention to scientific duties. Pilot-astronauts must regard such scientists as working passengers who cannot be expected to take on many crew responsibilities.

Types of Biological Operations Requiring Man

There is a broad overlap in what can be done by instruments, by pilot-technician, and by scientist-astronaut. In general, automated devices will be employed unless weight and complexity indicate that it will be more feasible to use man. Examples of situations in which man is preferable are:

A. Exploratory Observations. Because conditions on Mars may permit some type of biochemistry that differs from that on Earth, keen observational skill will be needed to discover it. It cannot be predicted what may be found on the Moon and Mars that might have biological relevance, and, although much information can be obtained from fly-bys and unmanned landings, the unexpected can be reported only by trained human observers.

B. Experiments on Man. Mainly for practical reasons, but partly also because of more fundamental considerations, the need is anticipated for a continuing program of physiological and psychological studies on man in space. These involve the sum of space stresses, particularly weightlessness over long periods of time. Areas of physiological interest, such as those listed below, are mainly the same as those for shorter periods: (i) cardiovascular physiology, particularly vasomotor tone, venous return, and cardiac output; (ii) optomotor responses, subjective orientation, visual discrimination; (iii) postural reflexes, effects on the central nervous system of deficit of proprioceptive input; (iv) metabolic alterations such as those that can be studied best by repeated blood and urine analysis; and (v) functions dependent on cell division, such as hematopoiesis in which a synergism between weightlessness and radiation is possible. The scientist-astronaut (passenger) is the best choice as the subject of physiological experiments because of motivation, experience, and freedom from competing duties. Since the number of available subjects is necessarily small, crew members other than the scientist should also be employed for this purpose. Special training of the astronauts will be necessary, as will full appreciation by scientists of those several factors that limit the usefulness of astronauts as experimental subjects.

C. Experiments on Organisms Other than Man. As knowledge increases, the kinds of study made in the space environment will become more sophisticated and the need for trained human experimenters will increase. Specific areas of experimentation that will utilize vehicles to study basic biological problems were treated above, in the section on environmental biology. The following discussion reviews the scientific reasons for the use of scientist-astronauts for experiments on animals and plants:

1. Experiments on Mammals Other than Man. Long-term tolerance studies will be difficult to interpret and of relatively little significance unless numerous physiological measurements are made during the state of weightlessness. These measurements will include blood and urine tests (chemical), microscopic examination of blood, and function tests that cannot be satisfactorily automated. Also, it may be expected that effects on growth and development will be found that cannot be seen in adult animals; hence, it will be necessary that animals be bred and reared in space. Breeding and maintenance of even a small colony of rodents can best be done by a scientist-astronaut, who can observe unexpected effects and can provide care as needed. Complex measurements of circulatory and muscular function that cannot be done safely on man, especially those requiring catheterization, should be done on mammals by scientist-astronauts.

2. Experiments on Nonmammalian Organisms. Simple manipulations, such as fixation of cells for later cytological study and photography of growing plants, can be done by instruments. However, microscopic observation in space vehicles requires preparing and mounting the objects, focusing, and visually identifying structure. Microscopy has never yet been fully automated on Earth. In some microscopic preparations fixation and staining will also be needed. It is probable that in the weightless state, more basic information will be obtained on cellular functions in developing organisms than in adults. Germination of seeds and fertilization of sea-urchin or frog eggs, for example, can be automated, but sophisticated observations on the course of their development require the judgment of a trained biologist. Another type of experiment for which man will be particularly useful is in biorhythm tests requiring manipulations on the organism. For example, transplantation in space of organs such as the corpora allata in cockroaches may be needed to localize environmental effects on rhythmic phenomena at the tissue or organ level.

3. Martian Observations. After a manned landing on the Martian surface, the elaborate automated laboratory technology that ought to have been brought to an advanced state by that time will be used by man rather

than replaced by him. Man's greatest contributions will be to shorten the control loop by being closer to the Martian laboratory and to aid in the general problem of sampling.

Vehicle Size

The programs now visualized of experiments on man himself, or on other animals and plants, can be accomplished with only a modest increase in vehicular capability. The principal need is for more space. A very large manned orbiting laboratory (with crew of 10 or more) could be employed but would not be required for the studies envisioned. The Extended Apollo system seems to be all that is necessary for these biological experiments involving scientist-astronauts.

Criteria of Choice Between Automation and Manned Observation

The question of where to place man in the framework of scientific observation has no general answer, and the final decision in each situation must be based on the balance of numerous criteria. Size of instruments and power requirements for automation must be balanced against the size of a man and his life-support system. The complexity of the experiment and nature of the information desired must be balanced against the flexibility, motivation, and judgment of a man.

In recording, storing, and transmitting bits of information, man is clearly inferior to machines. For certain kinds of preprogrammed observations such as photography, optical measurements, pressure determinations, and some chemical measurements, and also for simple manipulations such as histological fixation and animal feeding, instruments are superior or equal to man.

At the level of complexity above mere information transfer, remote control devices can be perfected, although they may be limited by the telecommunication system. One proposal of the 1962 study has not been explored adequately for space application, namely, the development of a telepuppet. This is an extension of remote handling of equipment: it is a machine that mimics the actions of man. It is controlled by a human operator whose movements are reproduced by the telepuppet in real time.

Design of an experiment or observation so that it can be conducted by automation may impose such restrictions that the biologist is discouraged from making important proposals for flight programs. Most scientists are accustomed to designing experiments to be performed at the laboratory bench rather than remotely; for space research, however, it is desirable that flight experiments be designed so that man, whenever possible, can carry them out using more or less familiar procedures.

Insistence on minimal packaging may provide economy of payload, but it may be unrealistic if it should reduce the possible number of measurements such that the value of the experiments is degraded and the potential of the investigator is not fully exploited. Considerations of weight and volume may have to be balanced against scientific excellence. Closer cooperation between the scientific investigator and the engineer, and between the investigator and the astronaut, will be needed. It is patently important that both the engineers and the members of the astronaut team clearly understand the meaning of each scientific experiment on a given mission.

Selection and Training of Scientist-Astronauts

A first assumption is that most scientific missions in space, whether of exploration or experimentation, will be designed by scientists who do not themselves make the flight. Hence, it is necessary, when a decision to use human observers is made, to have men of the highest scientific ability. Perceptiveness, ingenuity, and scientific originality are more important qualities than quick thinking and short reaction times for trained responses. Motivation and breadth of scientific background are equally important. The scientist-astronaut must be of outstanding caliber, at the Ph.D. or M.D. level.

The selection program should be broadened to include the scientist-passenger. Physical requirements should be consistent with the performance of the required tasks, but sufficiently elastic to include men of superior ability even though they may not meet the standard requirements for astronaut selection, such as those for height and vision. Intellectual and psychological qualifications are more important than physical qualifications except, of course, for those minimum physical requirements related to resistance to the stresses of space flight.

NATIONAL AERONAUTICS AND SPACE ADMINISTRATION PROCEDURES

Role of a NASA Center for Biological Research

The Working Group endorsed the idea of using a NASA Center (e.g., Ames) as NASA's center of in-house research and engineering competence in biology. The Group believes that this center should evolve as a leading institution where able resident and visiting experimenters may conduct research on biological problems directly or indirectly related to the broad objectives of NASA programs. It is hoped that the center will thus acquire a reputation for excellence that will attract capable scientists—creating an atmosphere that will in some measure be autocatalytic. The center should also be able to recruit capable junior investigators as it should be able to provide excellent experimental facilities and an atmosphere conducive to research.

Although the Working Group did not have sufficient information to evaluate in detail the circumstances that have prevailed at NASA's Ames Research Center, it was noted that the Center has not yet achieved a reputation among biologists for uniform scientific excellence throughout its broad range of biological activities. However, it is apparent that progress has been made and the Group is optimistic regarding the future of this important NASA activity. The Working Group recommends that support for biology at Ames be continued, based on the view than an in-house center of biological competence has a significant role in the NASA programs in biology.

One function that has properly been assigned to the Ames Center is the development of the Biosatellite capsule and experimental packages, and their integration. Development of prototype experiments and of the capsule has, for the most part, been subcontracted to industry owing to insufficient facilities and personnel at Ames. To accommodate experimenters who require such help, facilities for prototype construction and engineering consultation should be made available at a NASA Center. It seems worthwhile to formalize the function of an engineering group, as a general practice, to that of working closely with the experimenter to insure that the experiment is developed and tested appropriately from the engineering point of view. The necessary blending of the scientific and the engineering aspects of an experiment will not always be achieved without a formal mechanism. Additional personnel in the biosatellite effort would be required for this purpose.

The Working Group urged that relations between the experimenters and the Ames Center, and between the experimenters and the Ames subcontractors, be re-examined. Some communication problems have arisen that are cause for concern. When experimenters and equipment contractors cannot deal with each other directly on matters of experimental equipment design, but instead must channel all communication through Center personnel, unnecessary delays and frustrations ensue. The problem could be alleviated, or solved, by a redefinition of responsibilities.

In general, closer cooperation between scientific investigators, engineers, and astronauts is suggested. It is important that the engineers and the astronauts understand the meaning of each scientific experiment on a given mission.

Procedures for Screening Proposals

The Working Group on Biology urges that NASA adopt the use of formal panels of consultants to screen for scientific value proposals for both research grants and in-flight experiments. The panels should consist of non-NASA scientists chosen for their experience and reputations within their fields of interest. The Group's recommendation is based on the following considerations:

(1) Current screening procedures appear to be, in part, a matter of in-house subjective choice, with respect to nonagency opinion. A clearly defined policy for objective outside review and evaluation is desirable.

(2) The interdisciplinary character of space research makes outside review panels particularly appropriate for the evaluation of scientific proposals. Regardless of the competence of the in-house personnel who make the screening decisions, these few individuals cannot possess the diversity and intensity of training required to review critically proposals for experiments in all phases of biology and biophysics. The custom of choosing informally the outside assistance needed for program evaluation yields advice that varies in levels of competence and of responsibility.

(3) While evaluation of proposals for funding is the responsibility of NASA administrators, they should avail themselves of the best possible scientific advice. The scientific validity of a proposal may be best judged by a disinterested scientist who is not involved with its pertinency to mission objectives or to other matters of strictly administrative concern.

(4) It will be noted that both external research grants and in-flight experiments are recommended for panel review. The Working Group could see no difference between the two with respect to the type of judgments required as to scientific worth. Indeed, NASA might wish to consider the advisability of using outside review panels to evaluate in-house research programs as well—a procedure employed effectively by the Atomic Energy Commission, for example.

The Simpson Committee (an *ad hoc* committee of the Space Science Board, NAS-NRC), in its report, "University Research, Graduate Training and the Space Program" (see Chapter 8, this report) noted that the reactions of the academic community to NASA programs were often hostile and uninformed. The existence of formal tenured screening panels, composed of respected members of the academic community, would serve as a relay for information on rationale and programs, to mitigate uninformed hostility, and as a means to engender and capture the interest and participation of able men in the space program.

APPENDIX: LIST OF PARTICIPANTS

WORKING GROUP ON BIOLOGY

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X
Medicine and Physiology

1. SUMMARY AND RECOMMENDATIONS

TASK AND COMPOSITION OF THE GROUP

Man's ability to function effectively in space for at least 8 days has now been demonstrated. To determine whether knowledge and technology are or can become sufficient for flights lasting from 30 to 1,000 days was the principal objective of the Working Group on Medicine and Physiology. A corollary objective was to identify the research necessary to accomplish a manned planetary mission within the next two decades. The Working Group was asked also to comment on the scientific roles of man in space and on basic research in medicine, physiology, and the behavioral sciences that may be carried out in space.

The Working Group on Medicine and Physiology, was under the chairmanship of Loren D. Carlson of the University of Kentucky Medical Center, and included 27 participants from universities, educational foundations, private industry, government, and the biomedical divisions of the National Aeronautics and Space Administration and the U.S. Air Force (see list of participants, Appendix 1). The Working Group was in session from June 20 through July 6, 1965. Briefings and discussions were conducted during the first week. Discussions were continued, and a preliminary draft of the Working Group's report was written during the second week.

GENERAL REVIEW OF THE STUDY

The Working Group began its sessions with a review of the medical and behavioral findings from the U.S. and Soviet manned space flights. Information available from ground-based and aircraft experience was then appraised. The review revealed gaps in biomedical knowledge that could affect the progress of the manned program. Critical problem areas were identified, and panels were assigned to evaluate them. Future manned and unmanned programs were then studied to determine whether the biomedical experiments planned can be expected to secure the necessary data at a rate commensurate with the development of other space technology. The programs studied include Gemini, Apollo, proposed Apollo Applications Program, U.S. Air Force Manned Orbiting Laboratory, Biosatellite, and ground-based research. Finally, man's scientific role in space missions was evaluated, with emphasis on biomedical investigations that he may perform in space.

CONCLUSIONS

The conclusions reached by the Working Group on Medicine and Physiology are as follows:

(1) The probability that man can be supported on prolonged space missions at an acceptable physiological and behavioral cost is sufficiently high to permit favorable consideration of such missions.

(2) It is anticipated that medical, physiological, and behavioral knowledge essential to the success of prolonged manned space missions can be obtained from a series of manned flights of increasing length, coupled with systematic in-flight and ground-based biomedical research, in time to allow for final system development of a manned planetary mission by 1980-1985.

(3) The Mercury, Gemini, and Apollo programs were, and are, primarily engineering missions not intended to produce biomedical data of general predictive value for use in the design of advanced systems.

(4) Life-support systems currently in use or in advanced stages of development are inadequate for use in prolonged flights. They are deficient with respect to cabin atmosphere, waste management, humidity control, food handling, and, in some respects, biomedical data collection.

(5) Special study is required on physiological and behavioral processes that respond to stresses slowly with time and are likely to be of increased importance as flight duration increases. Among processes of particular interest are weight loss, cardio-vascular response, bone and muscle metabolism, red-blood-cell concentration, blood-clotting mechanisms, and long-term decrements in performance.

(6) Insufficient attention is given to behavioral investigations in present flight programs.

(7) The degree to which ground-based simulations of weightlessness reproduce the effects of actual weightlessness has not been established.

(8) Scientists and technologists of many disciplines will be involved in future manned flights. Thus crew composition and, in turn, crew selection,

task assignment, and training practices must be continually re-evaluated and adjusted to meet changing requirements.

(9) The success of the biomedical program would be enhanced if biomedical scientists of high competence were attracted to and retained in full-time scientific effort.

RECOMMENDATIONS

Recommendation 1. Because of the emphasis on engineering aspects of the current space programs, biomedical data and systems have not kept pace with vehicle technology and are, in some instances, deficient. A series of manned flights specifically designed to study the biomedical problems of prolonged space flight must therefore be provided.

Recommendation 2. A series of manned orbiting research laboratories is necessary to conduct the medical, physiological, and behavioral research required for prolonged space flight up to 1,000 days. Such laboratories are necessary to test life-support systems and to determine the validity of ground-based simulations. Present spacecraft allocated to the manned program are probably not capable of performing all the necessary research. Orbiting laboratories should be designed to accommodate at least six to eight men and should have ample working space for experimentation.

Recommendation 3. It is urgent that life-support systems be improved for long-term flights.

Recommendation 4. The supporting ground-based research program must be broadened and accelerated and should include study of the fundamental characteristics of the medical, physiological, and behavioral problems of prolonged space flight.

Recommendation 5. A study should be initiated to determine the best means of attracting specialists in bioastronautics to the NASA life sciences staff and training and retaining them.

Recommendation 6. The number of scientist-astronauts should be increased, and the classification "scientist-passenger" should be introduced.

Recommendation 7. Results of research on the biomedical aspects of space flight should be consistently and promptly published in appropriate scientific journals.

2. STATE OF MEDICAL, PHYSIOLOGICAL, AND BEHAVIORAL KNOWLEDGE

In space flight, man is subjected to a series of heavy and unaccustomed stresses: weightlessness, radiation, acceleration, vibration, confinement, sensory deprivation, artificial atmospheres, inactivity, reduced mobility, and silence. Some, like acceleration and vibration, occur rarely, but most are everpresent. The short-term effects of these stresses have been handled successfully in the U.S.S.R. and U.S. manned orbital flights. Man not only has survived but has returned with relatively modest disturbances. Although the physical performance and behavior of the astronauts were not adequately measured in flight, no significant debilitating medical problems were encountered.

The long-term effects of space-flight stresses have not, however, been experienced directly, and, until they are, man's responses will have to be a matter of speculation. A substantial body of information on man's responses to most of the stresses has been accumulated by experimentation on the ground and in aircraft. The relevance of these data to actual space flight cannot, of course, be certain. Nevertheless, they have formed the basis for cautious predictions about man's responses, and, against this background, the U.S. and U.S.S.R. flights have produced few surprises. Effects have been as anticipated. These experiences encourage the expectation that a series of flights of increasing length, coupled with a well-planned integrated program of ground-based and in-flight investigations, will permit the success of manned missions of substantial duration.

Complacency concerning prolonged flight, however, is by no means justified at this time. Reliable data are limited or nonexistent in several significant areas: 1) the behavior of physiological and behavioral systems that respond slowly with time, such as metabolism and smooth-muscle mass; 2) the extent to which physiological degradation or "deconditioning" may occur over an extended period of time; 3) the ability of man to adapt to the space environment, to attain a steady state of physiological and psychological adjustment, or, subsequently, to readapt to gravity and other planetary stresses; and 4) the possibility or likelihood of a combination of stresses producing a response greater than the sum of the responses to individual stresses. Finally, it cannot be ruled out that the space environment may induce totally unexpected responses.

The general symptoms of space-flight stress are not well understood. Has the exhibited level of fatigue been commensurate with what may be anticipated, or is it indicative of problems not yet identified? As space flights become longer, the answers to such questions will become increasingly vital. In the following sections, the specific medical, physiological, and behavioral factors which may be significant in extended space flight will be discussed.

MEDICAL AND PHYSIOLOGICAL FACTORS

Weight Loss. Weight losses experienced thus far apparently have not interfered with astronaut performance, but losses continuing at the same rate could seriously affect it during longer flights. Present data do not indicate whether the effect diminishes with time.

Body-Fluid Volume and Electrolyte Balance. Space-flight data suggest that a decrease in body-fluid volume occurs that could compromise circulatory reserve upon re-exposure to gravity. The evidence that a steady state is established is tenuous, and the factors that would control the new steady state, if it exists, are not definitely known. They are potentially important, however, and deserve further investigation.

A variety of stresses is known to derange nitrogen metabolism, and a prolonged negative nitrogen balance is debilitating. Space-flight data on nitrogen balance are inconclusive since, for various reasons, in-flight collection of urine and feces has produced no quantitative samples for laboratory study.

Dehydration, presumably the basis of much of the astronauts' weight loss (with a likely loss of electrolytes), such as occurred during the Gemini IV flight, could present serious problems during missions of 20 to 30 days if it continued at a linear rate.

Calcium Loss. It is well known that physical immobility and disuse is accompanied by a loss of calcium from the skeleton. It has been postulated that weightlessness will cause a progressive imbalance of calcium during prolonged space missions. However, the best data available are inadequate for prediction of the magnitude of the loss, since the same factors that preclude prediction of nitrogen loss operate in the case of calcium—i.e., the short duration of the flights and the inability to obtain reliable measurements of urinary calcium content.

Blood Volume and Change in Red-Blood-Cell Mass. There is some evidence from the Gemini IV flight data that the astronauts' blood volume was diminished—a state called hypovolemia, which brings a physical predisposition to deconditioning. There is also evidence that the red-blood-cell (RBC) mass decreases with inactivity (see Appendix 7). If this is true, acute lysis of the RBC mass could occur upon resumption of activity, such as during extravehicular work or the return to Earth. The time course, causes, and mechanism of this decrease are unclear. An understanding of the underlying physiological mechanisms is likely to be the quickest route to the evaluation of response to stress, to time development, return to normalcy, and control procedures.

Blood Coagulation. Coagulability and viscosity of the blood change under stress conditions, such as exercise. There are no data from weightless experiments as yet, but the problem of blood coagulation in the weightless state should be investigated.

Metabolic Changes. Environmental stresses associated with space flight can substantially change metabolism, thus altering nutritional requirements. Since the physiological systems involved respond fairly slowly, effects on these systems can be expected to increase as missions become longer. Experimental data are inadequate, however, to permit quantitative predictions of the times involved. Much remains to be learned concerning nutritional during space flight. It is well known that individual nutritional needs are as varied as food preferences. Quantitative data on such requirements have not been obtained and the energy requirements of man in space have not been measured. During brief flights, nutritional factors may be of little significance, but for prolonged missions, information upon individual nutritional requirements may be more important than data on individual food preferences.

Compatibility of Bacterial Flora. Studies of humans in confined spaces have shown the occurrence of complex and often potentially adverse interactions of the men's bacterial populations. Some strains of potentially pathogenic organisms that normally occur in low abundance become predominant, for reasons that are almost entirely unknown. It is impossible to predict on the basis of available information the nature and variety of bacterial interactions that may develop if a group of men are isolated for some months in an enclosure so confining as a spacecraft.

Space Radiation. The effects of radiation on man and the factors that intensify or modify these effects are only imperfectly understood. Nevertheless, there is no *a priori* reason to suppose that radiation effects in space will not be the same as on Earth. Biosatellite and Gemini experiments should provide information on radiation effects in weightlessness when combined with acceleration forces and vibration.

For the past year, the Space Radiation Study Panel of the Space Science Board has been reviewing the space-radiation environment and its possible long-term effects on man. The Panel's report will be submitted to NASA in 1966.

Readaptation to Gravity. When the astronauts have just returned from a space flight, they have shown manifestations of deconditioning such as abnormal responses to tilt-table tests or decreased orthostatic tolerances. These symptoms have disappeared within a few hours. Whether physiological readaptation to the stress of gravity will be as rapid and complete following longer flights and whether deconditioning will compromise a crew's performance during re-entry is not known. The dysfunctions and their consequences may be in direct proportion to length of flight.

Combined Stresses. Recent human-confinement studies have shown complex interactions and potentially adverse effects of gaseous environment, weightlessness, lack of room for physical exercise, termperature extremes, and other factors that may influence physiological systems. That combinations of such stresses may produce additive or exponential effects on performance has long been postulated, but reliable data on this possibility have yet to be obtained. In fact, it is questionable whether knowledge of the physiological and behavioral effects of simultaneously or sequentially applied stresses is any greater than it was several years ago (see list of combined stresses that may have synergistic effects, Appendix 2).

PSYCHOLOGICAL AND BEHAVIORAL FACTORS

The psychological effects of the expected conditions in prolonged space flights lie outside human experience. Physical stresses undoubtedly will have a cumulative impact on behavior, and to these stresses must be added the effects of isolation, confinement, monotony, social restrictions, threat of danger, noise and silence, and the enforced proximity of differing personalities. The ability of an individual to withstand these factors and to function effectively over a long period of time is questionable. There is reason to believe that the probability of deterioration of psychological functions with time follows an exponential course. The study of behavioral stress has been extensive and some of this study is relevant to space-flight conditions. However, when compared with other biomedical studies and relative to its importance for mission success, study of behavioral responses has been neglected in the manned-space-flight program. (For example, no systematic behavioral measurements are being taken in flight.) Ground-based and in-flight investigations on performance under adverse conditions should be given systematic attention in the manned-space-flight program.

Specific questions that should be explored are 1) the kinds and degree of decrement, if any, in crew motivation and performance caused by length of mission and combined influences of restricted physical, physiological, and social environments; 2) physiological disturbances arising from prolonged interpersonal relations in highly motivated groups, to permit assessment of the importance of selecting flight crews on the basis of mutual compatability; 3) specific levels and types of activity needed to maintain physiological systems and behavorial skills, e.g., psychomotor; and 4) the time required to perform tasks in space and the percentage of errors made. Performance of tasks in the U.S. and U.S.S.R. flights has in some instances taken longer than anticipated, but whether that is due to environmental factors or to lack of familiarity with weightlessness is not clear. In any case, no analyses of time requirements or error have been made—in the U.S. flights, at least. Since it is essential to satisfactory long-term performance in space that astronauts not be overtaxed either in forces exerted or in execution or repetition of tasks, detailed and realistic mission plans must be evolved and followed.

LIFE-SUPPORT SYSTEMS

The success of manned space flights to date does not justify an assumption that current life-support systems will function effectively for longer than a few weeks. Although it appears that satisfactory compromises between engineering and physiological requirements have been made in Mercury and Gemini, the principal factors responsible for success are, first, the superb training, physical condition, and courage of the astronauts and, second, skillful application of the art and care of the flyer. The fact is that most trade-offs between engineering and physiological requirements have been made on the basis of man's very considerable ability to adapt to adverse circumstances rather than on the basis of clearly defined optimal conditions for effective performance of his role in space. Present life support systems compromise man's performance both as a subsystem and as an experimenter. While this compromise can be tolerated by the Apollo missions, it may become increasingly expensive with longer flights.

Cabin Atmospheres. Even though an extensive program of ground-based research has been conducted, the development of life-support systems is far from satisfactory. The trained astronaut thus far has been able to tolerate, in the cabin and during extravehicular activity, the abnormal gaseous environment. However, data indicate that the 100 percent oxygen atmosphere could cause difficulties in longer flights, and the limiting values of oxygen partial pressure for production of toxic effects have not been established. Systematic evaluation of the effects of prolonged exposure to other atmospheric compositions being considered for use in space is essential. While animal studies are valuable, species differences are significant enough to require careful selection of the experimental animals and final validation of findings with human subjects.

Toxic Contaminants and Waste Management. Toxic environmental contaminants could constitute a hazard on extended flights if not effectively dealt with in the design of the capsule and in the selection of materials for the construction of the cabin. The major design objective is to ensure that significant contact with or atmospheric contamination by toxic or annoying substances does not occur, or, where it does, to devise acceptable protective procedures. Experience with nuclear submarines provides important relevant information on acceptable concentrations of many atmospheric constituents and trace elements, but in some instances, owing to specialized problems of space flight, additional toxicological information may be required.

Waste management in current life-support systems is unsatisfactory even for a 4-day flight, and sanitary conditions could well become intolerable during longer missions.

Human Engineering. As flights become longer and astronauts are required to play a more active role, the living and working space in the capsule, now very cramped, must be expanded and improved. Attention must be given to human engineering and its application to the design of equipment and utilization of space. Particularly important are considerations of 1) efficiency of maintenance (recognition, diagnosis, repair, replacement, and checkout), 2) efficiency of storage, 3) efficiency of assembly, calibration, and maintenance of apparatus to be erected outside the spacecraft, and 4) convenience and attractiveness of the living space.

The twin constraints of weight and space have placed a heavy burden on the engineering of life-support systems and have made some discomfort and sacrifice unavoidable. Nevertheless, along with engineering and mission objectives, the safety and comfort requirements of astronauts must receive careful attention by mission planners. The medical, physiological, and behavioral aspects operating during manned planetary missions must be taken into consideration during the earliest stages of planning.

Biomedical Data Collection and Data Analysis. Physiological monitoring systems should be improved to ensure the safe conduct of longer missions. Current methods of sensing, recording, and telemetry are primitive by comparison with the sophistication of many other spacecraft components. An ideal monitoring system would allow quantitative estimate of the astronaut's physiological reserve and prediction of his responses to activity and to emergency. Furthermore, much valuable data, which to date have not been received, could be derived from continuous in-flight monitoring of physiological systems.

In-Flight Medical Care. The possibility of illness or injury among the crew increases as larger crews embark on longer and more complex missions.

Statistical data from various population studies should permit estimates of the probability and types of serious illnesses during flights, and the fraction of such illnesses that would require attention of a physician. This information would assist in realistic planning of the needs for medical supplies and medical members of crews.

3. WAYS TO OBTAIN NEEDED KNOWLEDGE

Preceding discussions have indicated that serious gaps exist in knowledge and technology related to the biomedical aspects of space flight. In-flight biomedical studies have been minimal and have thrown little light on problems anticipated in missions of long duration. Engineering, vehicle performance, and survival considerations, have dominated the Mercury and Gemini programs and appear likely to continue to dominate in the Apollo program (flights up to 14 days) and even in the proposed Apollo Applications Program (flights up to 100 days). The structuring of experiments projected for the initial Gemini flights will not produce sufficient medical or physiological data to allow a firm prediction of the success of flights over 100 days. Experiments proposed for Apollo do not appear likely, at least in the early phases, to yield the data required for planetary missions. If, however, a broad, logical, and cohesive program of biomedical study is initiated promptly, there is good reason to predict that necessary biomedical knowledge will be available when needed to ensure the success of manned planetary exploration.

Necessary data may be obtained in three ways—by ground-based studies, by in-flight experiments using animals, and by in-flight experiments on man.

GROUND-BASED STUDIES

In addition to flight-support programs and control experiments, continuing ground-based biomedical research should be broadened and accelerated. The research should include not only study of the stresses of space flight and their effects—singly and combined—on medical, physiological, and behavioral processes, but also attempts to clarify the fundamental nature and morphology of the physiological and behavioral processes likely to be affected by space flight. In cases where the stresses must be simulated, the transfer or carry-over validity among the various types of simulation data, and from the simulation results to the actual reactions in space, must be carefully assessed. In regard to weightlessness, the degree to which the ground-based simulations reproduce the effects of actual zero g has not been established.

Ground-based research should also attempt to increase statistical certainty to a degree not feasible in space flight by more repetition of experiments and by using larger numbers of specimens of the same species. Space operations, because of their enormous expense if for no other reason, should be reserved for those experiments and activities that cannot be carried out successfully on Earth.

IN-FLIGHT EXPERIMENTS USING ANIMALS (BIOSATELLITE PROGRAM)

While all data derived from in-flight experiments with animals are not applicable to manned flight, they can be of material assistance to the manned program. Results of some of the Biosatellite experiments will bear directly on development of life-support systems, particularly in regard to the choice of gaseous environments. The Biosatellites, in the 3-, 21-, and 30-day flights, will use a sea level, two-gas system, as opposed to the 5 psi, 100 percent oxygen atmosphere used in Mercury, Gemini, and Apollo. The 21-day and 30-day primate flights will be of particular interest relative to man's survival in space and, with other experiments on the combined effects of weightlessness and radiation, should have predictive value for prolonged flights. Experiments beyond those scheduled for the first Biosatellite series could produce useful information on circulation, metabolism, neurophysiology, and behavior.

IN-FLIGHT EXPERIMENTS ON MAN

It is clear that in-flight animal experiments, together with ground-based investigations, can and should directly supplement the manned program. In most instances, however, it will be necessary to confirm the findings of the animal and ground-based experiments by further tests on man in actual space flight. The critical, definitive experiments and measurements necessary to extend mission duration with confidence must be made on man himself in flights of increasing length.

These critical investigations cannot be carried out in programs that are opportunistic or that cause undue competition for the astronauts' time and energy by attempting to combine systematic biomedical studies with responsibility for operating spacecraft. Any program to provide sufficient biomedical knowledge to justify subjecting man to prolonged flights within the next two decades must 1) have high priority in mission planning and include flights designed primarily for biomedical investigations; 2) progress by orderly increments, with thorough and systematic analysis of the results of preceding flights; 3) exploit the capabilities of the Apollo, Apollo Extension System, and manned orbiting laboratories, with direct biomedical control over an appropriate number of flights in each system; 4) be well backed by coordinated ground-based studies and, where appropriate, by in-flight studies on other species; and 5) make full use of biomedically trained scientist-astronauts and scientist-passengers.

MANNED ORBITING LABORATORY

In addition to the biomedical data obtained from the Apollo and Apollo Applications Programs, data acquired from a series of manned orbiting laboratories will be required. These satellites should be designed to accommodate at least six to eight men and have considerably more working space for experimentation than would probably be available in the Apollo capsule.

4. THE SCIENTIFIC ROLES OF MAN IN SPACE

On the premise that in-flight experiments on man are necessary before prolonged manned missions can be undertaken, it follows that—if only for this reason—scientifically trained crew members must be present as experimenters and experimental subjects. The nature of the medical, physiological, and behavioral processes to be studied is too complex, subtle, and time-dependent to permit satisfactory recording and analysis by automated instruments, telemetry, and ground-based control. As an experimenter, only man has the ability to deal with the unexpected—to devise and revise hypotheses, to adjust to varying conditions, and to calibrate, interpret, collate, and integrate information. As a scientific technician, he can detect, repair, or replace faulty experimental equipment. As an experimental subject, man takes into space intellectual functions that no other organism possesses. The influence of the space environment on these functions, particularly as a result of extended exposure, can be determined only with man serving as the experimental subject. Complex behavioral performance may shift before any physiological indicator can reveal it. Moreover, with no other subject can such complex schedules of investigations of physiological processes that are expected to be gravity-dependent be undertaken.

SCIENTIST-PASSENGERS

Scientist-astronauts—persons combining the skills of pilot and scientist—are already engaged in the manned-space-flight program and are expected to participate in the later Apollo missions. Substantial demands on their time and energy will be made in operating the spacecraft. Thus they may not be able, even with the aid of pilot-astronauts, to carry out the full-scale biomedical program discussed above. Therefore, scientist-passengers, able to devote their entire time and concentration to scientific investigations, should be included as crew members of future missions.

ASTRONAUT SELECTION AND TRAINING

As the space program evolves toward multimanned crews and longer flights, problems of crew structure and compatibility, task allocation and competence for emergency procedures, and crew time available for experimentation will emerge as important elements. Thus crew selection, training, tasks, and composition must be continually evaluated and adjusted to meet changing requirements. For example, physical and medical standards for crew selection should in the future depend upon the state of spaceflight technology, upon specific mission requirements, and upon the specialized types of astronauts sought. The imposition of the same minimum physical standards for scientist-passengers as for pilot-astronauts may prove unnecessarily stringent and could result in an excessive limitation on scientist-passenger candidates at the expense of needed scientific qualifications.

Specific types of medical evaluation during the selection process may warrant greater emphasis; for example, predisposition to nephrolithiasis, sensitivity to labyrinthine stimulation, and individual nutritional requirements. Criteria must be developed also for the assessment of individuals for group leadership, for the evaluation of personal compatibilities, and for the selection and composition of crews in terms of best sustained group performance. Training programs for scientist-astronauts and scientist-passengers should omit teaching of unnecessary engineering and piloting skills. Training programs must, however, maintain and augment the scientific interest and competence of participating scientists.

5. PUBLICATION OF RESULTS

Space biomedicine is a young specialty. Although an enormous amount has been published on the subject or relevant to it, this literature is less accessible to the scientific community than that of older disciplines. This is due in part to the lack of well-known communication channels or journals that have been devoted over the years to reporting biomedical findings. This problem is to be expected but it can be, and is being, overcome through standard methods of library science. Another problem exists, however, that is avoidable—some organizations evidently prefer to issue results of their biomedical research through their in-house publications rather than through national or international journals. Thus some findings are inaccessible to scientists who would be interested in them. These findings should be consistently and promptly published in appropriate scientific journals regardless of whether they appear as agency documents. It is necessary, both for adequate dissemination of biomedical information and for critical review by the scientific community, that the established channels of scientific communication be used insofar as possible.

APPENDIX 1: LIST OF PARTICIPANTS

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APPENDIX 2.

BIOMEDICAL EXPERIMENTS PRESENTLY PLANNED FOR THE MANNED-SPACE-FLIGHT PROGRAM

In the following pages are listed (1), Gemini and Apollo in-flight medical experiments and (2) Proposals for the Apollo Applications Program. These experiments are currently being redefined in terms of smaller units of measurement and technique to provide flexibility of protocol and to accommodate operational and programmatic regulations. The lists are largely based on consideration of (3) General Formulations of Human Psychophysiological Functions and of Significant Space Flight Environmental Factors, as evaluated by the Space Medicine Advisory Group.

Gemini and Apollo In-Flight Medical Experiments

M-1 Cardiovascular conditioning
M-2 Cardiovascular effects of space flight (now an operational procedure)
M-3 In-flight exercise; Bungee cord
M-4 In-flight phonocardiogram
M-5 Bioassays of body fluids
M-6 Bone demineralization; x-ray densitometry
M-7 Calcium balance study
M-8 In-flight sleep analysis (EEG)
M-9 Human otolith function
M-11 Cytogenic blood studies
M-12 Exercise ergometer
M-17 Thoracic blood flow
M-18 Vectorcardiogram
M-19 Metabolic rate measurement
M-20 Pulmonary function
M-21 Semicircular canal function
M-22 Red-blood-cell survival
M-23 Lower-body negative pressure

Proposals for Apollo Applications Program

010 Otolith sensitivity
102 Semicircular-canal sensitivity
103 Circulatory response
 Lower-body negative pressure
 Cardiac output by electric impedance across chest
 Electrocardiogram, blood pressure
 Clinical observation

104 Cardiovascular work capacity
  Tests on above in response to exercise
105 Fluid-volume shifts
106 Carotid sinus and arteriolar responses
107 Venous compliance
108 Evaluation of cardiovascular countermeasures
  Venous cuffs
  Exercise
  Lower-body negative pressure
109 Pulmonary function—respiratory mechanics
110 Ventilatory gas exchange
111 Muscle mass and strength
112 Mineral metabolism and fluid balance
  Calcium—bone density, etc.
113 Nutritional status such as
  Body volume, mass
  Glucose tolerance
  Fat tolerance
  Metabolism tests
  Nitrogen balance
  Serum proteins, liver function
114 Gastrointestinal motility and pH (hydrogen-ion concentration)
115 Thermal regulation
  Temperature measurement
  Correlation with endocrine and cardiovascular evaluations
117 Endocrine function
  Thyroid
  Adrenal, pituitary, etc.
118 Hemic cell study
  Hemograms
  Red-blood-cell survival
  Cytogenetic studies
119 Hematological defenses
  Immunological studies
  Leukocyte function
120 Hemostasis
  Clotting mechanisms
121 Microbiological evaluations
201 Sensory and perceptual processes
  Vision
  Audition
  Kinesthetic
  Orientation
202 Psychomotor functioning
203 Higher mental processes
  Vigilance and attention
  Memory and problem solving
  Speech and reading

General Formulations of Human Psychophysiological Functions and of Significant Space-Flight Environmental Factors

<u>Body Functions</u>

(1) Neurological function
- Central nervous system
  - States of consciousness
    - Sleep
    - Alertness
  - Speech functions
  - Adaptability
  - Emotional reactivity
- Special senses
  - Vision
  - Auditory
  - Smell
  - Taste
  - Vestibular
- Peripheral nervous system
  - Somatic
  - Autonomic

(2) Psychological performance
- Sensation
- Psychomotor
- Perception
- Higher mental processes

(3) Circulation
- Pump
- Blood-volume control
- Vasomotor control

(4) Respiration
- Lung
- Blood
- Tissue

(5) Digestion
(6) Metabolism
(7) Endocrine balance
(8) Thermo-regulatory and integumentary
(9) Neuromuscular
(10) Skeletal
(11) Fluid and electrolyte balance
- Renal and urinary tract functions

(12) Reproduction
(13) Hematological response
(14) Immunological response

<u>Environmental Factors (Stresses) of Space Flight</u>

Single Environmental Factors
Stresses (prolonged)
- Weightlessness
- Radiation

Confinement
Social restriction
Monotony
Threat of danger
Artificial atmosphere
Toxic substances
Particulate matter
Microorganisms
Change in circadian rhythms
Ultraviolet exposure
Infrared exposure
Noise
Thermal stress

Combined Stresses that May Have Synergistic Effects
Weightlessness and radiation
Weightlessness and confinement
Weightlessness and social restriction
Weightlessness and monotony
Weightlessness and threat of danger
Weightlessness and artificial atmosphere
Weightlessness and toxic substances
Weightlessness and particulate matter
Weightlessness and microorganisms
Weightlessness and circadian rhythm changes
Weightlessness and ultraviolet exposure
Weightlessness and infrared exposure
Weightlessness and noise
Weightlessness and thermal stress
Weightlessness and social restriction and confinement
Weightlessness and threat of danger and particulate matter
Weightlessness and artificial atmosphere and particulate matter
Weightlessness and toxic substances and particulate matter
Weightlessness and microorganisms and particulate matter
Weightlessness and thermal stress and particulate matter

APPENDIX 3.

BIOSATELLITE EXPERIMENTS PRESENTLY SCHEDULED

Biosatellite A—3-Day Mission

General Biology

- Liminal angle in the pepper plant
- Orientation of roots and shoots in wheat seedlings
- Nutrition and growth in pelomyxa
- Development of frog eggs
- Development of sea urchin eggs
- Emergency of wheat seedlings
- Orientation of roots and shoots in corn

Radiation Experiments

- Cellular inactivation and mutation in *neurospora* spores
- Embryonic development in *Tribolium*
- Chromosome translocation in *Tradescantia*
- Somatic mutation in *Tradescantia*
- Viral induction in lysogenic bacteria
- Genetic changes in mature germ cells of adult *Drosophila*
- Somatic damage in larvae of *Drosophila*

Biosatellite C—21-Day Mission

General Biology and Biorhythms

- Plant morphogenesis—*Arabidopsis*
- Human (lung) cells in tissue culture
- Metabolic rhythms in mammals
- Gross body composition in mammals

Biosatellite D—30-Day Mission

Primate

- CNS, C/V and metabolism of primates
- In-flight fluorometric urine analyses
- Ca^{++} determination and bone x-ray densitometry

APPENDIX 4.

CAPABILITIES AND REQUIREMENTS FOR MANNED PLANETARY MISSIONS

Franklin P. Dixon

Introduction

Expeditions to the planets of our solar system are the ultimate goal of manned space flight in this century. These will be undertakings of vast complexity and immense significance.

To date, NASA has concentrated study efforts to define the objectives of planetary exploration, the problem areas, and the means, methods, and resources required to accomplish such exploration. Several mission alternatives appear feasible, and the more promising missions and mission characteristics are being identified. The mission modes and system requirements are being determined. In addition, a series of trade-off studies is under way to evaluate alternative mission modes, define precursor program requirements, examine engineering solutions to major problems, and determine the technological programs required to achieve manned planetary flight capability.

Missions and Technology

On the basis of current study results, it is anticipated that the first manned planetary expedition will be a trip to Mars or Venus. The less hostile environment that appears to exist on Mars, the apparent technical feasibility of landing on that planet, and the fact that it offers the best possibility, in our solar system, for supporting extraterrestrial life make it the most likely candidate for a landing mission. Although our initial study efforts have concentrated on Mars missions, we have not by any means excluded considerations of missions to Venus. Capture missions with Venus orbiting should be easily achieved using the propulsion systems and spacecraft technology required for a Mars landing. Landing on Venus would be much more difficult, however.

The following facts about the planets strongly affect mission accomplishment, and must be considered in planning the details of planetary exploration.

(1) An <u>opposition</u> of Mars (see Figure 1) occurs when the Earth passes between Mars and the Sun. Oppositions occur approximately every 25 months. The time between oppositions is termed a <u>synodic period</u>. Earth and Mars are in <u>conjunction</u> when the distance between them is maximum (the Sun is between them). At this time, the separation distance can be as much as approximately 250 million miles.

(2) At time of opposition, Earth and Mars are closest to each other (see Figure 1). However, due to the eccentricity of the Martian orbit, this distance varies from approximately 34 million miles during a favorable

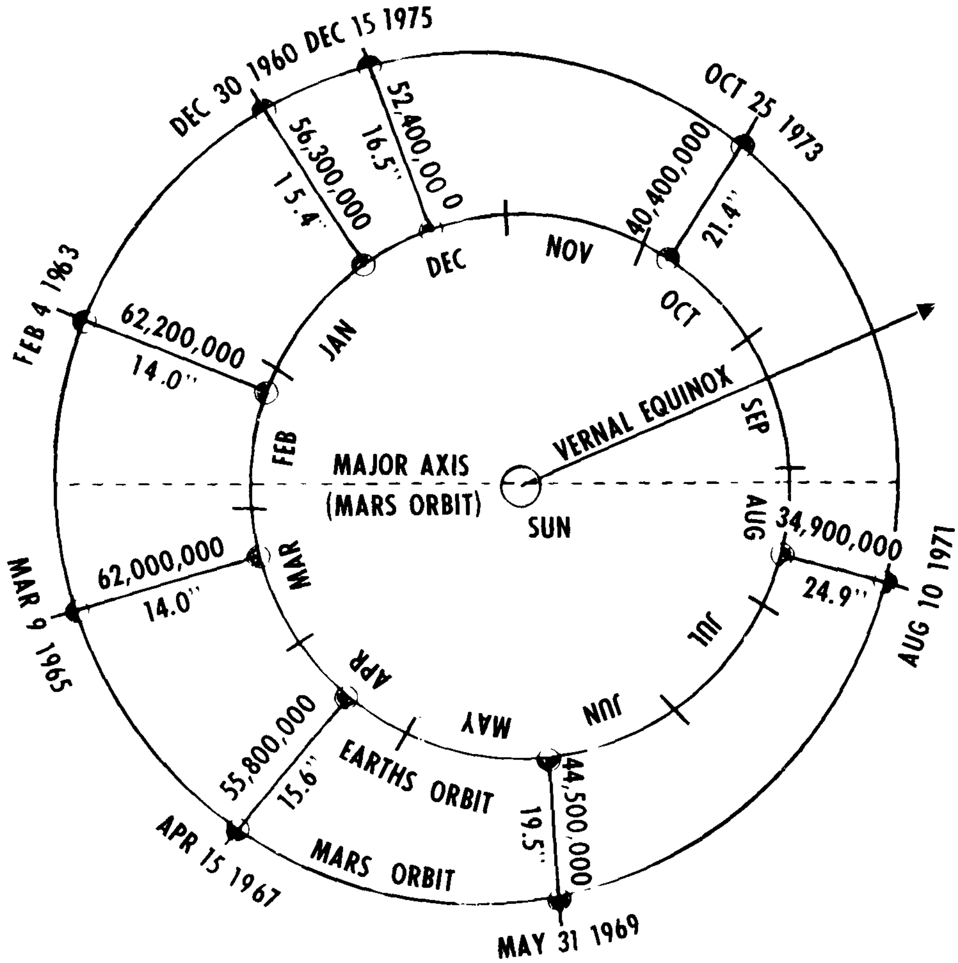
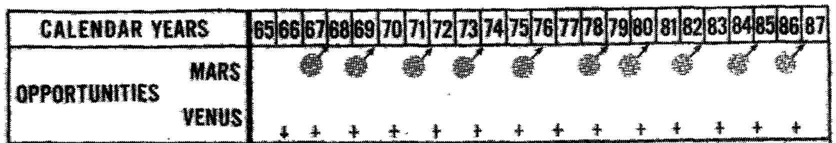


Figure 1. Oppositions of Mars.

opposition, such as in 1971, to as much as 64 million miles during an unfavorable opposition such as that of 1980. The cycle of oppositions repeats at 15- and 17-year intervals. This is termed a synodic cycle. (The orbits of Earth and Venus may for our purposes be considered circular.) The complexity of the task of preparing long-range plans for planetary exploration is suggested when one considers the many possible missions that have been identified for the earliest flights to the nearby planets (see Figure 2). Each of these missions has its unique requirements and alternative means of mission accomplishment. Of the missions shown, only the flybys lend themselves to accomplishment by means of extensions of technology and hardware being developed for present space-flight programs. The use of existing technologies and systems for the stopover missions would be ineffecient and expensive, and would provide little potential for meeting the requirements of more ambitious missions.

A flyby mission includes a pass near one or more target planets without stopping, and eventual return to the Earth. The spacecraft uses a close approach to target planets to modify its trajectory and to control mission



PROMISING MANNED MISSIONS		
TYPE MISSION	PLANET	NOMINAL DURATION(DAYS)
FLYBY	MARS	500-700
	VENUS	350-380
	MARS - VENUS	450-600
	VENUS - MARS - VENUS	700
ORBITING	MARS	350-500
	VENUS	360-400
LANDING	MARS	350-900
	VENUS(?)	360-400
FLYBY-LANDING	MARS LANDING-VENUS FLYBY	500-550
	VENUS FLYBY-MARS LANDING	500-550

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Figure 2. Planetary missions.

duration. This class of mission has low energy requirements and is relatively insensitive to the year of opposition in both duration and energy. Problems are introduced by the length of the mission (about 700 days), however, and by the fact that the trajectory carries the spacecraft beyond the orbit of Mars into the asteroid belt, where debris may be a hazard. The mass in Earth orbit required for this mission would vary from 1.0 to 1.3 million lb using chemical propulsion.

Existing technology is not sufficient to provide an acceptable margin of risk for stopover missions at Mars or Venus. Investigation of stopover Mars missions started with the opposition-class trips. An opposition-class trip to Mars is one in which the spacecraft arrives at and departs from Mars within a span of a few days near the time of opposition. These trips are characterized by high energy requirements, high entry speeds, and, for a minimum landing mission, by masses in Earth orbit on the order of several million lb. It was determined that mission requirements for these short missions (350-500 days) varied greatly (see Figure 3) across a synodic cycle due to the eccentricity of the Martian orbit. A great deal of uncertainty exists concerning the estimated minimum weights shown, due to such judgment factors as assumed spacecraft systems, reliability levels, meteoroid fluxes, anticipated propulsion-system performance, number of crew members, and similar variables. Figure 3 represents a consolidation of the latest study results, however. The high initial weight required in Earth orbit and the varying requirements have

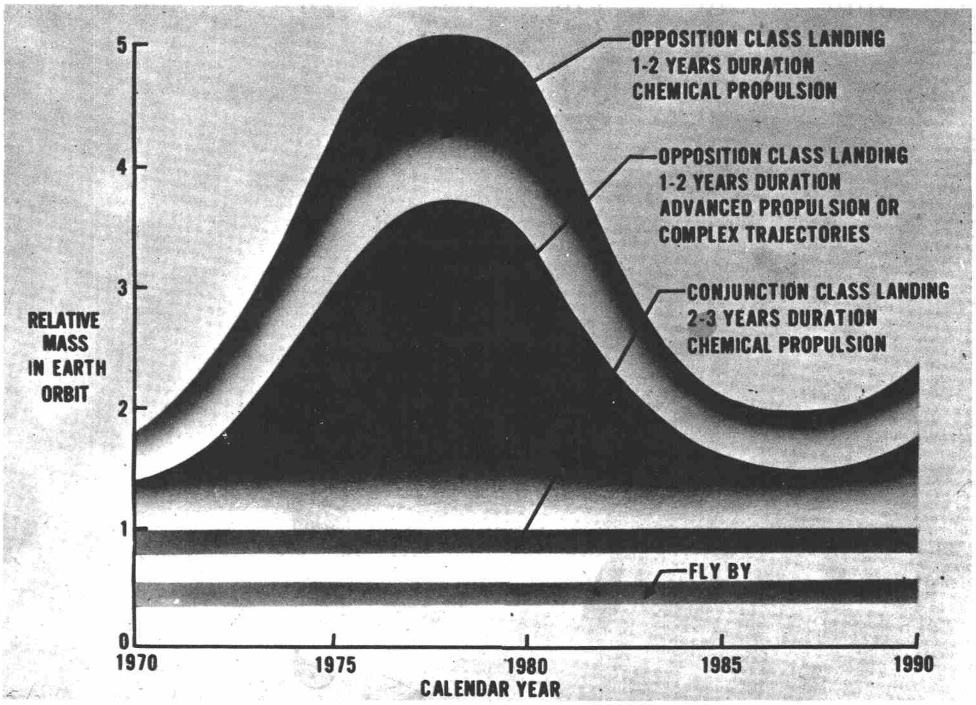

Figure 3. Manned Mars mission requirements.

led to a search for means to reduce mission requirements and to make them relatively constant across a cycle of opportunities.

Many approaches have been examined. In general, they involve advanced propulsion systems, complex maneuvers, or complex trajectories. For instance, the weight needed in Earth orbit for some Mars landing missions would be reduced by a factor of 2 to 5 if solid-core nuclear engines, presently contemplated, were used instead of chemical rockets. Other advanced rockets using nuclear pulse or gas-core reactor systems could have even greater advantages in performance. Aerodynamic braking instead of propulsive braking at Mars could result in a 20 percent reduction of initial mass in Earth orbit; direct Earth entry at high speeds (65,000 fps) could result in a similar reduction. Each of these solutions, however, introduces new problems.

Conjunction-class trips to Mars are characterized by arrival at Mars after one opposition date and departure before the next. These are long missions, typically 750 to 950 days, and provide a stay time at Mars from 0 to 550 days. This class of mission has low propulsion-energy requirements, relatively low entry speeds, and a mass in Earth orbit appreciably less than that of the opposition-class "fast" trips; and, unlike the opposition-class trips, the conjunction-class mission varies only slightly across the synodic cycle.

Of the available methods of mission improvement, the Venus swingby mode, (see Figure 4) shows great promise because of its very large reduction in Earth-entry velocity requirement. Essentially, this mode involves a transfer orbit in which the gravitational field of Venus is used to

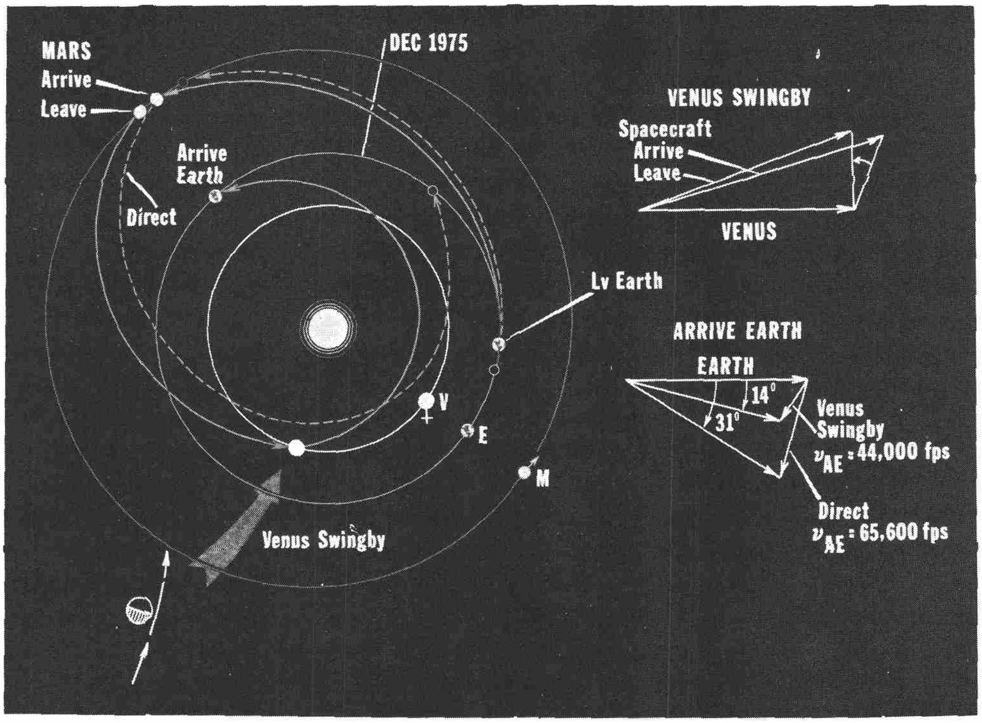

Figure 4. Venus swingby return mode.

alter the heliocentric velocity vector of the spacecraft as it passes near the planet. This results in a more nearly tangential approach to Earth than for direct trajectories. As shown for 1975, this maneuver would reduce Earth-entry speed by 21,600 fps (65,000 fps to 44,000 fps). This is extremely important since entry velocities of 65,000-70,000 fps are considered to be of marginal feasibility. About 75 percent of the missions can effectively use the Venus swingby mode returning from Mars.

In the remaining opportunities the whip effect on spacecraft of the gravitational field of Venus can be utilized to accelerate the craft. It has been determined that, regardless of opposition, Venus swingby trajectories have Earth-return velocities generally below 50,000 fps. It should be noted that swingby trajectories involve longer mission duration than direct trajectories. However, the increase has only a small effect on gross weight of the spacecraft.

The landing mission modes presently contemplated would offer staytime at Mars ranging from 0 to 550 days. Some idea of the alternatives available for Mars landing can be shown by Figure 5. It should be emphasized that this figure shows only the major operations involved mission and their associated major alternatives.

It now appears that a post-Saturn vehicle capable of delivering into low Earth orbit a payload of 1 to 3 million lb will be needed for manned planetary capture or landing missions. Even with this boost capability, some orbital operations—such as assembly, refueling, repair, and checkout—may be required. Design of the post-Saturn vehicle cannot proceed until

MAJOR OPERATIONS	MAJOR ALTERNATIVES
LAUNCH	SATURN V, POST-SATURN
ORBITAL OPERATIONS	ASSEMBLY, PROPELLANT TRANSFER
INJECTION	CHEMICAL , NUCLEAR, ADVANCED NUCLEAR ELECTRIC
INTERPLANETARY TRAJECTORY	OPPOSITION, CONJUNCTION, SWINGBY
SPACECRAFT ENVIRONMENTAL CONTROL	ZERO "G", ARTIFICIAL "G"
MID-COURSE	CHEMICAL, ELECTRIC, NUCLEAR
PLANETARY CAPTURE	PROPULSIVE, AEROBRAKING, COMBINATIONS
PLANETARY LANDING	DIRECT, LANDING MODULE
PLANETARY ASCENT	DIRECT INJECTION, RENDEZVOUS IN ORBIT
PLANETARY DEPARTURE	CHEMICAL, NUCLEAR, ADVANCED NUCLEAR ELECTRIC
MID-COURSE	CHEMICAL, ELECTRICAL , NUCLEAR
EARTH RETURN	RENDEZVOUS , DIRECT
EARTH ENTRY	PROPULSIVE, AEROBRAKING, COMBINATIONS
LANDING	BALLISTIC, LIFTING VEHICLE

Figure 5. Mars landing mission elements.

the mission weights, spacecraft configurations, and orbital-operation techniques are defined.

When man has been transported to the planet to conduct exploration and experimentation, means must be provided to carry him to the surface and to shelter him. A plan of scientific exploration must be available. Instrumentation and means of extending his range of exploration must be provided. The weights, volumes, power requirements, and related factors of these systems must be known and included in the mission studies so that crew size, systems, vehicles, and initial weights may be determined. Preliminary studies of this nature are under way with detailed examinations to follow.

Considerations Related to Man

The primary system essential to the attainment of manned planetary exploration is man himself. At the present time less is known about the capabilities and performance parameters of this system, under conditions of long-duration space flight, than about those of any of the other required systems. For example, adequate information on the effects of weightlessness on man's continued health and performance during trip durations of several hundred days is not available. The studies to date in the Manned Planetary Mission Office have not been concerned directly with obtaining experimental data to answer these human-factors questions. Efforts are directed, rather, toward outlining the specific problems relating to man, assessing methods of obtaining the knowledge required, and examining engineering solutions to the apparent problems.

The performance of man as a functional system is integrally related to his total environment and constitutes a major area in which additional information is required. Good experimental data are needed in planning the design of the environmental constraints of manned planetary spacecraft, with proper allowance for performance degradation, so that a minimum crew may be carried and a maximum amount of scientific information returned. A great many of the necessary experimental data will be obtained from the Gemini, Apollo, and Earth orbital-flight programs. Additional information must be gathered under a variety of conditions, some of which are indicated below.

The alternatives of weightlessness or artificial gravity, the problem of man-machine integration, the radiation environment, work-rest cycles, and acceleration tolerances are an intimate part of man's interaction with his spacecraft environment. Closely related for the accomplishment of long-term missions are the questions of confinement, volumetric requirements, spacecraft atmosphere, toxic atmospheric contaminants, spacesuit mobility and life support, prevention of Mars contamination, and prevention of back contamination of the Earth's biosphere.

Weightlessness constitutes a predominant problem to be resolved. Unless provisions are made for artificial gravity, the crew will be exposed to nearly zero gravity for long periods of time. The physiological adaptations that may result, the nature of the adaptive responses, the time to equilibrate (if this occurs), and the importance of equilibrium to health and performance are not now known. These factors are highly significant, however, for vehicle design, development, and operations. One obvious solution of these problems is to provide some level of artificial gravity. However, a rotating environment introduces its own physiological and operational problems. Engineering solutions to provide artificial g are available, but an operational penalty (increased weight) is incurred.

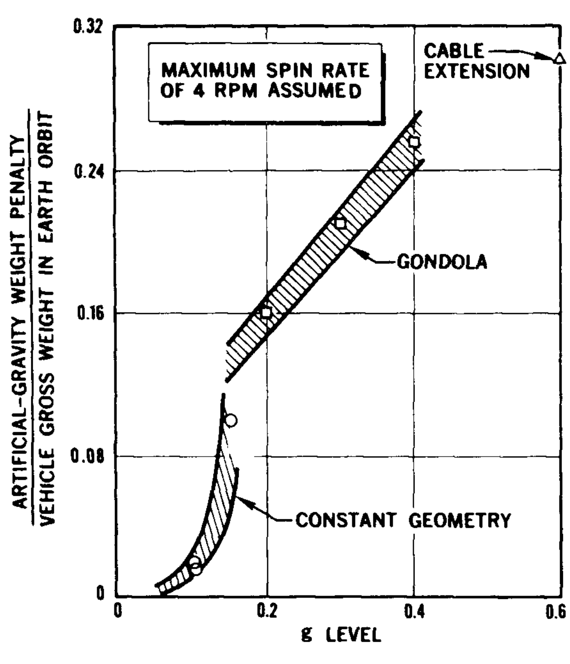

Figure 6. Artificial-gravity systems weight penalty.

Figure 6 shows this weight penalty as a fraction of the gross weight in Earth orbit that various artificial-gravity techniques place on a particular spacecraft system that NASA has studied. "Constant geometry" is merely rotation about the longitudinal axis of the cylindrical spacecraft. Artificial gravity is limited to about 0.15 g with this type of configuration. A gondola would have living quarters extended on booms, or arms, allowing greater radii of rotation and larger values of gravity, up to about 0.4 g, for the 4 rpm limit on spin rate. Cable-extension systems appear feasible; however, the weight penalty ranges from 10 to 30 percent for various cable-extension estimates, and the chart in Figure 6 identifies the most conservative (or largest) of these estimates.

It is possible that a centrifuge system or space-suit compensations could alleviate all major physiological difficulties of extended weightlessness. Before such problems can be adopted with confidence, however, it will be necessary to have experimental data, probably from Earth-orbital flights, to provide extended exposure to weightlessness and to various degrees of artificial gravity.

Another area of concern is exposure to space radiation and the systems needed to protect man from excessive doses or dose rates (see Figure 7). Shielding requirements are based on the allowable critical dosage per year. The maximum length of a planetary trip would be approximately 3 years for a conjunction-class mission; the necessary shielding thickness, with and without a nuclear auxiliary power supply is indicated in the figure. This shielding would surround a volume enclosing the crew during solar-flare activities.

One possible systems approach is to surround the command re-entry module for a Mars flight by a water wall during the interplanetary phase of the trip. Confinement to the biowell could last for many hours during fairly severe high-energy proton exposure from a solar flare. There are many approaches to solution of this problem, but additional information on man's resistance and protective techniques is needed.

Crew requirements in the form of crew functions, crew sizes, selection, training, and programming of activities will all be important for practical missions. Better data on which to base work-rest cycles and establish crew-selection criteria and training requirements are essential.

CRITICAL ORGAN	BODY SELF SHIELDING (gm/cm2)	ALLOWABLE DOSE (RAD/YR)	SHIELD MATERIAL THICKNESS (gm/cm2)		
			AL 2.7 gm/cm3	H_2O 1.0 gm/cm3	CH_2N 0.92 gm/cm3
BLOOD-FORMING ORGANS	5	50	16.1	11.2	10.3
			(18.3)	(13.0)	(11.9)
GENITALS	1	250	9.5	7.2	6.7
			(9.7)	(7.3)	(6.8)
GASTRO-INTESTINAL TRACK	3	80	14.0	10.0	9.2
			(14.8)	(10.7)	(10.0)
SKIN SURFACE	–	150	13.0	10.0	9.4
			(13.2)	(10.2)	(9.6)
EYES	0.3	25		23.6	22.3
				(34.0)	(31.5)

NOTE: ( ) INCLUDES AN ALLOWANCE FOR A NUCLEAR AUXILIARY POWER UNIT.

Figure 7. Biowell shield requirements.

Systems for the Mars missions where aerodynamic braking is used at Mars capture, or during Earth return, will be greatly influenced by man's allowable acceleration tolerances. In Figure 8 the acceleration profiles for Mars aerobraking and Earth return are plotted, with estimated average human tolerances and the maximum accelerations experienced. The variation of tolerances as a function of extended exposure to unusual environments will undoubtedly be an important criterion in designing systems so as not to exceed man's tolerances in the long planetary flights.

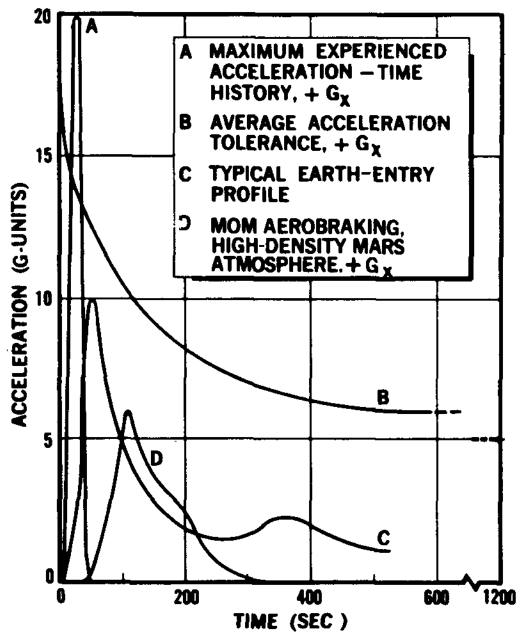

Figure 8. Acceleration tolerances.

Another critical area concerns the volumetric requirements of the spacecraft atmosphere. Figure 9 indicates the effect on spacecraft design of single-gas or two-gas atmospheres. A two-gas, oxygen-nitrogen atmosphere, with a total pressure of 7 psia, will give a slight increase in systems weight over a single-gas oxygen atmosphere at 5 psia. These weights represent a completely open system with oxygen replenishment and CO_2 removal. A regenerative system would show an even smaller difference in weight with time.

The problem of toxic contaminants appears to be of major concern. Figure 10 shows the threshold limit values for some examples of potential contaminants.

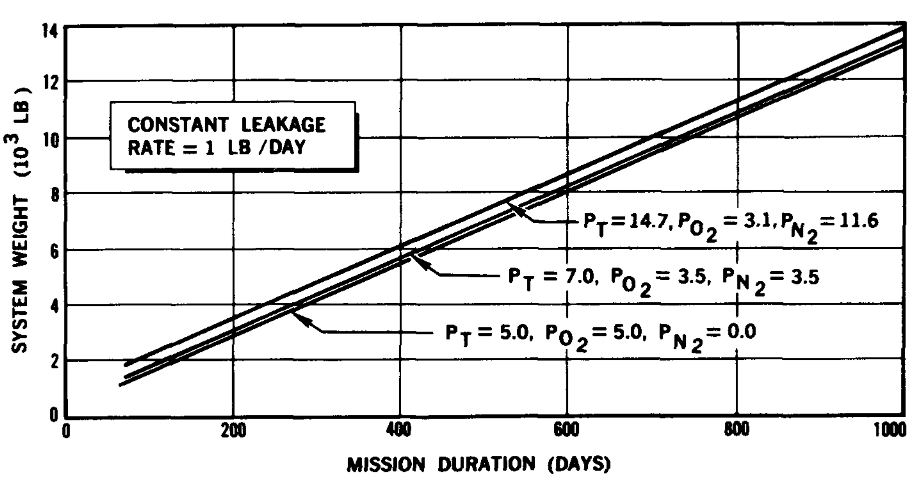

Figure 9. Effect of atmosphere composition on atmosphere supply system weight.

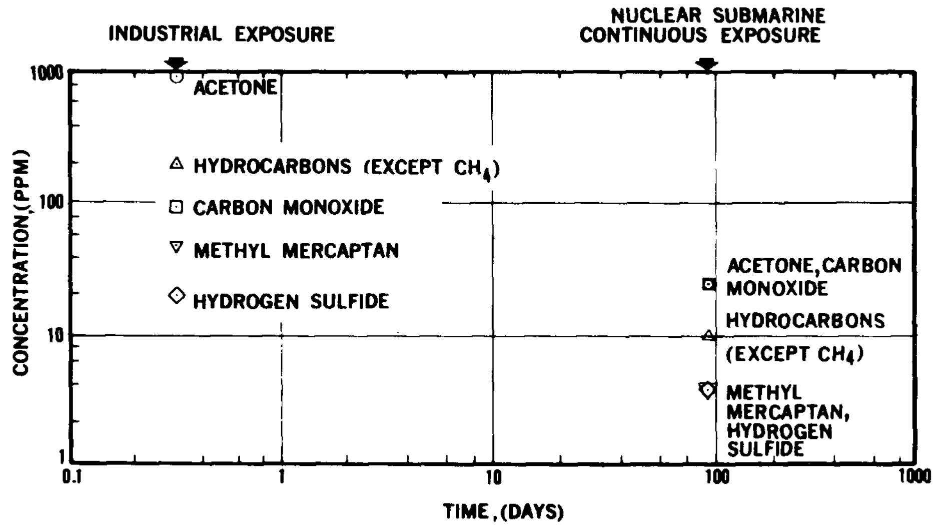

Figure 10. Contaminant threshold limit values.

Permissible industrial exposure is 8 hr, with a 16-hr recovery before additional exposure. The allowable parts per million are about one and a half orders of magnitude greater than that permitted for continuous hours of exposure, as indicated by the nuclear submarine data. To illustrate the complexity of this problem, 40 contaminants have been reported in the Mercury spacecraft atmospheres; atmospheric samples from nuclear submarines have indicated the presence of 200 contaminants, of which only 65 have been identified.

Although it is possible to purge the atmosphere when the contaminant level gets too high, it is desirable to be able to remove some of these contaminants (for example, with catalytic burners) without having to change the atmosphere. It is also desirable to be able to maintain good environmental conditions without substantially increasing on-board mass. Steps required to understand the potential problems and to eliminate risks will include (1) experimental tests of material under various conditions and with man; (2) establishment of the allowable concentrations of the identifiable contaminants, and (3) development of methods to monitor and control contaminant levels, which will be essential if we are to avoid the large mass penalties indicated above.

Surface operations on the planets will probably demand many more man-hours than were initially estimated for the Apollo program. Greater space-suit mobility and life-support capability are thus mandatory. Two of the requirements would be for non-expendable cooling systems and for regenerative CO_2 systems with low power requirements. Much less total weight is needed to support surface operations if a regenerative suit rather than the Apollo back-pack is used; whereas weight increases almost arithmetically with time for the Apollo suit, it is virtually constant with a regenerative system. For stay-time of 100 or more man-days the more advanced capability would obviously be of great benefit. Some of the Mars missions would allow up to 550 days in the vicinity of Mars with perhaps 1,000 man-days of actual surface operations (3.5 hr per day outside the

Mars landing spacecraft). This would represent a weight difference on the order of 10,000 lb between Apollo capabilities and an improved space suit with the indicated regenerative features. If we were limited to the Apollo suits, the water for suit cooling would represent approximately half of the total makeup weight. Thus, in the absence of CO_2 regeneration, a thermal-control capability without expendables would still be significant.

The last two items, which follow, while not limited to manned missions, are a major consideration in all our scientific planning. The prevention of contamination of Mars, so that true experimental results in exobiology may be obtained, is essential. It is desirable to have this no-contamination requirement for both manned and unmanned missions. Although it is much too early to indicate methods for accomplishing the goal when man is there, techniques used in the manned exploration of Mars will undoubtedly be influenced by this requirement.

In addition, it is important to prevent back contamination of Earth by specimens gathered on Mars, or by the crew members who, as hosts, may carry potential pathogens. Of course, the long return-trip times—on the order of 200 to 400 days—constitute periods in quarantine. During these periods, serious potential problems are likely to become apparent in the spacecraft long before they could harm the Earth's biosphere. The excitement of gathering scientific material from neighboring planets must not cause us to fail to take adequate precautions against unpredictable possibilities; nor should fear of these unknowns deter our progress.

Conclusion

Many factors remain to be considered and much supporting information is required from other space-flight programs before we can complete the design of an optimal system for manned planetary exploration. Toward this end, our studies have defined the tests and engineering developments necessary during Earth orbit to assess man's ability to survive and perform in space and to develop equipment and techniques needed for planetary flight. Guidelines are being prepared for the unmanned planetary program, specifying the engineering-oriented data required for manned-planetary-systems design. In addition, progress has been made in defining the advancements in technology that offer the most promise, and in specifying the basic system parameters to guide advanced technology programs.

Future efforts in manned planetary studies will continue to investigate missions and systems, starting with those capable of utilizing hardware currently under development or contemplated for development. Alternative and contingency modes will be explored and potential missions evaluated with regard to probability of mission success, resources required, and objectives to be accomplished. Studies will continue to refine data on spacecraft weights and configurations and to prepare plans for demonstration of orbital operations that will be required for actual planetary flights. Finally, missions employing advanced concepts will be examined so that program plans will provide for use of those concepts as they become available and so that NASA research may be directed toward the most promising goals.

The approved Apollo program and the supporting missions should represent the beginning of an evolutionary growth toward a manned Mars-landing capability. In mission support, Earth-orbital tests, in addition to Apollo, and extensions in growth of the presently approved program will play a major role. A manned orbital research laboratory (ORL) may be required to answer questions related to operations in space lasting 1 to 3 years. Apollo Applications, or Extended Apollo, should in any case provide experience with men in weightless condition for 45 to 90 days. Although major launch vehicles of the post-Saturn class are anticipated, Earth-orbital assembly, checkout, and possibly fuel transfer could be a major part of the evolutionary development toward a Mars-landing capability.

Unmanned probes, advanced research and technology, and manned Earth-orbital tests are integral precursors of any manned planetary program that could be evisioned. The securing of data on human performance and human requirements is an objective of the Gemini, Apollo, and subsequent manned-flight programs. Based on this evolutionary development an early manned planetary flyby might be planned for the last half of the 1970's with practice planetary missions and a manned Mars landing being realized during the 1980's.

This introduction to possible manned planetary missions and the cursory discussions of some of the problems related to man's presence constitute a status report on NASA's Manned Planetary Study Program. It is evident that many questions remain for theoretical and experimental investigation. However, with the exception of biomedical problems and questions of man's performance in extended space missions, there appear to be engineering solutions that lend confidence to our belief that we can develop a manned planetary-landing program in the not-too-distant future. The immense significance of placing scientists on a neighboring planet needs no elaboration here, and the capabilities of technology to support this goal are much closer than many of us realize.

APPENDIX 5.

STATUS OF KNOWLEDGE OF WEIGHTLESSNESS 1965

Lawrence E. Lamb

Weightlessness is the only known environmental factor involved in manned space flight that cannot be supplied in the Earth laboratory or by other means less difficult than space flight. As shown by many other examples in history, such an unknown factor gives rise to much speculation concerning its influence upon man. Opinion in such instances is often based more on fear than on fact. Study of the effects of weightlessness on man is bound to be even more complex because of the apparent similarity of action of other environmental factors. To date the alteration in cardiovascular dynamics, and particularly decreased orthostatic tolerance, has been one of the major considerations of the effects of weightlessness on man. It is well known that there are many other factors that may influence orthostatic tolerance or other aspects of circulatory dynamics. Many of these factors have been part of the manned space-flight experience to date. One of the factors is inactivity secondary to confinement in a small vehicle or, as in the case of Russian flights, being strapped to a chair. Other factors that have a major influence on cardiovascular dynamics are heat loads, adequate water intake, and excitement and other emotional factors. From this conglomerate of factors it is impossible with the experience to date to state specifically what alterations were secondary to weightlessness encountered during manned space flight and what alterations were secondary to problems in maintaining life-support systems or other features of missions.

Experience gained from any one manned space flight is not necessarily applicable to other flights because of individual variations characteristic of cardiovascular dynamics. It is well known that an individual may be susceptible to fainting one day and not the next, and that while one person may tend to faint with a given stimulus, such as a venipuncture, another person may have no significant reaction to the same stimulus. Thus, human variability greatly curtails the applicability of knowledge gained on any one subject under one set of circumstances to any other anticipated manned space flight. The problem is further compounded by the variation in environmental factors from one type of mission to another. The problems of confinement, fatigue, and mission workloads are not the same from one vehicle to another. For example, the available space within the Mercury vehicle was considerably different from that of the Gemini vehicle and will be different from that of the Apollo vehicle. All these missions have been significantly different in physical characteristics from the Russian vehicles. Psychological factors are also important, and these can hardly be the same, of course, for a single man as for a crew of two or more. All these points are emphasized in an attempt to discourage generalizations based on observations of current manned space flights or observations likely to be made in the immediate future.

If a crew member faints under operational circumstances at the end of a manned space flight, it should not hastily be concluded that the fainting was due to weightlessness, since heat loads, fluid intake, fatigue factors, and confinement are all significant factors and can, by themselves, produce such effects. It should be kept in mind that the causes of syncope may be multiple and are often obscure.

One may make certain assumptions concerning the effects of weightlessness on man and on these assumptions make certain analogies to ground-based study modes that provide some information on the probable influences of weightlessness on man during space flight. The two apparent effects of weightlessness on man are decrease of power requirements associated with body movements and absence of the hydrostatic influences induced by gravity.

The support of body weight and the movement of the body against the gravitational field impose work through time or a power requirement. To accomplish the work, energy must be expended by the working muscle mass. This requires delivery of oxygen by the circulatory system. Thus, the movement of man in the presence of the gravitational field imposes a workload upon the circulatory system. This workload is quite small compared to the full workload capacity of the circulatory system.

A comparison of body oxygen consumption while standing, as opposed to lying recumbent, shows that support of body weight while standing requires more energy while standing than while lying down. The increased oxygen requirement, however, is minimal compared to the total capacity of the circulatory system to provide oxygen to working tissues.

Much of the work entailed in muscular contraction is not associated with weight-bearing or weight movement. One of the energy requirements in contracting a muscle is to overcome the natural viscosity of the muscle itself. The more rapid a muscular contraction, the greater the energy expended in overcoming muscle viscosity. Thus, during running or rapid arm movements, the load imposed on the circulatory system to overcome the viscosity of the muscle mass is in addition to the workload requirement to move the body mass in the gravitational field. The portion of the workload to overcome viscosity is not removed in the weightless or subgravic environment, since it is in no way dependent upon gravity. A workload upon the circulatory system can be imposed during manned space flight, in the absence of weight, by the rapid contraction of skeletal muscles. Rapid contraction of the biceps in a weightless environment will still require an appreciable quantity of energy because of the natural viscosity of the biceps muscle. Movement and locomotion within suits or any form of restricting or cumbersome garments may well require a greater energy expenditure than normal movement and activity on Earth.

Absence of any appreciable body movement would be expected to have the same deleterious effects in space that it would have on Earth. It has been observed in daily life that limited activity or immobilization is not compatible with optimal physiological function and significantly decreases the load upon the circulatory system leading to a series of physiological changes called deconditioning or hypokinesia.

It is generally assumed that, in the absence of restricting garments or other devices, ordinary body movement in space requires less energy because of the absence of body weight. Insofar as this assumption is correct, the workload upon the circulatory system as a transport mechanism for oxygen would be decreased but not abolished in the weightless environment of manned space flight.

The changes in hydrostatic pressure caused by gravity may affect fluids anywhere within the body. Much has been learned about the effects of the changes on circulatory dynamics by the study of changes in fluid pressure, distribution, and flow in subjects in the recumbent state with relatively insignificant influence of gravity on the alteration of hydrostatic pressures in the circulation, and from studies of individuals standing where hydrostatic pressures may significantly influence these aspects of cardiovascular dynamics.

When the subject is recumbent, there is no fluid column of any appreciable height. Thus, hydrostatic pressure secondary to gravity is minimized. With this minimized hydrostatic pressure, the intralumen pressure, volume, and flow are related to inherent factors of the circulatory system, such as cardiac output, vessel size, peripheral resistance, and various reflex mechanisms. This leads to a state in which the intralumen pressure within the large arteries is relatively the same as the pressure observed at the level of the aortic valve, assuming that one is not dealing with any poststenotic or postobstructive area. For purposes of definition these will be called simple cardiovascular dynamics.

When a person stands, the influence of gravity alters the simple cardiovascular dynamics in his body. The pressure at given levels below the heart is increased because of the height of the column of blood. The increase occurs in both the arterial and venous portions. Above the heart the pressure is decreased below that noted at the level of the aortic valve. Under these circumstances cardiovascular dynamics are significantly altered and for purposes of classification may be called complex cardiovascular dynamics. The mechanisms that are changed in the presence of hydrostatic factors, such as simple standing, consist of three major factors—changes in arteriolar tone, changes in distribution of blood volume (venous volume), and alterations in transmembrane circulation.

In the upright posture the peripheral arterioles constrict. This action is secondary to stretch receptors initially stimulated on assuming the upright posture. The closure of the peripheral arterioles or increased peripheral resistance in the lower portion of the body control the distribution of cardiac output or blood flow. The principal action of the closure is to ensure that an adequate portion of blood flow reaches the brain. Without this mechanism most of the cardiac output would flow to the dependent portions of the body and inadequate cerebral perfusion would result even though cardiac output may be normal. The reflex mechanisms activated by posture or gravity under these circumstances are apparently the same ones influenced by other physiological means including exercise, which may require relaxation of peripheral arteriolar tone to increase the distribution of cardiac output to the working muscle groups or constriction of peripheral arteriolar tone to nonacting muscles in the presence of vigorous exercise by some other portion of the body. The latter is illustrated when there is an apparent decrease in blood flow to the resting arm during treadmill exertions, as indicated from venous oxygen content samples obtained from the resting arm during maximum treadmill exertion.

Obviously, any failure in peripheral arteriolar tone would destroy the normal mechanism to maintain appropriate distribution of cardiac output when the subject assumes an upright posture. A straightforward clinical example is that noted after sympathectomy, either surgical or medical. It is important to realize, however, that the body may be compensated for absence of peripheral arteriolar constriction by the use of pressure garments or ace bandages such as are used to prevent fainting in individuals who have recently had sympathectomy or individuals who have a neurological deficit that destroys the necessary reflex mechanisms for increasing peripheral resistance. If the absence of gravity results in inactivation of such reflexes, the pressure garments that seem to prevent fainting in the apparent total absence of the neurogenic reflexes may provide adequate similar protection during space flight if the only deficit secondary to weightlessness were absence or diminution of the activity level of the peripheral arteriolar mechanisms. It must be strongly emphasized, however, that it has not been proved that absence of gravity results in inactivity of the reflex mechanisms necessary for proper peripheral resistance.

A second effect of hydrostatic pressure upon circulation is pooling of venous blood below the heart—a higher percentage of the blood volume is pooled below the diaphragm. In essence, blood flow to the lower regions is decreased by the increased peripheral resistance, and blood volume below the diaphragm is increased due to the pooling effect in the venous reservoir. The amount of pooling of blood is dependent upon venous tone and extravascular muscle tone creating pressure against the distensible venous reservoir. When venous pooling is excessive, and particularly if the pooling represents a high percentage of the total blood volume, venous return is inadequate and simple fainting results.

Of great importance in space flight from an operational point of view is the fact that a single muscle contraction can significantly lower venous pressure or, in effect, significantly decrease the pooling of blood in the venous reservoir. The contraction of the skeletal muscle should be regarded as the pumping of an auxiliary pump to return blood to the heart. Walking or body movements produce a significant alteration in the conditions noted during simple standing. Shortening of the column of blood by the seated posture, crouching, or any other such maneuver, including placing the feet above the head or the head between the knees, greatly reduces any of the hydrostatic influences, including diminishing the volume of venous pooling that may be noted during simple standing without such maneuvers or protective measures. It is normal after manned space flight for the participants to be active, utilizing muscular contraction and changing body position.

The third effect of hydrostatic pressure is a change in transmembrane capillary circulation and, thus, a change in the level of hydration of the body. This process should be considered as one of the probable factors associated with the decreased body weight consistently observed during manned space flight. The migration of fluid to the extravascular spaces is caused by the intravascular pressure being relatively greater than tissue pressure. The plasma proteins exert an effective osmotic pressure of approximately 40 cm H_2O. The difference between the osmotic pressure and the intraluminal pressure represents the force that causes migration of fluid outward. This force is balanced by whatever tissue pressure is present. Well-hydrated tissues may create a significant degree of extravascular pressure. In the venous capillary areas where the arterial pressure is no longer present, the decreased intraluminal pressure in the presence of normal tissue hydration allows the plasma proteins to return fluid to the vascular bed through osmotic influences. During quiet standing, the hydrostatic pressure is markedly increased in both the arterial and the venous systems. The increased intraluminal pressure accelerates the migration of fluid to the extravascular spaces. The large muscle groups will be progressively hydrated as a result of the fluid migration. Muscles enclosed in tight fascial sheaths such as the soleus and anterior tibial muscles will develop an intramuscular tension up to 50 cm H_2O. This tension plus the osmotic pressure (50 + 40 = 90) is sufficient in the average individual to counterbalance the hydrostatic pressure caused by standing and bring a cessation of fluid flow to the extravascular spaces in those muscle groups at the level of tissue tension.

In the subcutaneous tissue areas and muscles not bound by tight fascial sheaths, it is not possible for the same degree of tissue tension to develop, and in these areas migration of fluid to the extravascular space has been observed to occur progressively for up to 2-1/2 hr. It has been postulated that this migration would continue indefinitely were it not for the occurrence of circulatory collapse with such prolonged periods of standing.

Normal ambulatory activity in the earth environment results in increased hydration of the dependent tissues. This aids in maintaining tissue tension and, to some extent, in counterbalancing the loss of intravascular fluid during standing. The accumulation of fluid in the tissues also maintains tissue tension by acting as an external pressure garment about the venous reservoir. It is likely that the tissue tension associated with hydration is of greater importance in providing pressure about the venous tree than is muscle tone in the relaxed muscle.

When the recumbent posture is assumed after normal periods of ambulatory activity or standing, the accumulation of extravascular fluid and increased tissue tension that occurred during this activity are no longer needed. The excess pressure created by overhydration of the extravascular tissues results in increased migration of fluid into the intravascular compartment, until tissue hydration has significantly decreased consistent with simple cardiovascular dynamics in the recumbent state. The migration of the excess fluid to the intravascular spaces and the subsequent fluid loss through diuresis brings the acute loss of body water and acute reduction of body weight noted during simple recumbency and observed after orbital space flight of significant duration.

Ground-Based Study Modes

Attempts have been made to study the theoretical implications of the influence of gravity upon cardiovascular dynamics in the laboratory. Perhaps the most successful study has been with the use of simple bed rest. In the prone position most of the hydrostatic influences are removed since no major fluid column has a significant height. In this sense it would be expected that the circulatory dynamic changes in bed rest would be similar to those noted during weightlessness. The body is supported by the bed and requires no major muscular effort. Thus the power requirements of the body and the workload upon the circulatory system are minimized. This, too, simulates what would be expected in weightlessness. Bed rest also induces an element of confinement, noted as part of current manned space flights, the effects of which should not be attributed to weightlessness.

Water immersion has been used for these studies. One of the reasons for its early use was the support of the body by the buoyancy of the water. It was learned early that water immersion was associated with significant diuresis, even in individuals who were seated in a tank of water in which other environmental factors such as temperature had been relatively well controlled. Water immersion of this type creates an external pressure on the body and contracts the venous reservoir. This leads to a series of processes causing acute diuresis with resultant dehydration. In this regard water immersion causes some similar effects to the events noted in weightlessness. Short-term water immersion is more closely akin to acute dehydration and has no significant influence upon muscular, skeletal, or hemopoietic activities that are related to longer-term environmental changes. Acute dehydration does not permit the more gradual development of adaptive mechanisms that might occur under a more physiological state. Short-term water immersion should be regarded as a ground-based study mode that has its principal effect on salt and water metabolism.

Immobilization of subjects in chairs to maintain normal hydrostatic influences provides a means for studying long-term effects of inactivity. The only similarity of this condition to weightlessness is in the decreased power requirements in weightlessness, which may be duplicated to some extent in the seated position. This study mode also relates to the problems of confinement in manned space flight and conventional aerial flight, independent of the environmental influences of weightlessness.

Studies of subjects with reduced levels of activity in oxygen-enriched environments have been conducted in space-cabin simulators with 5 psi and 100 percent oxygen. Prolonged confinement to a small area with a gaseous environment, currently used by U.S. space vehicles, provides an opportunity to study these combined events independent of weightlessness in producing physiological changes.

The Syndrome of Deconditioning

Blood Volume. Using the ground-based study mode of recumbency during simple bed rest as analogous to weightlessness, one may construct the pattern of circulatory functions—simple cardiovascular dynamics—to be observed during manned space flight. The concept of Gauer and Henry on recumbency diuresis is sufficiently well substantiated to be advanced as the most likely explanation of the mechanisms associated with diuresis. In the recumbent posture, the increased venous return to the heart leads to distension of the left atrium. Through reflex action, initiated by stretch receptors within the left atrium, the secretion of the antidiuretic hormone is inhibited. This results in a rapid diuresis. The plasma volume continues to be augmented by the flow of water from the extravascular tissues into the vascular bed. When a subject becomes ambulatory his superhydrated tissue no longer requires this tissue tension, since the increased intravascular hydrostatic pressure that resulted in the accumulation of tissue tension has been removed. The maintenance of the blood volume by progressive tissue dehydration continues to maintain left atrial distension and promote diuresis. The effect then is to decrease significantly the level of hydration of the entire body, which is shown by the increased urinary output and the significant decrease in body weight.

It has been observed on acute studies that recumbency diuresis occurs principally within 24 hr and in almost all subjects within 48 hr. It is commonly accompanied by a decrease in body weight of 1 or 2 kg. Larger decreases in body weight have been noted in some subjects. The resultant decrease in hydration is also associated with a decrease in plasma volume. The average decrease in plasma volume in healthy young subjects is 500 ml. Once the initial decrease in hydration has occurred the fluid balance may be relatively constant, similar to that noted during standard ambulatory states. Body weight may remain relatively steady and plasma volume will not be significantly altered for at least 4 weeks. The acutely decreased hydration effectively decreases blood volume, and, of particular importance, it decreases tissue tension. Both blood volume and tissue tension are significant in maintaining orthostatic tolerance. Accompanying the acute decrease in hydration and loss of water from the circulating blood volume is hemoconcentration, manifested by an increased hematocrit. If bed rest is continued, the hematocrit gradually falls. This is thought to be due to a progressive decrease in red-cell mass. This is a gradual process and, after 4 weeks of simple bed rest, normal human subjects have an average loss of 300 ml of red-cell mass. If this decrease process continues through longer periods of bed rest, on which data are not now available, the normal ratio of plasma to red cells would probably be established and a normal hematocrit, similar to that noted during the ambulatory state, would probably be observed. The decrease in red-cell mass, accompanied by the initial loss in body water and decreased plasma volume, results in an average decrease in blood volume in normal healthy subjects of approximately 800 ml over a 4-week period.

When normal ambulatory activity is resumed, there is a return to the level of hydration needed in the presence of complex cardiovascular dynamics. This is associated with a sudden increase in body weight and a return of plasma volume to a normal level. This results in hemo-dilution. In addition, there is a consistent acute decrease in red-cell mass. This is consistent with the earlier observations of Broun demonstrating an acute lysis of red-cell mass in caged dogs when they resumed physical activity. Broun demonstrated that this cell loss could be as much as 12 to 30 percent of the red cell mass. Other usual features associated with acute loss in red-cell mass were also demonstrated, such as the increasing of the reticulocyte count during the regeneration period of red cells. Broun postulated that levels of physical activity correlated with levels of daily destruction of red-cell mass - that, on a daily basis, inactivity (he was not speaking of weightlessness) was accompanied by a decrease in destruction of red cells and a decreased requirement for daily replacement of red cells by bone marrow. He demonstrated that the bone marrow of caged inactive dogs was pale and yellow as compared to the red cellular marrow of physically active animals. Initiation of vigorous exercise of the confined animals caused an acute destruction of red cells that their bone marrow was not prepared to replace, and as long as 3 weeks were required for the bone marrow to adequately replace the red-cell mass of such animals. Similar decreases in blood volume of approximately 15 percent have been consistently observed in studies with human subjects during the ambulatory phase following periods of bed rest of 4 weeks. This, combined with the expansion of plasma volume, results in hemodilution with a marked decrease in hematocrit values.

The loss of body water with recumbency, with its associated decrease in body weight and plasma volume, can be incurred by any of the other mechanisms that contribute to dehydration, including heat loads, inadequate water intake, and any of a number of factors, such as emotional reactions, that may inhibit antidiuretic hormone action. The problems of fluid intake, heat load, inactivity, and weightlessness, all of which have been observed in manned space flights, have one common end result—dehydration. This result is known to alter significantly cardiovascular dynamics and specifically to decrease orthostatic tolerance. Dehydration from any cause may result in significantly decreased orthostatic tolerance. The cause need not be weightlessness.

The observations on recumbency and the general observations on manned space flights suggest that the initial physiological changes during orbital flight will show a general decrease in hydration level and, within a 48-hr period, the changes due to weightlessness. It is assumed that weightlessness, like recumbency, removes the necessity for high-level tissue hydration. Weightlessness, then results in an increased hydrostatic pressure below the diaphragm, which causes a left atrial distension and the physiological actions of recumbency duiresis. Once the new homeostatic level for hydration has been achieved, provided the life-support system does not create heat loads and there are no problems of fluid intake or any of the other mechanisms that influence diuresis, it should remain at a relative plateau.

Results from chair-rest studies indicate that, after 10 days of chair rest, a subject has a significant decrease in plasma volume. This is true even though hydrostatic pressure is present. This decrease, along with the greater plasma volume in athletic individuals, indicates that activity level, independent of hydrostatic factors, may, on a long-term basis, influence plasma volume. Whether the lack of activity along with the absence of hydrostatic factors further decreases the hydration level remains to be established. Current studies have demonstrated that the major decrease in plasma volume occurs within 48 hr. There has been little evidence of further change during the course of 4 weeks of simple bed rest.

Recent studies with lower-body negative pressure have demonstrated that this pressure can return the level of hydration to the ambulatory level on a short-term basis. This has major implications in the management of the hydration problem for prolonged manned space flights.

Resting Heart Rate. Initially the heart rate during bed rest is not altered significantly; in some instances it may become slower. As bed rest is continued, the heart rate gradually increases. The Taylor and Keys Minnesota study of 1948 indicated that afternoon pulse rates are of greater use in detecting this variation resulting from bed rest than are morning basal heart rates. The changes in heart rate are undoubtedly secondary to other factors that are altered during prolonged inactivity. It is not likely that heart-rate change is directly associated with the decrease in blood volume since decrease may occur acutely, as described above, and the resting heart rates are often not significantly increased until the third or fourth week of bed rest.

Heart Size. Heart size has been observed to decrease after prolonged bed rest. It is not certain either whether this is due to orthostatic influences or whether it also occurs in the recumbent state since, insofar as is known, all x-ray studies of heart size have been done with upright-chest film. Increased venous pooling after bed rest could significantly decrease heart size. It is generally accepted, however, that markedly insufficient physical activity is associated with decreased heart size and, in extreme degrees, cardiac atrophy. This is the opposite of the athletic heart, which may be dilated and have a relatively large diastolic volume. The changes in myocardial compliance associated with activity levels have not been studied sufficiently. Myocardial compliance is of considerable importance in cardiovascular physiology associated with congenital shunts and septal defects. The large dilated heart of an athlete that is capable of peak performance logically is associated with a low level of viscosity, which decreases the work required of myocardial contraction. A relatively nonpliable myocardial tissue of the small inactive heart could significantly decrease the work of the heart muscle in achieving cardiac output. Since these factors have not been carefully studied under optimal circumstances within the laboratory, knowledge of their probable changes in an environment of weightlessness is nonexistent; information is too scanty to permit any safe prediction, particularly with reference to time or to establishment of homeostatic levels.

Coronary Blood Flow. It is reasonably well established that the level of regular physical activity may influence significantly the capacity to increase coronary blood flow. In general, athletes engaged in endurance-type activities are presumed to have the capacity to develop a large coronary blood flow. The physiological basis for this is that since the heart muscle extracts almost all the oxygen from the circulating blood during rest, the only apparent way to increase oxygenation of the working myocardium significantly is to increase coronary blood flow, and the only apparent means of accomplishing this is by dilation of the coronary vascular bed. In contrast, the inactive, sedentary individual who does not require regular expansion of his coronary vascular bed is thought to have a coronary blood flow inadequate to meet any major increased workload requirement for increased oxygen delivery to the myocardium. Animal experiments of coronary blood flow in dogs and anatomical studies in dogs and rats have supported this physiological concept. The capacity to expand the coronary blood flow is not likely to be impaired on a short-term basis, but during long periods (months) of physical inactivity, such as may be encountered during weightlessness and confinement to a space vehicle, impairment may be a consideration.

Blood Lipids. Population studies on astronaut candidates, space pilots, athletes, and aircrew members at the School of Aerospace Medicine have shown a significant correlation between values of blood lipids and levels of physical activity. In general, those individuals, such as astronaut candidates, with high physical fitness tend to have significantly lower blood-cholesterol values. Not infrequently the values may be one-third less than those noted in their less physically fit counterparts of the same age group. These observations and other independent observations have also demonstrated that physical fitness correlates with a favorable lowering of triglyceride levels. Some authorities suggest that physical activity must occur as often as every second day to maintain a favorable influence on triglyceride levels. Insofar as these levels are significant in the problems of coronary insufficiency and atherosclerosis, they are important in long-term space flight. Data on coronary-artery disease for study in relation to weightlessness and prolonged inactivity can be gathered from health statistics of the American population. Coronary-artery disease has reached an epidemic proportion exceeding that of any other disease in the history of mankind. Nearly a million individuals died from heart disease in the United States in 1963. The vast majority of these deaths were from coronary-artery disease. A great number of people had clinical episodes of angina or coronary occlusion and recovered. Autopsy studies on individuals in their twenties have shown a high incidence of coronary-artery disease. Over 70 percent of tested individuals at an average age of 22 years demonstrated gross evidence of coronary atherosclerosis. In the age group of individuals commonly engaged in manned space flight, it may be presumed that anatomical evidence of coronary-artery disease is present in all. The complications of the disease are most apt to occur in the adult American male, but they may occur in all age groups and have been noted with increasing frequency in the age group below 35 years. Thus, more consideration must be given to the problem of coronary-artery disease in the age group commonly entering into space-flight activity.

Catecholamine Products. Raab and associates have pointed out that physical inactivity is accompanied by a storage of catecholamine products in the myocardium and that physical activity tends to metabolize these products and remove them. Catecholamine products decrease the ability of the myocardial cells to utilize oxygen efficiently. They result in increased myocardial irritability with an increased propensity toward arrhythmia. It is possible that storage of these products is in part responsible for the increased heart rate noted with prolonged inactivity and bed rest.

Electrolyte Metabolism. The most important aspect of the electrolyte metabolism is calcium mobilization as observed during bed rest, particularly in patients with ailments such as fractures. Dietrick and associates demonstrated on four subjects in 1948 that calcium mobilization gradually increased to a peak level during the fourth and fifth weeks of bed rest. This meant literally that during the first three weeks the levels of calcium excretion were not high, and, as Dietrick and co-authors pointed out, repeat levels of calcium excretion in healthy individuals during bed rest was appreciably below those values noted in fracture cases and appreciably below the levels thought to be associated with the formation of renal stones. On the basis of clinical observation it is deemed unlikely that with normal hydration or normal urinary excretion there would be any stone formation in less than 10 weeks. In the study of 72 subjects for two 2-week periods and study of more than 50 subjects at bed rest for over five weeks at the School of Aerospace Medicine, no evidence of renal stones has been observed. At 4 weeks the mean values for the peak levels of urinary calcium excretion have been as follows:

Control week	I	114 mg/d
Control week	II	109 mg/d
Bed rest	I	156 mg/d
Bed rest	II	225 mg/d
Bed rest	III	255 mg/d
Bed rest	IV	273 mg/d
Recovery	I	243 mg/d
Recovery	II	170 mg/d
Recovery	III	142 mg/d

Calcium excretion appears to be constant during simple bed rest once the plateau has been achieved. This implies gradual and progressive demineralization of the skeleton. At this writing, an end point beyond which further decalcification would not occur cannot be established.

Muscle Tone. Decrease in muscle girth and muscle strength was demonstrated in the initial studies by Dietrick and associates and by Taylor and Keys in 1948. General loss of muscle strength and size has been repeatedly observed in bed rest studies. Of operational significance is the pain occurring during ambulation after prolonged periods of inactivity. This is particularly evident during treadmill walking or simple standing. Apparently the foot muscles normally active in providing arch support are sufficiently weakened so that simple standing can bring physical pain. This could seriously impair an individual's capacity to walk, stand, or run after prolonged periods of manned space flight up to 4 weeks. The shoes provide support, but even this is not sufficient during treadmill exercise. Foot pain on simple standing can contribute to decreased orthostatic tolerance. This factor, of course, is not present during tilt-table studies in which there is no weight bearing. Muscle tone is of considerable importance to the vascular system in that it provides some external compression to the venous reservoir and prevents pooling of too large a portion of the total blood volume in the dependent areas.

Orthostatic Tolerance. Orthostatic tolerance is decreased by simple bed rest and low levels of physical activity. In practical terms for manned space flight, the principal question on orthostatic tolerance is whether or not decrease in orthostatic tolerance after manned space flight will compromise mission ability. In the study of this problem, there are no mission counterparts to the studies with tilt tables and rigid standing, leaning against the wall. These tests are strictly innovations for studying physiological responses that have become part of the program used by medical personnel and not dictated by mission requirements.

During the descent phase of the Gemini vehicle, for a period of less than 6 minutes the individual is in the seated position. The length of the column of blood in the seated individual is sufficiently less than that of the standing individual. The hydrostatic pressure associated with gravity in the seated position is far less than in the standing position. Consequently, the degree of adaptation necessary to maintain adequate cerebral blood flow is significantly below that required for standing or hanging from a tilt table. In the final state of the Mercury flight, the individual descended on his back and, once the vehicle landed, was able to lie prone in the capsule. In the Gemini vehicle the individual is able to get his feet above his head or can place his head between his knees. Any of these maneuvers enables one to favorably influence the height of the column of blood and hydrostatic pressure factors in a manner not possible with tilt table or simple standing procedures. Body movements possible during this time enable skeletal-muscle contractions to augment venous return and significantly diminish any of the venous pooling factors associated with possible fainting. These maneuvers, normal to the operational aspects of the postflight period, significantly decrease the probability of actual fainting.

Severe dehydration caused by heat load or other factors or severe muscular weakness might lower capability. It is important to emphasize that the responses on tilt-table testing after bed rest or after manned space flight and the responses after simple standing should not be directly equated to mission requirements.

The loss of tissue tension, muscle tone, and perhaps venous tone enables the pooling of a greater percentage of the blood volume in the dependent portion of the body. This may contribute to decreased orthostatic tolerance. The decrease in blood volume associated with dehydration on short-term flights and with dehydration and decreased red-cell mass in prolonged bed rest contributes to the increased blood volume pooled below the diaphragm and thereby decreases orthostatic tolerance. The role of peripheral arteriolar tone is less clear, since even increased peripheral resistance may not be adequate to provide sufficient blood flow to the brain when there is a significantly reduced cardiac output. Peripheral resistance may best be described as inadequate for the total hemodynamics changes that have permitted pooling of an excessive percentage of the blood volume in the dependent portions of the body.

Decreased orthostatic tolerance brings a drop in systolic pressure with narrowing of the pulse pressure and an initial increase in heart rate. This may be followed by a simple faint. In some instances immediately preceding the faint or at the time of its occurrence, a relative bradycardia ensues in the face of falling blood pressure and decreasing pulse pressure.

Exercise Tolerance. The ability to perform work is usually diminished after prolonged periods of bed rest and, by analogy, would seem to be diminished after prolonged manned space flight. This diminished ability after bed rest has been demonstrated by increased heart rates after the simple double Master's exercise-tolerance test as compared to results of similar studies prior to bed rest. It has also been demonstrated in treadmill studies in which the time to reach the level of maximum exertion is considerably less after prolonged bed rest than before. Also, the value for the maximum oxygen consumption during peak exertion is significantly lowered after bed rest.

Protective Measures

The practical protective measures against the adverse effects of the deconditioning syndrome are well suited to operational requirements. Simple muscular contraction and changing of body positions by crouching or by elevating the feet are measures that protect significantly against any adverse change in cardiovascular dynamics or against the tendency to faint. On the tilt table, the use of the g suit pressure garment has proved to be adequate to prevent fainting after 4 weeks of bed rest. The external pressure prevents excessive venous pooling and thereby maintains adequate venous return to the heart. The external pressure probably prevents the continued excessive loss of plasma volume to the relatively dehydrated tissues. Any effective pressure garment, such as the Jobst stocking, that can be applied at the end of space flight will provide a large measure of protection against possible significant decrease in orthostatic tolerance. If the g suit is not acceptable for mission requirements at that point in the flight, the Jobst stocking or elastic leotard, which can be adequately ventilated and permit normal body movement, could easily be slipped on immediately prior to deorbit. In such a garment one could swim, walk, or perform normal activity.

Exercise. Although physical exercise may counteract some of the adverse influences of inactivity, it is not likely to counteract them all. It is one means by which an adequate workload may be placed upon the circulatory system. Unfortunately, if an adequate level of physical fitness is to be maintained, exercise must be daily and would require large portions of crew time. For these reasons it is not likely that physical activity alone will provide an adequate means of protection against the changes noted in prolonged space flight. Exercise will not prevent the decrease in hydration noted during recumbency since it cannot replace the needed hydrostatic factors related to fluid migration across the capillaries. Extensive exercise would require a prohibitive period of crew time in the mission schedule. The most practical purpose of muscular activity in manned space flight is maintenance of muscle tone. To this end simple isometric exercises are useful. The extension and flexion of the foot muscles supporting the arch and toes are important for bed-rest subjects in the prevention of foot pain on resuming ambulatory status. A regular set of exercises to maintain muscle tone in the lower portion of the body, used for a short-term basis of perhaps 10 to 15 min four times a day, could be adequate to maintain muscle tone. These periods of time would not significantly interfere with mission requirements. These levels of physical activity are not sufficient to influence markedly other factors of the deconditioning syndrome.

Hypoxia. Studies at the School of Aerospace Medicine have demonstrated that moderate hypoxia is effective in reversing part of the deconditioning problems. At simulated altitudes of 10,000 ft, the decrease in red-cell mass noted with 4 weeks of bed rest is obviated and, at 12,000 ft of altitude, the red-cell mass is increased. Hypoxia during bed rest appeared to be a specific agent in the prevention of loss of red-cell mass and, inferentially, in the prevention of decreased activity of bone marrow and erythropoietic action. In eight hypoxic subjects calcium excretion appeared to be substantially the same as that noted in bed-rest subjects through a period of 3 weeks. During the fourth week, however, there was a decrease in the level of urinary calcium excretion from the previously attained peak value at the third week. Four weeks of bed rest at 12,000 ft simulated altitude produced minor change in urinary excretion of calcium of the hypoxic subjects compared to when they were ambulatory; six of these subjects had no important increase in calcium excretion at bed rest over the ambulatory phase. It is not known whether this result would be noted for longer periods of exposure to hypoxia or whether the same effect would be observed in the presence of acclimatized individuals. In the event that hypoxia might be used in manned space flight, it would be well to study bed-rest modes in acclimatized subjects, both on the ward under normal atmospheric conditions and during simulated altitude. Acclimatized subjects would have the obvious advantage of removing any possible factors associated with acute hypoxia in the unacclimatized individual.

In addition to its demonstrated effect on red-cell mass and calcium, hypoxia is known to create a workload on the circulatory system. It increases cardiac output, it dilates the coronary artery as does exercise, and, when combined with physical exertion, augments the workload on the circulatory system. For these reasons, hypoxia may have a place in prolonged manned space flight in maintaining erythropoietic action and preventing decrease in red-cell mass, in preventing mobilization of calcium, and in making effective workloads of exercise in the weightless environment. Hypoxia has no favorable influence upon the level of hydration or plasma volume.

Lower-Body Negative Pressure. Lower-body negative pressure may be used to suck blood by vacuum to the lower portion of the body. A relative vacuum in an airtight box enclosing the lower half of the body has effects similar to standing. Since the levels of negative pressure needed are relatively low (-30 mm Hg), the principal effect would be on the venous reservoir and upon transudation of fluid across capillary membranes. Studies of individuals during exposure to lower-body negative pressure have illustrated that its action is similar although not exactly the same as simple standing or tilt-table studies. One of the differences is probably related to the absence of a pressure gradient from heart level to the base of the skull during lower-body negative-pressure application in the recumbent subject as opposed to the subject hanging on a tilt table or standing.

A recent study of salt and water metabolism with the use of lower-body negative pressure demonstrated that as little as -30 mm Hg negative pressure for the lower portion of the body will rehydrate a subject to levels equal to or above that in the ambulatory state. This may be done acutely in a period of 2 days—water is retained, body weight increases acutely, and plasma volume returns to normal or above. This is of particular importance in terms of prolonged manned space flight, for it means that an individual with relative decrease in hydration and all its attendant implications to orthostatic tolerance may have his hydration level acutely improved immediately prior to deorbit. It would not make any difference whether the flight were of 5 days or 5 months duration, since the rehydration process could be accomplished on a short-term basis immediately prior to deorbit. This is entirely analogous to the clinical situation in which gradual ambulation at the end of prolonged bed rest will in a relatively short time return an individual's orthostatic tolerance to normal levels. Lower-body negative pressure apparently does not have any favorable influences upon red-cell mass, bone marrow function, or calcium metabolism. Its action on muscle tone or tissue tension would be complicated with its action in increasing the level of hydration. The improvement in blood volume and tissue hydration complicates clear-cut understanding of the effects of lower-body negative pressure on venomotor tone and arteriolar tone independently of these factors.

Operational Significance

On the basis of available information, it does not seem likely there will be any major physiological problems during prolonged manned space flight for the orbital phase. The two most likely problem areas secondary to weightlessness are the formation of renal stone or the progressive decalcification of the skeleton. The low plateau values for calcium suggest that stone formation is not very likely. The problem potential of demineralization of the skeleton is not clear. It is known from clinical experience, however, that complete immobilization for long periods of time during body casting does not cause decalcification of such a degree as to hinder fracture healing or to cause insurmountable difficulties in an otherwise healthy individual. At this writing, the only practical means that has been found to prevent calcium mobilization in the recumbent individual appears to be hypoxia. There is a possibility that various chemical approaches may prove to be beneficial. Body compression and work efforts have yielded no results that have practical application to mission requirements at this date.

Another consideration for operational problems is subject tolerance to g load for deorbiting. Insofar as 4 weeks of bed rest is analogous to weightlessness, normal subjects can tolerate transverse g loads considerably in excess of those currently employed in manned space flight or any levels anticipated for future manned space flight. After 4 weeks of bed rest, subjects exposed to 10.6 $+G_X$ acceleration had no major changes from values observed prior to bed rest. These tests indicate that manned space flight up to 4 weeks' duration should not pose any g load tolerance difficulties for re-entry or deorbit.

In the immediate post-flight period, the problem of orthostatic tolerance or tendency toward fainting is a major consideration. The use of normal protective measures such as muscle contraction, body positions, and maneuvers will add greatly to the protection of the individual. Available data suggest that these alone are probably sufficient for subjects exposed to up to 4 weeks of space flight, provided the life-support system does not impose excessive heat loads or other causes for dehydration. Once returned to Earth, if the individual has not yet achieved normal postural adaptation, gradual ambulation can be employed. A host of other protective devices are available, including the pressure garments such as g suits or elastic leotards. Finally, lower-body negative pressure can be used immediately preceding deorbit to rehydrate the individual to levels consistent with ambulatory activities. If necessary, one could use a combination of isometric exercises for maintenance of muscle tone, hypoxia for maintenance of red-cell mass, and lower-body negative pressure for maintenance of appropriate level of hydration, coupled with appropriate body maneuvers and application of pressure garments—all as means of protecting against significantly decreased orthostatic tolerance. No doubt other protective devices can and will be developed for selective use. Nevertheless, an impressive array of means to improve orthostatic tolerance exist today, and suggest that this problem can be managed for extended manned space flight of several months.

Conclusion

Available information indicates that the initial physiological changes during manned space flight are related to dehydration. From knowledge of recumbency diuresis and decrease in the hydration level with recumbency, it may be presumed that weightlessness is a contributory factor in dehydration. The occurrence of dehydration in recumbency, and presumably initially during manned space flight, is an acute self-limiting process that is rapidly reversible when normal ambulatory earth activities are resumed. It can be sharply reversed by short-term application of lower-body negative pressure. A host of other factors are associated with prolonged deconditioning or inactivity, which have not yet been identified with a point in time for a new homeostatic level of adaptation. These include biochemical alterations, coronary blood flow, and skeletal demineralization. Information has been obtained on tolerance to $+G_x$ acceleration that suggests that tolerance to transverse g loads will not be a significant problem in manned space flights up to 4 weeks' duration, and since the g levels employed have been appreciably in excess of those anticipated for manned space missions, it may be presumed that this time limit may be extended somewhat further.

It is known that the immediate post-flight events permit the use of a number of physiological maneuvers that provide protection against decreased orthostatic tolerance and that, in this sense, the operational environment is not analogous to the responses noted from tilt-table or simple standing studies. These maneuvers may offer major protection against post-flight decreased orthostatic tolerance. It is known, and has been demonstrated, that the g suit or other pressure garments can be used to provide significant protection against decreased orthostatic tolerance. Preliminary evidence suggests that the acute improvement in hydration brought on by short-term application of lower-body negative pressure abolishes the problem of decreased orthostatic tolerance after bed rest. All these factors suggest that sufficient knowledge now exists to ensure that man would be physiologically safe on manned space flights for periods in excess of 1 month if available protective devices were employed. Additional information suggests that even considerably longer space flights may be accomplished safely with proper use of hypoxia, isometric contractions for maintenance of muscle tone, acute short-term application of lower-body negative pressure in the presence of adequate salt and water intake, and, if need be on landing, pressure garments. The rapid growth and knowledge of ways to protect man against significant complications of deconditioning in relation to operational requirements suggest that the problems imposed by weightlessness during long-term space flights of many months can be met successfully by the time the capability to carry out such flights exists.

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APPENDIX 6.

REPORT OF PANEL ON CIRCULATION AND RESPIRATION

J. T. Shepherd, S. O. Bondurant, and Norton Nelson

Some general comments can be made with regard to needed experimental work relating to the blood, circulation, and respiration, particularly concerning the poorly understood responses to stresses of possible consequence in longer space missions. Parenthetically, understanding of the pattern and mechanisms of these responses will, in a number of instances, add significantly to the general fund of biomedical knowledge.

Data from the current experiences with man in space permit some estimation of the effects of the space environment on those components of the circulatory system having controls that respond rapidly or have short-time constants. While the information is extremely limited and has not been systematically collected for this purpose, it supports the judgment that these systems respond in ways that are familiar and will not limit man's effectiveness during longer space flights. There are some data on the effects of simulated prolonged space flight on the circulation, but the validity of the simulations is imperfect. There are no data that bear directly on the effects of the space environment on those circulatory processes that have long-time constants, including, for example, the adrenal and vascular catecholamine stores and smooth-muscle mass.

The various components of the demonstrated orthostatic instability resulting from the combined inactivity and weightlessness of space flight (or inactivity and water immersion) do not all have the same time pattern of development or disappearances. Apparently the decrease in plasma volume occurs quite rapidly, being almost complete in a day or two. The change in red-blood-cell volume is considerably slower—on the order of some weeks—and appears to be related to red-cell lifetime. Venous tone and vasomotor control are perhaps the most critical aspects of the orthostatic instability that follows weightlessness; neither the basic physiological mechanisms nor the time patterns is well understood. Obviously, if the deterioration of venous tone continues indefinitely, a severe handicap would be imposed on extended missions. Resolution of these uncertainties can be aided by ground-based investigations in close coordination with studies in orbit. Two suggestions emerge: (1) the validation and refinement of ground-based stresses that simulate weightlessness, e.g., bed rest and water immersion, and (2) support of fundamental research on the underlying physiological processes involved. These are likely to provide the quickest route to the understanding and evaluation of responses to stress, to time factors of incidence and return to normal, and to antidotal or control procedures.

Other factors that may respond to weightlessness or inactivity merit consideration, including hematopoetic activity, size of heart and coronary arteries, catecholamine metabolism, and general muscle tone.

It is well recognized that the cardiovascular system can be conditioned to perform optimally in various dissimilar situations. Perhaps the circulatory conditioning of the athlete (large heart, slow pulse) is most familiar. It is not certain, as has been assumed, that athlete conditioning is the best preparation for the space environment. There are sufficient differences among the circulatory characteristics of various kinds of physical conditioning to justify a systematic effort to determine the best means of preflight and in-flight circulatory conditioning.

Monitoring Techniques

The cardiovascular and pulmonary monitoring systems of the Mercury, Gemini, and Apollo programs are intended to show the condition of the astronaut at all times, and these systems are adequate for that purpose. However, resting heart and respiratory rates have little value in predicting circulatory response to physical stress. With the development of longer and more complex missions—during which man will be required to carry out extensive physical work, to land and depart from other planets, and to decelerate and re-enter the Earth's gravitational field after long and variable experiences in space—it will be important to have circulatory monitoring techniques that allow continuing quantitative prediction of the circulatory response to every anticipated flight activity and to possible emergencies.

Such techniques are not presently available, nor is the current understanding of circulatory physiology sufficient for the development of such techniques. There is, however, in both sports medicine and clinical medicine, sufficient information from various stress tests that estimate circulatory reserve to indicate that circulatory monitoring techniques of predictive value can be developed. Among the types of tests that may prove useful are those that provide quantitative mechanical or biochemical measurements of resting circulatory function, which correlate with circulatory function during flight activity, such as of catecholamines and angiotension. It is more likely that precise quantitative simulation of some aspects of the expected activity can be used as a forcing function while critical measurements of the responses of the circulatory system are made, both for correlation with current resting state and for prediction of response during future similar activity.

APPENDIX 7.

REPORT OF THE PANEL ON METABOLISM AND NUTRITION

C. O. Chichester, R. L. Lawton, and G. V. LeRoy

Water and Calcium Balance

In all the space flights to date there has been a significant decrease in the weight of the astronauts, which, it can be assumed, is due primarily to loss of water. In the Gemini IV flight there was no malfunction of the cooling system, so high in-flight temperatures can not be held responsible for the weight loss. It is impossible, however, to ascertain when during the flights the weight loss—or water loss—occurred. Since severe dehydration is rapidly debilitating, the determination of water requirements and state of hydration under flight conditions is urgent.

Since negative water balance is ordinarily accompanied by loss of one or more electrolytes, every study of water loss should include measurements of changes in electrolyte concentration in urine, blood, sweat, etc. The proposed experiments on electrolyte balance are needed to provide a base line for forecasting changes that may occur during longer flights. Thus, it is obvious that a reliable mechanism for the collection of urine samples must be developed. It is necessary to have information on total urine volume and the times at which urine is voided. For complete information, measurements are also required on the water and electrolyte content of sweat and feces.

The experiments proposed to measure calcium balance are urgently required even though the results may be difficult to interpret because a negative calcium balance can occur under conditions other than inactivity. For example, anxiety may be related to any increased excretion of calcium in urine, and the calcium balance even of normal active subjects may alternate between negative and positive in weeks-long cycles. Nevertheless, satisfactory in-flight measurements of calcium metabolism should give some indication of the validity of extrapolating results from ground-based bed-rest or water-immersion studies.

Metabolic Requirements

The metabolic energy requirements for space flight have not been determined in either simulated or actual weightlessness. In order to determine food and oxygen requirements, in-flight oxygen consumption should be measured as soon as possible.

To date, no studies have been performed on astronaut candidates to identify their individual nutritional requirements. It is well known that individual requirements for nitrogen, essential amino acids, and vitamins may vary by a factor of 2 or more among a group of healthy adults. If the astronauts exhibited comparable variations and yet were maintained for long periods on the standard National Research Council nutritional allowances, difficulties would surely occur even during flights of moderately long duration. It should be noted that, under protein deprivation, mental acuity declines within a comparatively short time.

Artificial Diets

Owing to individual variations and to the fact that total human requirements for trace minerals and accessory nutritional factors are not accurately known, the use of completely artificial diets cannot be recommended before they have been unequivocally proved out for periods of time at least as long as the projected flights. Even if such studies should be made, the use of completely artificial diets should be viewed with caution because of individual variability. In addition, there is some evidence that the requirement for certain amino acids is higher when they are taken as part of a liquid diet than when the same amount is consumed in solid natural protein foods.

The psychological problem associated with monotonous or highly formulated diets over a long period of time is a serious one. There is ample evidence that a diet of this type is extremely undesirable even in highly motivated subjects. These facts, however, should not preclude the use of formula diets for short periods in the metabolic experiments that are needed to plan long flights. In summary, the proposed use of artificial diets for long space flights should be approached with caution for nutritional as well as psychological reasons.

Diets and Intestinal Flora

Any protracted disturbance of bowel function during flight could be catastrophic. Diets selected for flights must therefore be thoroughly tested by individual astronauts prior to a mission. Care should be taken in the preparation of diets to avoid conditions that would lead to a drastic change in intestinal flora. Completely sterilized foods, for example, could lead to serious consequences if an astronaut's flora mutated as a result of absence of customary food contaminants. Because of the restricted size of space vehicles, interchange of microflora among astronauts would probably occur; dangerous pathogens could become dominant and be exchanged. In several simulations involving four to six subjects, serious difficulties of this type have been encountered. It may prove to be desirable to supply astronauts in flight with regular rations of bacteria to maintain optimal or normal conditions in the intestinal tract.

Life-Support Systems

In planning the design of future space vehicles, designers should give early consideration to problems of food storage, food handling, and management of food wastes. In present vehicles it appears that such facilities have been added as an afterthought. The effect of unsatisfactory or inefficient design of food-handling facilities could well impair a long flight and have profound psychological effects on astronauts.

At the present time and with the present state of knowledge, a completely closed ecological system does not seem feasible. The operation of all such systems poses numerous problems other than the efficiency of oxygen regeneration and the production of palatable food; these problems include the accumulation of toxic substances in discrete parts of the system and the accumulation of essential nutrients in parts of the loop aside from man.

There are many unanswered problems in metabolism and nutrition. Any valid prediction as to the feasibility of long-duration missions depends in a large measure upon data that frequently can be obtained only by inflight experiments on man himself. The selection of astronauts with respect to certain factors involving metabolism and nutrition poses problems that apparently have not been appreciated.

APPENDIX 8.

SOME CHARACTERISTICS OF MAN PERTINENT TO SPACECRAFT DESIGN AND OPERATIONS

Julien M. Christensen and Conrad L. Kraft

1. INTRODUCTION

Why Man Into Space?

There are three principal reasons that suggest the desirability of including man in certain types of space studies. The first of these is to utilize man as a functional subsystem. Even in an environment so exotic as space, man, for the foreseeable future, will be the element that assures successful performance of the complex, provides an interpretation of the unusual, serves as an unmatched resource in emergencies, and embodies the characteristics that permit appreciation of the various qualities of such experiences.

Several writers (Bauer Schmidt et al., 1962; Voas, 1961) have suggested that it may be desirable to provide certain subsystems that, strictly from the standpoint of performance, are somewhat less than optimal if they are thus more reliable or easier to maintain. Westbrook (1959) employing traditional reliability computational techniques and data available from missile-control systems estimated that, based on the then current technology, the reliability of a completely automatic attitude-control system (three-axis) for a round trip between Earth and Moon would be 0.22. Simply by adding an operator to provide attitude sensing and corrective inputs to the remainder of the system, the reliability was 0.70. With a very modest amount of spare parts (one space-rate gyro, shaping network, and wheel system) and a competent crew member, the reliability reached 0.93. There is a real, practical requirement to determine the simplest system (taking advantage of man's capabilities) that can meet mission requirements; complexity must constantly be questioned.

The second function that man will perform in space systems is that of scientific observer. It is true that man has devised instruments that are sensitive to all sorts of energy. Some far surpass man in selected areas such as the detection of ultraviolet radiation. However, man still has unparalleled ability to deal with the unexpected, to revise or devise hypotheses, to determine what instruments should be used under various conditions, to calibrate and repair, and, most important, to interpret, collate, and integrate the information obtained from his personal and electromechanical sensors.

Finally, we feel that there is a third advantage in providing for man to go into space. Man will serve as a scientific subject. The use of space to study man rather than the use of man to study space, as Major Simons of the Aerospace Medical Research Laboratories has so aptly described it. The study of man is obviously of the utmost importance to mankind—important not only for the advancement of civilization but also perhaps critical for its very survival. We suggest that whenever man attempts to conquer a new frontier—whether it be geographic, scientific, or spiritual—he, in the process, learns more about himself and his fellowman.

In this paper we shall try to demonstrate not only that man has qualities that will make him exceedingly useful in some space vehicles, but also that the consideration of space studies and the problems attendant thereto are stimulating a new look at old behavioral data and theories, and generating experimental work that will increase man's effectiveness and happiness in space and on Earth.

Timely Consideration of Man

As with other disciplines, contributions relating to man's performance are most effective if introduced during the planning and development stages in a timely, coordinated manner. The recommendations concerning human factors should be considered during the conceptual stage and continuously thereafter during design, manufacture, and test. Under no circumstances should man be viewed as a subsystem that does what is left over after the design engineers have reached the end of their current technological rope. Nor should man be inserted as an afterthought and handed a few superficial duties simply to keep him busy. Man, like any other subsystem, has certain capabilities that, if intelligently allowed for, usually will do much to enhance overall system effectiveness. One of the writers has emphasized elsewhere the importance of considering man's capabilities simultaneously and coordinately with the other resources available to design engineers (Christensen, 1957, 1958, 1962a, 1962b). Any other approach will, at best, result in inefficiencies in design or operation and, at worst, in failure or catastrophe.

Techniques are available for analyzing and defining mission profiles and for estimating generally, but with increasing specificity as development proceeds, the functions and tasks to be performed by men and machines. Such analyses should be done by a team of design engineers and human factors specialists with the help of the scientists involved in particular missions. Such analyses yield specific, definitive information that dramatically reduces the number of ambiguous generalizations that characterize many discussions on space missions. When specific tasks are delineated and defined, it usually becomes obvious whether man is needed in the system. Once it has been decided that he is needed, it is advisable to reanalyze the entire mission with this fact in mind as it then becomes possible to use his talents in other ways, thus reducing complexity and increasing reliability.

The Nature of Human Engineering Design Data

It is not our intent even to summarize the enormous amount of human engineering material that has been developed for use by engineers engaged in the design of high-performance aircraft and other complex systems. Such a report would occupy several volumes, and most of the information is adequately summarized in various texts and handbooks (see, for example, Fogel, 1963; Kennedy et al., 1952; McCormick, 1964; Morgan et al., 1963; U.S. Air Force; Webb, 1964). However, it might be instructive to look at an example or two, since continuity very clearly exists between the human-performance requirements of complex Earth systems and space systems. We quote with minor changes from a recent report by one of the present writers (Christensen, 1965).

> "Continuity in the space tasks thus far assigned to astronauts is self-evident, our astronauts having performed tracking, monitoring, and observational tasks that, psychologically, are very similar to those that these same men previously had performed in high performance aircraft. This was so evident to those in charge of selecting the initial groups of astronauts that experience as a test pilot was a prime requirement for qualification as an astronaut. . . . The controls, displays, workplaces, etc., found in the Mercury and Gemini series of spacecraft are logical extensions of those found in high performance aircraft."

Unfortunately, it is possible also to show that the same types of errors made in the engineering of aircraft also are being made in the engineering of space systems. We have selected two pairs of examples to illustrate this point.

The first incident involved a fighter. We quote from part of the report: "Technical Order 1F-...had been complied with the night before, and, during the course of the TOC, the valve had been wired in reverse" (our underline) (Alluisi et al., 1963). Note the remarkable similarity between this incident and one involving a missile. Again we quote: "One...technician lost a $2,000,000...missile by carelessly crossing the wrong wires...." (Pearson et al., 1958).

In another pair of incidents—the first involved an aircraft, and the second a missile. "Maintenance personnel had crossed the cannon plugs of the prop circuit at the commutator position" (our underline) (Alluisi et al., 1963). And the second, a missile incident, reads as follows, "Failure analysis disclosed that a technician had carelessly mismated two electrical connectors...." (our underline) (Pearson et al., 1958).

The unfortunate element in the above reports is that all these incidents were considered to be simply acts of carelessness on the part of the technicians involved. For example, nowhere was it even suggested that the designer who selected duplicate cannon plugs for dissimilar functions should share the blame.

It might be instructive to examine a few studies whose results, when applied, have tended to reduce errors such as those just described. First, consider some of the work done by Jenkins on the coding of controls (Jenkins, 1946a, 1946b, 1946c). From this and follow-on work by Jones, Hunt, and Craig (Hunt et al., 1954; Jones, 1947), a standard set of coded controls for use in aircraft was established. This intial set of coded controls was intended for use with bare hands. Bradley, recognizing that gloves often must be worn, evaluated gloved manipulation of controls and found that interactions exist between glove type and control type (Bradley, 1956, 1957, 1961). For example, changes in performance were greatest for push buttons and least for toggle switches and knobs.

More recent developments along this line have included techniques for evaluating the effects of wearing pressure suits on ability to reach and manipulate controls (Sharp and Bowen, 1960). Thus we can detect a rather reasonable (intended or not!) progression from the earliest work on coding of aircraft controls to their employment in space vehicles.

A somewhat similar progression can be traced in the area of control dynamics. In 1949 Warrick conceived and carried out a simple but very important experiment (Warrick, 1949). Employing a simple compensatory tracking task, he showed that 40 millisec (and perhaps even less) of transmission-type control lag has an adverse effect on tracking accuracy (see Figure 1). This work was followed by the well-known work of Birmingham and Taylor on unburdening and quickening (Birmingham and Taylor, 1954). Quickening provided the operator with immediate knowledge of the results of his control actions. The next major step was taken by Kelley with his work on predictor instruments (Kelley, 1960a, 1960b, 1961). A predictor simulates and displays in fast time the predicted trajectory of a vehicle.

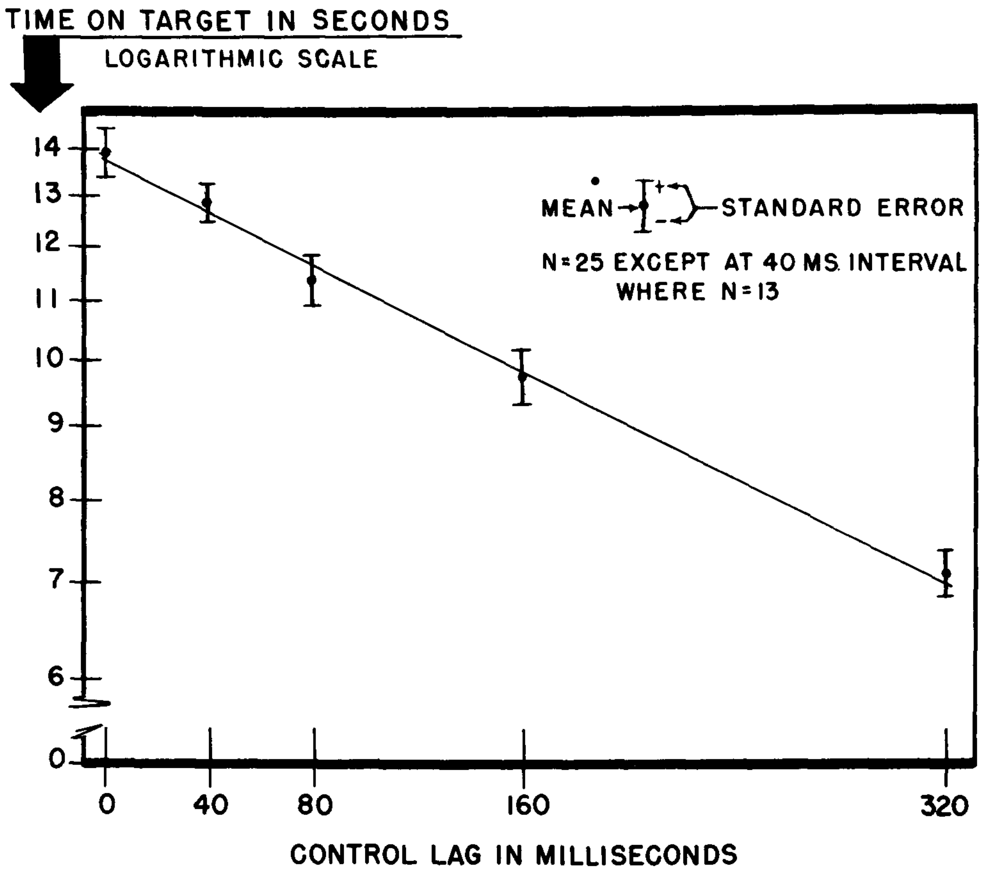

Figure 1. Effect of control lags on tracking accuracy (Warrick, 1949).

In October 1963, Frost of the Aerospace Medical Research Laboratores suggested the idea of a trial predictor display that would allow the operator to assess the merits of alternative control actions without committing the vehicle to any one of them. This idea, which in retrospect we view as a culmination of the initial work on lag by Warrick, should have important applications in simplifying the task of the controller of space vehicles when he is confronted by problems requiring complex changes in orbit.

Again we can see that this latest development in control dynamics rests on a foundation of approximately 20 years of research by many investigators. As scientists, we would be somewhat surprised to find it otherwise.

Figure 2 shows the results of a study conducted on the altimeter. The instrument on the left represents the standard altimeter. Subject pilots (97) had an average of 1,500 hr of experience with this instrument. Yet, when confronted with an improved design (center instrument), these same pilots made only one fourth as many errors and read the experimental

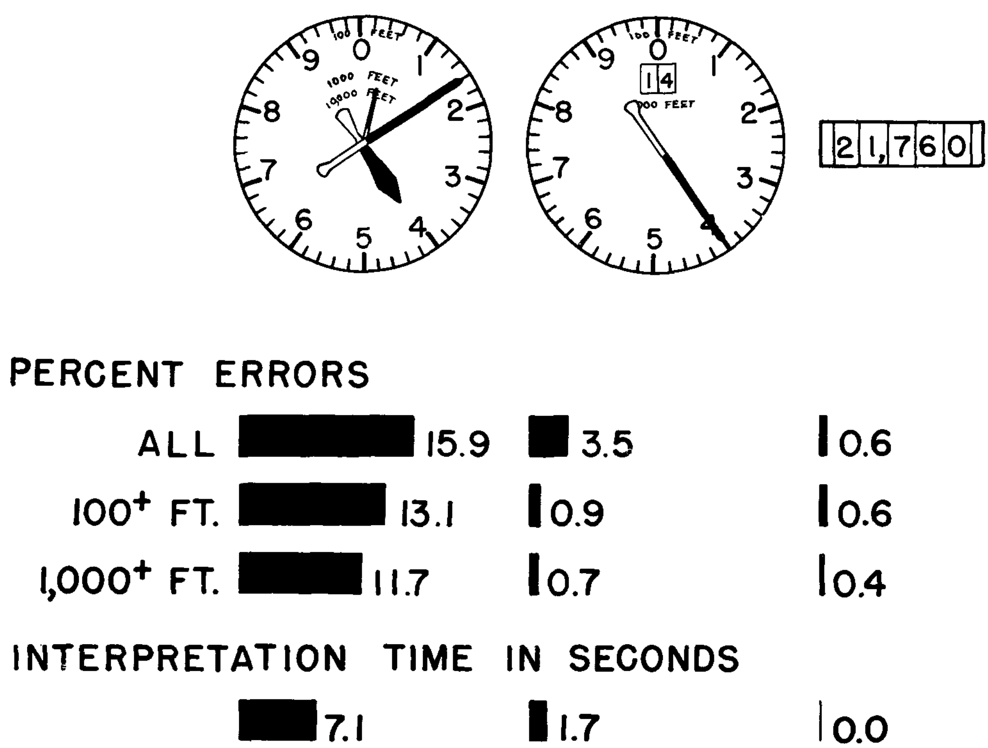

Figure 2. Speed and accuracy of reading altitude from three types of indicators. Instrument on left is standard altimeter. Instrument in center is experimental instrument. Instrument on right is a straight veeder counter, which is satisfactory for quantitative readings but not for check reading (Grether, 1947).

instrument in one fourth of the time taken to read the standard. It is important to note that this impressive improvement was made without any additional training; it depended solely on making changes in the design so as to make the instrument more compatible with the pilots' capabilities and limitations.

What lesson can be learned from these and similar examples? Simply this: that by taking cognizance of man's capabilities during all stages of planning and by providing well-designed equipment and workplaces, man's performance can be markedly improved, thus significantly increasing the chances of mission success. Only a few years ago it was considered unlikely that man could ever control vehicles at supersonic speeds. Now we witness man controlling vehicles traveling at velocities of several thousand fps—and this is only the beginning. There is virtually no limit to man's capabilities if he is provided proper tools.

Operational Example of a Need for adaptability

Two hypothetical models of the lunar surface were popular when the Ranger program was planned. One was in line with a theory of volcanic origin of the lunar surface—highlands of rough volcanic rock and seas formed as craters filled with molten lava that was hardened and overlayed with dust. These surfaces were pock-marked with secondary craters, products of subsequent bombardment by meteors and meteorites. The second lunar model followed a theory that the moon's surface was of a relatively soft material—sand or cinder-like material of varying depth, with craters formed in it by meteoritic bombardment. The latter would require a specific vehicle design to make a landing feasible. It was the mission of the Ranger system to secure information on the nature of the lunar surface and initial data on whether a feasible landing site exists for the Apollo Lunar Excursion Module (LEM).

Rangers 7, 8, and 9 were certainly spectacular successes. They contributed a total of 17,259 photographs, 8,000 of which contained more detail than had ever been seen with Earth-based telescopes. Ranger 7 took 4,316 photographs and provided resolutions of one-half mile to 3 ft, an increase of 1,300 to one over that which had been obtained previously. The Ranger probe supplied new information about the size and distribution of large numbers of secondary craters, but, despite the high-quality pictures, it did not supply the necessary information to discard either of the previously mentioned hypotheses about the structure of the surface. The photographs suggest to some experts the possibility of a very porous surface layer, while others, interpreting the same photographs, believe that the features are indicative of a hard surface.

Ranger 8 provided another series of very excellent pictures but did not supply information that would settle the controversy.

In March 1965, Ranger 9 supplied another 5,814 photographs. This Ranger mission was slightly different, in that its goal was to provide more basic information of the formation processes of the lunar surface. Ranger 9 was aimed at the crater Alphonsus and did provide the scientific world with the first conclusive evidence of lunar volcanic activity. This mission surprised the same community by depicting a relative smoothness of the walls and central ridges of the crater and a higher frequency of small craters on the crater floor. This latter information and the additional information on the rills raised the question of whether the seas were necessarily the best places to land or whether there might be some selected areas within the crater walls or on the highlands that are relatively smooth and of much harder material.

Thus, after three successful shots, we find neither resolution between the two lunar-surface theories nor an unambiguous answer as to whether there are feasible landing sites for the Apollo LEM. We are left with a highly successful technical program which few would deny has been outstanding achievement but which leaves us without an answer to the original problems.

Did 17,250 photographs of three separate areas constitute an insufficient sample, or did the problem hinge on the fact that Ranger was a nonadaptive system? If an adaptive system was needed, how much adaptability was required? For example, could Ranger 8 have carried a small explosive warhead that would have been launched from the Ranger toward the lunar surface in such a manner that it preceded Ranger and exploded on the surface, thus providing information on the relative hardness and distributions of fine particles on the surface? On the other hand, perhaps a much more adaptable system was needed, a system including a skilled scientist with a series of alternatives that he could have exercised, depending upon the conditions he encountered.

2. HUMAN PERFORMANCE EFFICIENCY

Sensory Capabilities

Absolute and differential thresholds. While the theoretical limits of the human senses have been fairly well established, there are so many factors that affect perception in a given instance that it is difficult to state human capabilities without defining the situation in which the individual is involved. There are, however, some concepts that will help us understand the general nature of all man's senses.

The first of these concepts is "threshold" or "limen," which refers to a boundary that separates stimuli that elicit no response from those that elicit some response (absolute threshold) or a boundary that separates stimuli that elicit different responses (differential thresholds). Both of these are statistical values dependent upon variations in individual senses over time, variations among individuals, and dependent to no small extent upon the method used in determining the thresholds. For example, the range of audible pitches for the young adult is usually considered roughly to be between 20 and 20,000 cps. (Men and women begin to lose their sensitivity to the higher frequencies around 20 years of age, with women apparently losing this sensitivity only about half as rapidly as men.) Without the benefit of a reference tone, most individuals can discriminate with a high degree of confidence only about eight separate tones in this range, whereas if the individual is permitted to listen to a reference tone simultaneously with the variable tone, he can (by listening for beats) distinguish tones that differ by less than 1 cps. There is an important lesson here—individuals can make much finer discriminations if they can compare the variable stimulus directly and simultaneously with a standard than if they have to compare the variable stimulus with their recollection of a standard.

It is apparent that man's higher-level (intellectual) capabilities are directly dependent on the sensitivity and range of sensitivities of his several senses. Man has been able partially to overcome this limitation, however, by designing electromechanical devices that are sensitive to energies that he cannot sense directly. Radar, radio, and measurement of ultraviolet radiation are obvious examples. By appropriate transformation, these are brought within the range of man's senses so that he can deal with them. Much more thorough treatments of thresholds and psychophysical methods used in their determination are given in standard experimental psychology texts, such as Woodworth and Schlosberg (1956).

A few threshold values for some of the senses may be of interest. (More detailed information is given in Haythorn, 1963; Klier, 1962; Myers, 1964; Pearson and Anderson, 1958; Sasaki, 1964.)

Visual thresholds: acuity and movement. There is no lower-limit threshold for visual acuity, since at the very lower limits visual detection is dependent on object intensity. (For a detailed discussion, including discrimination of movement and color, see Webb, 1964.)

Auditory thresholds: acuity and pitch. The limits of pitch for the ear of a young, healthy adult have already been mentioned. With respect to auditory acuity, it has been stated that the human ear is so sensitive that if auditory acuity is distributed throughout the human race in accordance with the Gaussian distribution theory, then there are people who should be able to detect the striking of a tympanic membrane by the molecules in the air. (We probably will never know whether this is true since, if there are such persons in the world, they long since have adapted to this random stimulation and no longer notice it. This, incidentally, points up another of man's significant features—adaptability—about which more will be said later.)

Other sense thresholds. The nose is a sense organ that generally is considered somewhat less sensitive than the eye and ear, and yet for certain substances it can detect dilutions of one part in 460 billion. The utility of this sense in operational situations has not been sufficiently exploited. For example, if you wish a demonstration of the effectiveness of olfactory stimuli, slip a small sponge containing a few drops of gasoline or hydraulic fluid into the cockpit of an airplane sometime during flight, and observe the results.

The sensitivities of the other senses—taste, touch, pain, pressure, kniesthesis, warm, cold, distension, etc.—are known to greater or lesser degrees, and information on them may be found in Stevens, (1951), and Woodworth and Schlosberg, (1958). Two of these, the vestibular apparatus, which contains the semicircular canals and the otoliths, and kinesthesis, assume unusual importance during conditions of weightlessness.

Sensory and Social Deprivation

Perception may be thought of as the product of temporal input and past experience. Experimental evidence suggests that if temporal input is reduced essentially to zero, one's percepts eventually may become quite distorted and bizarre. It is important from this point of view, and others, that persons participating directly in space studies be provided with a variety and abundance of stimuli.

Laymen and scientists alike have been especially interested in the nature of personality changes resulting from isolation since its imposition on Korean War prisoners. The early work in this field indicated that extensive and bizarre responses to isolation were to be expected, and such incidents were prophesied for extended space missions. The scientific interest in this problem has resulted in several well-controlled experimental investigations. Myers, et al. (1961) summarized a series of studies on the effects on man of a comfortable environment with limited sensory and social stimulation. After comparing the behavior of subjects housed in dark, soundproof cubicles with that of controls, the authors concluded that, although isolation is a formidable experience, hallucinations occur much less frequently and are much less complex than first believed. Furthermore, it is now known that various unusual experiences similar to those described in the early literature on deprivation are quite common, even under relatively normal conditions. Myers et al. (1961) and Zubek (1964) point out the extreme importance of establishing a base line for all qualitative performance measurements in this area. Without such data, results of investigations on the influence of deprivation are exceedingly difficult to interpret.

An individual deprived for 4 or more days of light, visual form, tactual stimulation, ambulation, sound, dreams, and sleep—separately or in combination—will show significant changes in behavior. For example, Myers, et al. (1961) found an enhancement of auditory vigilance; Zubek, et al. (1963) found a progressive decrease in acuity for frequencies in the alpha range when the subject was exposed for 14 days to unpatterned light and white noise. (The electroencephalographic records were still abnormal 1 week later, and long-lasting motivational losses were observed.) Zubek (Zubek and Wilgosh, 1963; Zubek, 1964), in studies with 7 days of perceptual isolation, found significant deficits in the solution of arithmetic problems, numerical reasoning, verbal fluency, and space visualization.

Haythorn (1963) deprived teams of two individuals of normal social interaction for 10 days. He examined their needs for achievement, application, dominance, and dogmatism. With one exception, all pairs of the confined subjects developed a high degree of hostility toward each other, while the control groups developed none. The authors suggest that this is because the control pairs interacted on a relatively superficial level, relying on access to other persons for gratification of their social needs. The experimental pairs did not have such access, and apparently the obtaining of such satisfaction within a small group is fraught with frustrations that may lead to aggression.

A better understanding of this process would enable us to select compatible individuals and to teach them how to live together under conditions of isolation and confinement. This will be especially important for scientific space missions, as the goals of such missions require the selection of individuals in terms of two distinct and not necessarily compatible criteria—scientific skill and astronautic skill.

The isolation studies reviewed above represent one end of a continuum. In space the combined influence of a number of less-intensive isolations may create the main problem. The engineering psychologist must establish design requirements for spacecraft interiors that will promote effective operations, not only through proper design of such features as dials, controls and workplace layout but also through meeting the requirements for leisure activities, social interactions, and the like.

Motor Capabilities

The sensory sensitivities are matched on the motor side. Consider, for example, the skill of a concert pianist, a surgeon performing a stapedectomy, or a pilot landing a high-speed aircraft.

One of the motor skills man has been called upon to exercise most frequently in space systems is tracking. Various attempts have been made to write describing functions for man acting as a controller. A wide range of compensatory tracking situations has been studied with resultant changes in the describing function. These studies have important methodological implications as well as implications for immediate application to specific tracking situations, because they represent attempts to describe man in mathematical terms that are consistent and compatible with those used to describe many of the other elements in complex man-machine systems. Senders and Frost provide an excellent summary of many of these studies (see Webb, 1964, pp. 350-51).

Central Capabilities*

We have examined, admittedly in rather cursory fashion, some of man's sensory-motor capabilities. However, these qualities, exquisite as they are in some cases, are not the characteristics that make man invaluable in modern systems. Electromechanical sensors and control devices exist that rival and even exceed man's abilities in the ranges of energy that he can accommodate and, as suggested previously, have the added advantage of extending to energies beyond the range of man's senses.

Man's value is to be measured rather in terms of what he can do with the impulses that his higher centers receive from his senses. His remarkable system can select, weigh, process, determine, and initiate, using,

*One of the writers is indebted to Dr. Dwight Erlick, Aerospace Medical Research Laboratories, for factual information and enlightening discussions in this interesting and challenging area.

in addition to the raw materials furnished immediately by the senses, those already resident (memory). Further, man has the ability to execute decisions by means of his motor system (as in a tracking task) or by initiating, through equipment, actions that require greater power and/or finesse than his own.

Let us briefly examine some of the more central capabilities. (We are using here an arbitrary distinction between sensory, motor, and central. What is probably involved is one continuous, highly integrated system, not only replete at virtually all levels with feedback loops, but also capable of information-processing and decision-making at levels more peripheral than had originally been thought. However, the sensory-motor-central paradigm seems to serve a useful purpose in our attempt to organize some of the relevant human-performance data.)

Flexibility. Man, if properly integrated into a system, can add a significant measure of flexibility to the system. He can choose at will from any number of programs that have been devised, and can select the one most appropriate for even highly complex situations.

Adaptability. A special feature of man's flexibility is his capacity to originate and change programs as needed to handle new and unforeseen circumstances. War records and accounts of explorations cite many instances where carefully trained men, when confronted with emergencies, have been able to devise and execute procedures to handle situations with which no one had previously been confronted. As of today, this represents one of man's great advantages over computers and automatic systems—his ready reprogram-ability and his ability to develop programs quickly for completely unexpected or very-low-probability events.

Judgment and decision-making. These are terms that we use every day, but precisely what do they imply? First, they are generally dynamic rather than static processes. They begin when an individual, confronted with a problem, selects certain variables, assigns certain weights to each possibility, and, by manipulations that are at best only poorly understood but certainly rely heavily on his memory, determines a course of action—makes a decision. To the extent that the individual selects relevant variables, weighs them judiciously, carefully considers their simple and complex interactions, and relates the current situation to the past, he arrives at a sound decision. We say that such an individual displays good judgment. Well-trained, experienced men make important decisions, involving many variables, very well, and often do it in less time than it has taken to describe the process briefly in this discussion. In exercising his judgment, we recognize that man makes use of processes including integration, differentiation, interpolation, extrapolation, selection of variables, assignment of weights, memory, experience, and interaction. Computers are helping man to make better decisions because they can handle certain elements of the process more quickly; for example, computers can integrate much more information in a given period of time. But the final decision in complex situations is generally better left to man.

With the advent of the high-speed computer we occasionally are confronted with a different sort of problem—too much information. A man can judiciously handle only so many data in a given period of time. The rest is neglected or, worse, actually interferes with his decision-making, becoming essentially noise and not information.

The above discussion on higher-level abilities contains no references to scientific literature, since it is difficult to apply the results of such research to a specific, practical problem like space exploration. This is often true in a field so new and difficult as the analysis of intellectual activities. The difficulties in this important area include at least the following:

(1) Decision-making usually takes place in a dynamic, reactive environment. It is very difficult to model such activity and to do well-controlled experiments in it (Edwards, 1961).

(2) Where decisions have personal significance for the decision-maker, it is very difficult to measure the nature and degree of the significance, and yet it may have a profound effect on his decision.

(3) The amount of experience the individual has had has a material effect on his decision-making process, and thus on the resulting decision.

(4) The more complex the decision (in terms of assessment of informational content and number of alternatives), the more difficult it is to study, because the process then depends increasingly on what is already resident in the individual's central nervous system, and decreasingly on immediate stimuli. Even if this statement is open to argument, we feel safe in saying, at least, that, no matter how numerous and complex the immediate stimuli in such instances the ideas they evoke—i. e., from memory—will be more so.

(5) The personality of an individual can have a profound effect on his decisions. For example, is he basically conservative or liberal? Such variables are difficult to scale and to control experimentally. However, although many of the experiments in this area to date are rather esoteric in nature, there are test results that might help us predict what a man is likely to do in a specific instance (Erlick, 1961, 1964). Suppose, for example, that a scientist-astronaut has landed on the Moon with the primary objective of assaying the lunar surface to a depth of 1 meter. There are a number of things that might prevent his returning with an accurate description of lunar material to a depth of 1 meter.

First, assume that the lunar surface contains only two materials, granite and gold, in the proportion of 1:1. There is some evidence that, if in obtaining his core samples the scientist-astronaut should get an initial run of the gold, he would overestimate the proportion of gold. That is, if he took 500 samples and the gold samples appeared in clusters either during the initial sampling or the final sampling, while the other material was more or less interspersed randomly, he would be likely to overestimate the proportion of gold he actually sampled on the lunar surface (Erlick, 1961). Second, suppose that this lunar expert were one who had staked his professional reputation on his theory that the "lunar surface is 90 percent gold and 10 percent granite." It is not inconceivable that he might, quite unintentionally and unconsciously, select samples that would disclose at least a 60:40 ratio of gold to granite. Third, unless the scientist-astronaut were very sophisticated in mineralogy and related sciences and in the laws of probability and the rules of sampling, there is a fair chance that he would not obtain a representative sample. Finally, there is always the possibility that "lunar gold" is really "fool's gold" that is sufficiently similar to real gold to pass all the traditional field tests for gold, even though made by an experienced mineralogist.

It is comforting but probably deluding to believe that simple awareness of these pitfalls would enable an observer to correct them. Unintentional sampling biases have been found in psychophysical studies of human taste preferences. Scientifically trained experimenters, individually allowed to randomize the order of first and second position of the "variable" in 20 paired comparisons of 45-proof whiskey, obtained data indicating significant taste preferences between identical samples. The same experimenters, using a table of random numbers for the ordering of the variable position within pairs, obtained only indications of chance differences in 100 subsequent and blind tests of identical samples. And, of course, a significant portion of each scientist's training is devoted to methods designed to eliminate or reduce personal bias and methodological limitations. However, scientists, despite substantial opinion to the contrary, are also human beings and are subject to bias, prejudice, and subjectivism. Consider, for example, the famous case of Maskelyne's dismissal of his assistant, Kinnebrook, from the Greenwich Observatory in 1796 (Boring, 1950). Not only did this renowned astronomer show himself completely unable to grasp an obvious finding from another area of science, but even after Bessel obtained accurate scientific measures of the differences in the "personal equation," it was some time before enough attention was attracted from the members of this elite profession to cause corrective steps to be taken.

The above may appear to be an indictment of science and scientists. In a sense, it is meant to alert us once again to our limitations both as observers and recorders of phenomena. In addition, and more importantly, it is intended as a justification for the use of well-trained scientists in space missions. While well-trained test pilots may serve satisfactorily as recorders of data, it is not reasonable to expect them to interpret highly technical data or to formulate testable hypotheses in fields of specialization other than their own. Incidentally, it is equally unreasonable to expect a scientist to do so in areas that are unfamiliar to him.

Mission Duration and Work-Rest Cycles

For space missions of the immediate future we suggest that such ideas as an 8-hr day or a 40-hr week be discarded. With time and weight at a great premium, there appears to be no reason why the members of a highly motivated and well integrated crew cannot be active 14 to 16 hr a day, 7 days a week, for up to 30 days at least.

Men can also endure unusually severe departures from their usual work-rest cycles if the occasion requires it. Investigations under a contract between the Aerospace Medical Research Laboratories and the Marietta division of the Lockheed Company have uncovered some interesting information regarding work-rest cycles (Adams and Chiles, 1960, 1961; Alluisi et al., 1962). In one of these experiments, 5-man crews from the Strategic Air Command spent 15 days in a space-like capsule on a work-rest cycle of 4 hours' work and 2 hours' rest. Actually these men seldom slept over 4 or 5 hr during a 24-hr day. Several performance and physiological measures were taken. A low point in performance was reached each day at approximately 0930. In fact, the intra-daily variability is significantly greater than the quotidian variability. Alluisi and his fellow experimenters also found that, by knowing when performance tends to deteriorate, properly motivated crew members can overcome the expected performance decrement by exerting extra effort. The decremental effects are subtle, however, and it might require clear, objective evidence to convince a crew member that his performance is not up to par.

Evidence of boredom was found after the men had learned their tasks well. Boredom undoubtedly will set in on some of the longer scientific journeys; it might be partially overcome by varying tasks, switching jobs, and providing suitable leisure activities. Some of these measures would, of course, complicate the selection and training program.

It was noted that the heart rate parallels the performance measures far more closely than is usually found between physiological and psychological measures.

Chiles and Adams demonstrated that two jobs can be manned full time, 24 hr per day, by only three men. The significance of this finding for shorter missions is obvious. For longer missions, it would appear that a minimum crew of four would be required if two jobs were to be manned 24 hr per day. A schedule of "4-on" and "4-off" apparently can be maintained for extended periods.

Chiles also reported that some crew members revealed in post-mission interviews that, although marked interpersonal antagonisms existed among some of the crew, these were never evident during the experiment. The men did not permit personal feelings to interfere seriously with the accomplishment of their tasks.

These results suggest that present screening procedures may be adequate for selecting and assigning men even to extended scientific missions. This statement may place us in a position of disfavor with many psychologists and psychiatrists, because some suggest that fatal frictions may develop unless greatly improved methods for examining the personalities of crew members are developed and applied. One author (Hauty, 1958) states, "The direct effects of confinement, irritability and hostility, and boredom and fatigue could be intensified to detrimental levels. On prolonged flights, the monotony of an unchanging environment may produce striking mental abnormalities in normal human passengers, like impaired thinking, childish emotional responses, disturbed visual perception and hallucinations." This may be so. On the other hand, men under such circumstances are highly motivated and have had previous experience performing difficult tasks under hazardous conditions. In fact, there is some evidence that organisms that are too highly motivated may tend to make more errors. Without intending to detract one iota from the superb performance of our astronauts to date, it might be in order to mention that even these outstanding men occasionally have forgotten to perform certain tasks during their missions. Thus it is important that suitable cross-checks be provided for every task and operation, whether that task is performed by man, by machine, or by a combination of the two. There is no doubt that errors, machine and human, will occur on missions as complex as these. But careful attention during the design of equipment, development of procedures, and selection and training of personnel will assure that these do not develop into catastrophes.

In summary, we feel that schedules can be maintained for periods up to 30 days (and perhaps longer) that would require 14 to 16 hours of duty each day from each crew member. Evidence has been presented which suggests that, should it become necessary, rather severe departures from our traditional cycles of waking and sleeping can be made for periods of at least a few weeks.

We have omitted intentionally any reference to the use of techniques involving hypnosis, pharmacological agents, and suspended animation, since we are not aware of any findings definitive enough to allow us to make firm recommendations at this time.

Determining the Behavioral Requirements of Scientific Missions

Traditional methods of job analysis can contribute much to our understanding of what men in space will be called upon to do. Individual tasks, when related to the time lines of missions, yield information regarding how frequently and under what time restrictions various activities must be performed. Such information is very useful to mission planners, to human factors specialists, and to design engineers. Particular care must be given in planning space operations to include estimates of maintenance activities.

A matrical analysis is also very helpful in summarizing and highlighting those elements of activity that may require special attention. A taxonomy such as Warrick's is particularly useful in such an analysis (see Appendix A). Table 1 contains two absurdly simple examples showing how such an analysis might proceed. While it is considerable and rather tedious work, such an analysis is believed, nevertheless, to be well worth the effort.

Roberts (1963) has shown how closely the resources required to support a space vehicle are related to crew size. He has also examined the number of launches required to man a station with four men for one year, replacing two of them on each ferry trip. His calculations clearly show the advantages to be realized from keeping men on station for at least 30 days and, preferably, much longer, and the necessity for making maximum use of each crew member. When one considers, then, the cost of putting 1 lb of payload in space and maintaining it there, such detailed analyses appear to be justified.

In addition to analyses such as the above, it is mandatory that a complete breakdown of all functions and tasks be made and related to time. This should be done first on paper and then in a simulator in which the crew can actually perform as many of the tasks as possible, including fault-diagnosis and repair.

Table 1

PERFORMANCE REQUIREMENTS OF SELECTED OPERATIONAL TASKS

Function: Navigation (celestial observation and computation)

Human Performance Functions	Star Identification	Operation of Octant	Altitude Computation	Plot of L.O.P.
Sensory Input				
Visual				
Acuity	X			
Brightness	X			
Location	X			
Spatial Pattern	X			
Motion		X		
Color				
Auditory				
Loudness				
Central Processing				
Remembering				
Procedures		X	X	X
Timing (what to do when)		X		
Symbols	X			
Identifying				
Target form				
Target size	?			
Target context	X			
Motor Response				
Voluntary				
Static		X	X	
Strength				
Coordination		X	X	X

Function: Assembly of large structure (telescope) in space

Human Performance Functions	Assembly of Structure	Alignment	Calibration	Maintain	Replace
Visual					
Acuity		X	X		
Size - Ext. Area	X		X		
Intensity					
Brightness		X			
Contrast		X			
Form	X				
Location					

(Table 1, cont.)

Function: Assembly of large structure (telescope) in space (cont.)

Human Performance Functions	Assembly of Structure	Alignment	Calibration	Maintain	Replace
Distance	X		X		X
Depth					
Spatial Seq. and Pattern					
Temp. Seq. and Pattern					
Motion			X		
Color	X			X	X
Flicker					
Apparent Movement		X			
Whole Body Movement					
Linear	X			X	X
Rotational	X			X	X
Whole Body Acceleration					
Linear	X			X	X
Rotational	X			X	X
Temperature					
Whole Body	X	X	X	X	X
Localized					
Touch and/or Pressure					
Intensity					
Frequency					
Duration					
Direction					
Direction of travel	X			X	
Localization					X
Angularity	X				
Area					
Shape	X	X	X	X	X
Roughness	X			X	X
Vibration					
Whole body					
Localization				X	X
Smell					
Type				X	X
Intensity				X	X

Measuring behavior during simulation of an entire mission is exceedingly important for at least the following reasons: (i) It enables one to check the estimated times for completing various tasks; (ii) It provides a check on operations and maintenance procedures, usually resulting in changes that improve procedures and efficiency; (iii) It discloses inefficiencies in workplace layout; (iv) It discloses sources of possible error; (v) It provides training for the crew; (vi) It discloses the degree of use and importance of each instrument and control; (vii) It permits measurement of communication load both within the crew and between the crew and Earth; (viii) By uncovering inefficiencies, it gives designers clues as to equipment that could profitably be developed for use on the mission, and, similarly, it discloses equipment functions that might better be given to man, thus effecting a savings in weight and possibly increasing reliability; (ix) It builds crew confidence; (x) It permits experimental study of such items as different crew cross-training combinations and emergency procedures.

Body Dimensions

Availability of adequate helmets, pressure suits, and other items of personal equipment requires that accurate dimensions of the human body be available to the designers of this equipment. The efficient layout of workplaces depends on accurate statistics on such dimensions as arm reach. Excellent summaries of some of the relevant body dimensions appear in the Bioastronautics Data Book (1964) and will not be given here. One or two general points may be of interest, however, regarding body dimensions and structure. First, although stature (standing height) and weight are the two best measurements for prediction, respectively, of linear and circumferential body dimensions, the correlations among the various linear dimensions and among the circumferential dimensions are, nevertheless, not sufficiently high to make predictions with the precision necessary for all applications to be made from only these two dimensions. For example, while stature and sitting height correlate to the extent of +.76, about 40 percent of the variance in this instance is still unaccounted for. Stature and weight correlate only to the extent of +.48 (Webb, 1964).

The concept of an "average man," anthropometrically, is virtually meaningless, and is dangerous. To find an "average man" one must decide which dimensions are critical in a specific instance and then select people that meet those requirements. What this can mean has been dramatically illustrated by Daniels (1952): If one takes the ten measurements most frequently used for designing clothing and decides to include as his "average man" only those men who lie within ± 15 percent of the mean on each successive dimension, he will have less than one percent of his candidates still eligible after checking through only five of the ten dimensions. By the time he has applied the criterion through the tenth dimension, no one would be left.

Quantification of Human Reliability

Recently there have been attempts to quantify human reliability in terms similar to those used in designating equipment reliability (Rook, 1962;

Swain, 1963a, 1963b; Stave, 1965). Altman and his associates have developed estimates on the probability that a certain specified behavior will not lead to incorrect performance of an action. Rook (1962) has described a quantitative approach to reduction of human error in industrial production. Swain has extended Rook's method to military systems, calling his method THERP (Technique for Human-Error Rate Production). The method consists, essentially, of four steps: (1) listing the human operations, (2) predicting error rates, (3) determining error effects, and (4) recommending necessary changes. For example, the probability that an individual will read a two-digit figure correctly is .9998, while the probability that an individual will read a 6- or 7-digit figure correctly is .9991 (Swain, 1963).

One of the limitations on determining such probabilities to date has been the unavailability of sufficient data gathered under typical operational conditions. Error rates from experimental situations in laboratories cannot be used without correction, since in many laboratory situations special measures are taken to induce errors (by time stress, for example) so as to establish statistical significance more clearly and with fewer cases. (One might also question the representativeness of much of the reliability data obtained on hardware components in the laboratory.) As Swain points out, these estimates, even if off by a factor of 2 or 3, may still be useful since we are generally dealing with figures on the order of 10^{-3} to 10^{-5}. In many cases, the estimates are probably as accurate as some of those used to predict hardware component reliability.

3. WITHIN VEHICLE OPERATIONS

Some Similarities Between Zero-Gravity Operations and Terrestrial Experiences*

Man's performance is generally enhanced in a new situation whenever that situation permits high positive transfer of skills that he has acquired previously. This transfer reduces the requirements for extensive retraining and permits the influence of Jost's second law, which states that if two associations are of equal strength but of different age, the older diminishes less with time. Thus, there is the possibility that in a critical situation that calls for a retrained response, the operator will respond with the first procedure that he learned whether it is appropriate or not. Therefore, preparation for a new and different situation requires intensive training to make the newer association stronger than the older in order to avoid this inadvertent utilization of an older, inappropriate response.

It is important in the design of space vehicles to provide for extensive utilization of those skills of astronauts or scientist-astronauts that are already well learned. Thus, we can anticipate that the bulk of the information that has been acquired on Earth will transfer positively to the operations of space vehicles. In addition, there is some evidence that the performance of well-learned skills is less likely to deteriorate under stress.

If the up-down reference is maintained, then current principles of design for controls and displays can generally be extended to space operations. The population stereotypes and recommended procedures for designing and arranging controls and displays in terrestrial vehicles can be utilized in space vehicles. Such positional references would be meaningless if there were no standard reference planes in the vehicle. Controls requiring small actuation forces will be the same as those for terrestrial vehicles while those controls requiring large actuation forces will require the designer to provide appropriate restraints.

To illustrate that the bulk of our motor skills will transfer positively to space tasks, let us project a hypothetical operation and examine some experimental evidence. Consider a space laboratory that has two cylinders—one for sleeping and living, the other for working. The lower cylinder has a console on one side with two seats facing it. The operator's initial task is to move from the upper chamber into position in the lower chamber. Using the neutral buoyancy technique as a means of simulating zero g, Mabry et al. (in preparation) determined the ingress times with and without the assistance of handholds. Despite the subjects' limited training (3 practice trials), it took an average of 10 sec with the handholds and 9 sec without, there being no significant difference between these two conditions. They restrained themselves within their seats by fastening a halyard snap on a seat belt. This took an average of 10 sec.

In the course of 12 trails, neither subject inadvertently hit any of the console surfaces. Egress from the working area back to the living area

*For a summary of present knowledge and methods of obtaining or simulating zero gravity, see Appendix B.

was 1 sec faster than the ingress. These data suggest that for these tasks no particular problem should be found in the absence of gravity.

The same investigators studied two routine maintenance tasks in a shirt-sleeve, neutrally buoyant environment. One task involved the removal and replacement of a neutrally buoyant mock-up of an electronic module 12 x 10 x 6 in. in size. The module was attached to the mock-up by four dzus fasteners and the removal tool was a blade-type screwdriver. The mean removal time for the 6 trials was 17 sec, with a standard deviation of 1.9 sec. The mean replacement time was 39 sec, with a standard deviation of 11.4 sec. There was no reliable difference between subjects or across trials for the replacement of the module. (The relatively longer time of replacement over removal is attributed at least in part to the fact that the dzus fasteners were too short.)

A second task involved the removal and replacement of a "press-to-test" lamp. One subject performed this task, and his mean time for removal of the bulb was 24 sec, while the mean time for replacement was 11 sec. The relative difficulty of removal was due to the shortness of the lamp and its relatively recessed position in the socket. The same relatively greater difficulty in lamp removal was found in a 1 g environment. None of these exploratory studies indicated that zero g situations, as simulated by neutral buoyance, are substantially different from the 1 g situation.

Mabry, Chaffee, and Emanuel (in preparation) also investigated the possible use of Velcro as a restraint and as an aid to walking under zero g. (Velcro is a plastic material, two sections of which, if pressed together, form a zipper-like adhesion. The pieces can be separated with a stripping motion but have a greater resistance to other separating motions.) A comparison of walking times under the Velcro and free-floating conditions within the capsule was made. The mean time was 30 sec with the Velcro, with a standard deviation of 6.3 sec while the mean time under the free-floating condition was 8.0 sec, with a standard deviation of 1.2 sec. The difference between the two means is statistically significant. This experiment supports other studies that suggest that special materials are not needed for walking under zero g, since it will be more efficient to free-float (Erlick, 1961; Simons, 1959). Velcro, however, could be used to secure an operator while he is completing tasks in a stationary position.

A study performed at the Aerospace Medical Research Laboratories compared times for terrestrial gravity and weightlessness (aircraft method) for some routine maintenance operations on the RL-10 rocket engine. The maintenance men were properly tethered at the engine. No decrement of practice significance was found for the weightless condition.

There would appear to be significant advantages to be realized in the design of space-vehicle workplaces, due to the fact that the operator is weightless. For example, no chair is needed. In addition, movement from one area to another requires very little effort, and it is as easy to work near the ceiling as it is near the floor.

In summary, these and other studies have shown that under weightlessness a properly tethered man can perform the same functions and use the same tools that he employs under Earth gravity, with a decrement in time that seldom exceeds 10 percent. Free-floating around the workplace is no problem for an operator experienced in zero g operations and usually takes less time than walking.

However, the picture is not quite so optimistic if the operator must wear a full pressure suit. This is particularly true when the suit is inflated. For example, in a task consisting of reaching and manipulating controls, if we take as 100 percent the work volume which a man in his underwear can accomplish, the usable work volume is reduced by 23 percent if the operator wears a certain recently designed pressure suit. If this same suite is inflated to 3.5 psi, the usable work volume is reduced 69 percent; that is, the usable volume is less than one third of its initial value. This has some rather obvious implications for the placement of emergency controls. (Some of this reduction might be compensated for by the fact that, if weightless, the operator or scientist can turn and can move around the work area more readily. However, with these conditions, care must be taken to prevent inadvertent operation of controls and damage to displays by bumping or kicking them.)

Some Specialized Problems of Zero Gravity

Effects of prolonged exposure on physical condition. Problems may arise owing to the physiological changes or subsequent behavioral effects that may occur under extended periods of weightlessness. Sometime before this was considered a problem associated with space operations, two groups (Taylor et al., 1949; Whedon et al., 1949) investigated the influence of extended bed rest on the physiology of hospitalized individuals. Taylor et al. (1964) subjected six healthy men between the ages of 20 and 33 years to a 3- to 4-week period of bed rest. Measurements were made of the cardiovascular system during rest, during upright posture, and during work, and of speed, coordination, and strength.

The investigation disclosed a 17 percent decrease in heart volume and an 8 percent decrease in the transverse diameter of the heart. There was a highly significant increase in the resting pulse rate, averaging roughly 0.5 beats per min for each day of bed rest. Pulse rate at the end of a half-hour walk at 3.5 mph up a 10 percent grade was increased by 40 beats per min in subjects having had a period of bed rest. There was no change in mechanical efficiency during this walk. The intake of oxygen during a 90-sec run at 7 mph up a 15 percent grade was reduced by 16 percent after 3 to 4 weeks of bed rest. The bed rest produced a marked deterioration in cardiovascular response as measured by pulse rate and blood pressure changes on a tilt table. Taximeter studies showed a definite increase in sway. Coordination, as measured by pattern tracing, suffered a small loss, while speed of small hand movements, medium arm and hand movements, and gross body and arm movements showed no deterioration. Grip strength was not influenced by bed rest and back strength showed only a small decrease.

After bed rest, the rate of recovery of various functions was roughly proportional to the extent of the deterioration during bed rest. Strength, coordination, and postural sway recovered quickly (4 days); blood lactate after exhausting work and the oxygen cost of exhausting work returned to normal in an intermediate time (2 weeks); pulse rate while walking up a grade and oxygen intake during exhausting work recovered slowly (between 2 and 5 weeks), and the cardiovascular response to posture was very slow in returning to normal (more than 7 weeks). The authors concluded that bed rest results in major cardiovascular deconditioning.

The second group of investigators (Whedon et al., 1949) studied the influence of an oscillating bed on the metabolic and physiologic disturbances associated with immobilization and bed rest.

Many investigations currently under way suggest that cardiovascular deconditioning is still the major problem in space flight. Investigations as to how to prevent this type of deconditioning under zero g have again utilized bed rest.

The methods to prevent deconditioning that will eventually be used, their power requirements, their complexity, type of equipment, and time requirements are critical since the amount of scientific equipment that will be available to the scientist-astronaut will be inversely related to the weight and power requirements of the equipment required to maintain life.

Approximately 2 years ago Grave et al. (1964) investigated four different types of activity that might support the cardiovascular system. Each of four subjects served for 4 days under each of four activity regimes: complete bed rest, isotonic exercising of the legs, active horizontal trampoline, and passive trampoline. The dependent measures were pulse rate and blood pressure changes as produced by tilt-table tests. Results of the study suggest that the most effective method to prevent deconditioning is use of passive trampoline, with isometrics next, and then active trampoline. The poorest method was bed rest.

Later studies by Grave indicated that isometric exercising loses much of its effectiveness after 4 days and that the difference between active and passive trampoline depends on how soon after the activity regime the tilt-table test is administered. Immediate testing favored the passive trampoline. The results of an investigation comparing the centrifuge and the trampoline as methods of supporting the cardiovascular system, though not completely conclusive, suggest that centrifugal forces without physical exercise are not sufficient to prevent cardiovascular debilitation.

White's (1965) investigations with centrifuges of short (approximately 30 in) radii found that the deterioration produced by recumbency was largely prevented by centrifugation. White's experiments called for 20 days of bed rest followed by 20 days of bed rest with four 7.5 min rides on the centrifuge each day. The acceleration gradient was 225 percent, while the acceleration level was +1 g for one group and +4 g for the other. The latter group showed less liability in blood pressure and heart rate during tilt tests than did the subjects exposed to +1 g.

Grave, Chase, and Rowell (1965a) recently subjected three groups of four men to 15 days of bed rest. One group had no scheduled exercise, one group had bicycle exercise, and the third group had active trampoline exercise. The results appear encouraging as two of the four men in the active trampoline group maintained their cardiovascular condition. These two individuals exercised at a level equivalent to 70 percent or more (respiratory intake) of their pretest maximum exercise per day. The remaining two men in the active trampoline group, exercising at 50 to 60 percent of their maximum, did not maintain their cardiovascular performance. All four men in both the "bicycle" and "no scheduled exercise" groups evidenced cardiovascular deconditioning.

Grave et al. (1965b) also subjected two groups of four men each to 30 days of bed rest and to a medium-calcium, high-calorie, 35 percent fat, drink-base diet. The two groups differed as to their prescribed exercise duration, 15 to 45 min per day of active trampoline. The general results indicated a marked deconditioning at the end of 15 days, and, although some deconditioning still existed at the end of 30 days, it was measurably less than it had been after 15 days.

Other types of physiological debilitation that may be critical in zero g involve the otoliths and the potential loss of calcium from the bony structures. The maintenance of physical strength does not appear to be a problem, for even one isotonic contraction per day against some specified force appears to be enough to maintain a specific set of muscles.

Effects of duration of exposure on performance. Information on the influence of extended weightlessness on man's performance is not so extensive as that on the influence of extended weightlessness on man's physiology. One of the reasons for this is the lack of a true simulation of zero g for extended periods.

Because of the limited use of distance vision in some space vehicles one might expect changes in accommodation and visual acuity. In series of investigations dating back to 1961 (Young, 1961a, 1961b, 1962, 1963; Young and Farrer, 1964), Young has found that the refractive power of the primate eye is affected by a restriction of visual space. Monkeys housed in cages that limited their vision to 20 in. had a higher refractive error than primate controls that could view objects 20 ft away. He subsequently tested primates housed in cages in larger rooms; these groups had higher refractive error than primates just received in the United States.

In one of his studies, Young (1963) restrained seven young monkeys (Macaca nemestrina) in chairs under hoods that restricted their vision to a distance of 20 in. or less for 1 year. In comparison with adult control monkeys, the young monkeys did not evidence any effects for 4 or 5 months, whereas the adults began to show effects within the first month. The young animals changed more rapidly once the effect became apparent and continued to change as long as they were in the situation. In contrast, the adults changed more slowly and leveled off at the end of 5 months with no further changes. The amount of myopia developed by the young animals was one and three fourths diopters as compared to three fourths of a diopter for the adults in the same period.

These data suggest that a man in a space vehicle of restricted size doing primarily nearpoint tasks might develop a refractive error. This, together with reduced activity and the absence of gravitational stimulation, suggests the possibility of an interaction among these factors that might accelerate the development of the refractive error.

Smith and Farrell (1964), working with humans in a 3-day confinement study, were unable to draw any conclusion from the limited visual-acuity shifts that were observed. However, their nearpoint lateral phoria measures showed the same trend for all five subjects: a mean increase of

7 diopters of exophoria from a base of 7 diopters, or an increase to 14 diopters by the end of 30 days. Three days after the five individuals were released from confinement, the nearpoint lateral phoria of 7 diopters was the same as in the pretest. This development or extension of an exophoria at nearpoint is characteristically observed among students and adults who are doing considerable nearpoint work; the trend is called a stress pattern by some optometrists.

It is not proposed that an investigation be carried out in space on the development of accommodative error, but it is suggested that simple procedures involving farpoint visual tasks be practiced to prevent the development of myopia. Research is needed to establish how much activity at the farpoint should be recommended to prevent the development of accommodative error.

Performing Tasks with Remote Manipulators

The ability of the human to detect differences among lifted weights was one of the first problems considered by experimental psychologists. Under weightlessness, however, objects have only mass. Also, it is probable that many tasks will be performed remotely to avoid exposing man unnecessarily to the dangers of radiation, suit puncture, and the like. In fact, until there is a significant improvement in the mobility afforded by pressure suits, activity in a space suit outside the vehicle may be quite restricted (Peters, 1962). Orlansky (personal communication) also points out that there may well be instances in which the operator will be separated by considerable distances from his work, and that radio relay and television will be his only contact (e.g., a remote, maneuvering "bug") or the distances could be so vast that the time delay in radio transmission may be appreciable. We know that a delay in feedback generally results in a decrement in performance, but the nature and degree of the decrement under the conditions that Dr. Orlansky postulates would be hazardous to predict. Certainly more work is needed in this significant area. With respect to the more direct case, Crawford and Kama (1961) have studied the direct and remote handling of weights and masses. Similar results were obtained when masses were removed (not lifted) directly and remotely. Crawford and Kama note that the loss in sensitivity is roughly proportional to the mass of the components of the manipulator used in the experiments.

Experience suggests that operations generally take 6 to 8 times longer with current remote manipulators than by direct manipulation. A report by Peters, et al. (1962) suggests, however, that performance of at least some tasks in a pressurized suit takes just as long and certainly would be more fatiguing. Two conclusions seem inescapable: the mobility of workers in pressurized suits must be significantly increased and remote manipulators must continue to be improved. If manipulators are refined and if, particularly, equipment is designed that is compatible with the requirements of remote manipulations, this mode of operation should become an attractive one for a variety of space operations.

Walking, as a means of locomotion under zero g, is, at best, difficult and inefficient (Christensen, 1957). Various decives involving magnets, adhesive materials, and other devices have been tried with varying degrees of success. One point of special interest, although only tentatively established since it is based on only four subjects, is that, when asked to walk under weightless conditions on the ceiling of an aircraft, subjects stated that "down" shifted toward the direction of their feet as soon as their shoes became attached to the ceiling (Simons, 1959). To these subjects, everyone and everything else in the airplane was upside down. This impression was very strong to them and was independent of whether their eyes were open or shut.

But why should man walk at all under weightless conditions? It may be simpler and more efficient to soar. Experiments in the C-131B Flying Laboratory show that most men can attain velocities of approximately 9 mph simply by pushing off from the side of a weightless vehicle. The current world's record holder, Sgt. Espensen, attained a velocity of 13.3 mph. While this is not as fast as man can sprint, it is, nevertheless, approximately as fast as he can run for an extended distance and much faster than he can walk, and it requires very little energy.

Man must learn to control his movements under weightlessness in order to maintain or change his body orientation. Kulwicki, Schlei, and Vergamini have theoretically analyzed the problem of self-rotation, and this analysis has been verified by Bennett in C-131 and KC-135 aircraft. Proficiency in self-rotation and soaring techniques would enable a man to control himself within space vehicles and even to move from one vehicle to another. Do not be misled, however; self-rotation is not easy. At present it appears feasible only if the operator is unencumbered. Many of the required movements cannot be executed efficiently, if at all, when the operator is clothed in one of today's pressure suits.

Neutral buoyancy has been used to simulate weightlessness in locomotion studies. The soaring that can be accomplished in water after a man has been carefully balanced for neutral in roll and pitch is an inexact simulation in at least two ways. First, in soaring underwater, man can plane or obtain "life" by shifting body configurations. Obviously, this is impossible in space. Second, the initial force required for a given transit is greater in water than in space. However, soaring while neutrally buoyant in water does appear to be strikingly similar to soaring in a weightless aircraft. The neutral buoyancy technique has also been found useful for mobility studies, for training individuals to go through hatches, for studying the assembly of large structures, and for using legs and feet in ways that are uncommon or impossible in a 1 g environment. Nearly all the large companies in the space industry are using the neutral-buoyancy technique. The General Electric Company has a well-designed methodological program that should provide considerable information on the sorts of activities that can be validly studied with this technique.

As man moves around his workplace, he will find it inconvenient to tether himself repeatedly to the work sites. Dzendolet (1959) has determined by theoretical analysis and by experimentation the body positions that the weightless worker should assume with respect to his work. For example, to exert torque with minimum body movement, Dzendolet recommends that the untethered maintenance man position himself so that ". . . his body is at right angles to the axis of rotation of that which he is to turn . . . ."

Grether (1962) reminds us that although, with the exception of Titov, no cosmonaut or astronaut has experienced nausea, and although the results of the experiences of Leonov and of White are heartening, we still should be cautious about predicting what will happen on extended flights when crew members will be required to move around extensively in and about a spacecraft rather than remain strapped in one position.

In summary, it appears that walking is a rather inefficient method of locomotion to use inside a space vehicle. Transfer with the aid of simple handholds should prove adequate in small vehicles, and soaring appears attractive for transfer inside large vehicles and perhaps for short distances between vehicles (up to 50 ft). If properly tethered, experienced personnel should be able to perform tasks involved in the operation and maintenance of space vehicles within times that compare favorably with those obtained under terrestrial gravity. (It appears that it took Komarov about twice as long initially to orient his space ship and Yegorov about twice as long to take physiological measurements as it did on the ground, but these decrements probably were due to the novelty of the situation, and they appear almost to have largely disappeared with practice.)

Rendezvous and Docking

Rendezvous and docking may be among the most important functions that man will perform from within a space vehicle. Baker and Steedman have considered several aspects of the visual problems involved (see especially, Baker and Steedman, 1961a).

On the basis of their studies, they have made what appear to be reasonable inferences regarding man's ability to perform the functions required in terminal navigation and in rendezvous with another vehicle in space when he uses only direct vision. To quote them:

> "With no training, man can preceive with considerable precision under certain conditions whether an object is approaching or departing. As the luminance of the object is reduced below 0.1 foot-Lamberts, however, his accuracy in making these perceptual judgments decreases rapidly. As the angular size of the stimulus is reduced, man's performance also deteriorates. His ability to perceive movement in depth extends over a reasonably large range of absolute rates of movement. An untrained man, however, is poor at estimating closure rates when the rate of change of the angular subtense of the stimulus is the only perceptual cue. Large variable and mean constant errors are evident in the data obtained from the performance of such a task." (Baker and Steedman, 1961a).

However, with intensive training, these errors are reduced considerably.

Houbolt et al. (1962) report a simulator study of the rendezvous action that seems partially to confirm Baker and Steedman. The operation was started as a simulated distance of 40 ft with a thrust of 0.1 to 1.0 fps. Rendezvous required approximately 5 min, and velocity at contact was approximately 0.1 fps. Such a performance is judged satisfactory. Rendezvous was performed with visual cues only. The authors do not report how much training, if any, the subjects required to achieve this level of performance.

Clark (1965) had subjects fly a simulated short-range coplanar orbital rendezvous maneuver using only the cues provided by direct vision. He compared line-of-sight and trajectory techniques. All subjects were able to maneuver successfully to a position 10 ft directly in front of the target at a terminal velocity of less than 5 fps.

Thus, it appears that man should be able to make satisfactory rendezvous in space with little, if any, help from automatic equipment. Incidentally, this is a good example of an instance in which space-inspired studies have made a significant contribution to our knowledge in a critical area of human behavior: depth perception—the use of space to study man, as we have termed it.

Reach Studies

As designers face the problem of planning displays, controls, restraints, and other man-machine interfacing hardware, they will need information on how far the astronaut can reach. Zero g causes a significant change in the reach potential. Man in space can and will utilize the possibility of tilting his body in any direction, thus effectively extending his reach. However, the interaction of such parameters as position and bulk of equipment, type of restraint, task, and frequency of use will continue to exist.

Recent studies by Mabry et al. (in preparation) with human subjects indicate that, in general, they could reach a considerably greater area when they were restrained by their feet than when restrained by the seat. Also, the large subjects could reach somewhat larger areas then the medium-large subjects. The extent of reach was not significantly affected by the type of breathing system used, i.e., backpack or chestpack. It was observed that neither the Velcro plates nor the seat provided easily controlled body restraint. It was very difficult to remain attached to the Velcro plates without using a hand to grab structure in the mock-up. It was also extremely difficult to move the seat without using the hands in this way.

In summary, the ability to reach and manipulate controls under weightlessness is quite satisfactory if man is not in a pressurized suit. Reach potential is reduced significantly, however, if man is in a pressure suit and to a serious extent if the suit is pressurized. Reach envelope and mobility are materially increased if the chair is omitted.

Reconnaissance Interpretation from Space

Scientific photography of the planets, Moon, and Earth is one of the greatest potentials of a space orbiting platform. The ultimate criterion of success of such a system is the quantity and reliability of information extracted from the photographs. Natural and engineering conditions may restrict the extraction of information contained in such photographs. Photographs of the Earth will be attenuated by such phenomena as atmospheric turbulence, cloud cover, and snow, while photographs of the lunar surface will be attenuated by variation in luminance as a function of luminescence resulting from the bombardment of the Moon by high-energy ultraviolet, x-ray, and corpuscular radiation. The natural attenuations to be found on the planets are not known in detail by the authors. Sun angle will serve as an attenuator for all three-- planets, Moon, and Earth.

Numerous engineering conditions may restrict the extraction of information from photographs. Inadequate image-motion compensation, film-grain size, and optical defraction are just a few of the many things that will degrade interpretation by decreasing resolution, contrast, and number of gray steps and by increasing blur. The method employed to display the information for image extraction is a major consideration. It has been shown that the method of display, such as using photographs taken at different times for the purpose of detecting natural and cultural changes, can improve the possibility of detection as much as 42 times (Klingberg et al., 1963).

Rapid processing equipment within the vehicle will make real-time viewing a possibility. The speed of the orbiting platform interacts with the optical variables of focal length and angle of coverage to produce a display problem for the interpreter. Wide-angle photography yields coverage of large areas, a large display scale ratio, and a longer time to view. Conversely, narrow-angle photography provides a smaller area coverage, lower display scale ratio, and a shorter time to view. The interpreter has to choose between the alternatives of viewing large images for a short period of time and small images for a longer period of time.

The relative merit of these alternatives was studied with a dynamic display and the task of identification of complex polyhedra. Display scale ratios between 1:85 and 1:94,000 and viewing times between 0.75 and 90 sec were investigated. Within the range of 1:8,000 and 1:30,000, similar identification performance was measured.

Elworth (1964) extended these studies of dynamic real-time displays, using aerial photographs with missile sites as targets. Holding scale constant at 1:5,800, he found detection of Minuteman sites to be related inversely to the logarithm of the angular velocity of the moving image. These results apply to a 10-in. square display and viewing times between 28 and 3.5 sec. Gilmour and Iuliano, employing motion pictures of real terrain, have been measuring the probability of detection and target-acquisition range as a function of altitude and speed for very acute (85° from nadir) viewing angles (Gilmour, 1964; Gilmour and Iuliano, 1964).

Klingberg et al. (1964) studied camera angles ranging from directly above (nadir view) to 80° off nadir for five different target orientations. Their study results suggest that angle of view, target orientation, display scale (displayed image size to actual size expressed as a ratio), and degree of blur are variables that have significant effects on performance—both singly and in interaction.

Simulations of photographs from orbital altitudes, with realistic and complex backgrounds have now been added to these investigations. A model of rural terrain has made possible the systematic investigation of the common (to Earth, Moon, and Mars) and important variable of Sun angle.

Sun angle is important because its effect can be to enhance or to reduce target detection or identification. Lengthened shadows can obscure small objects or change the outlines of large objects and thereby hinder the interpreter. On the other hand targets painted to camouflage their shape may be easily detected by the contrasting shadow and sometimes identified by the shape of their shadow. Target detection is increased by shadows to the extent that many investigators include the measurement of shadows in representing target areas in their predictive mathematical models (Taylor, 1964).

The influence of Sun angle was quantified in a series of three experiments all employing the model of rural terrain constructed to a scale of 1:87, the actual size of the model being 8 ft^2. In addition to a model of a Minuteman missile site, the simulated terrain included a model railroad and model powerlines, buildings, and vehicles. Six objects were designated as targets—two tanks, two trucks, and two half-tracks.

The first study investigated four Sun angles—0°, 20°, 40°, and 74°—two photographic scale ratios—1:1098 and 1:2196—and camouflaged and non-camouflaged targets. The observers' tasks were to detect and to identify the latter by matching the target with one of nine photos of similar vehicles. Figure 3 shows detection performance as a function of Sun angle for both photographic scales. Sun angle appears to have a small effect on detection up to 40° but becomes significant at about 75°. The identification performance data plotted in Figure 4 show a more nearly linear function due to changes in Sun angle with a clearer separation of the curves for the two scales than was the case for the detection data (Elworth et al., in preparation).

The second investigation used the same four Sun angles but three levels of image quality—1.5, 4, and 6 ft of ground resolution. Sun angle and image quality interacted significantly to change both detection and identification performance. Detection exceeded 80 percent at 0° Sun angle and 1.5 ft ground resolution and decreased to 3.3 percent for 74° of Sun angle and 6 ft ground resolution. Peak identification performance was with 20° of Sun angle, when results were collapsed for different image qualities (Elworth et al., in preparation).

The third study in the series (Elworth et al., 1965) was designed to investigate the relative effectiveness of single and multiple viewing angles for the detection and identification of targets at two different Sun angles (20° and 80° from zenith). Scales were used which represented those obtainable by use of large orbiting telescopes. Thirty-six different scale model targets were mounted in different positions on a terrain model. Position selection was done by means of a 10 x 10 numbered matrix and a table of random numbers. Six different sets of six models were used. Sixty photos were obtained (six model sets x five viewing angles x two sun angles) which had a ground resolution of approximately 1.5 ft. Sunlight in this experiment was simulated by a projection arc lamp placed 120 ft from the target, which provided illumination with a beam that diverged approximately

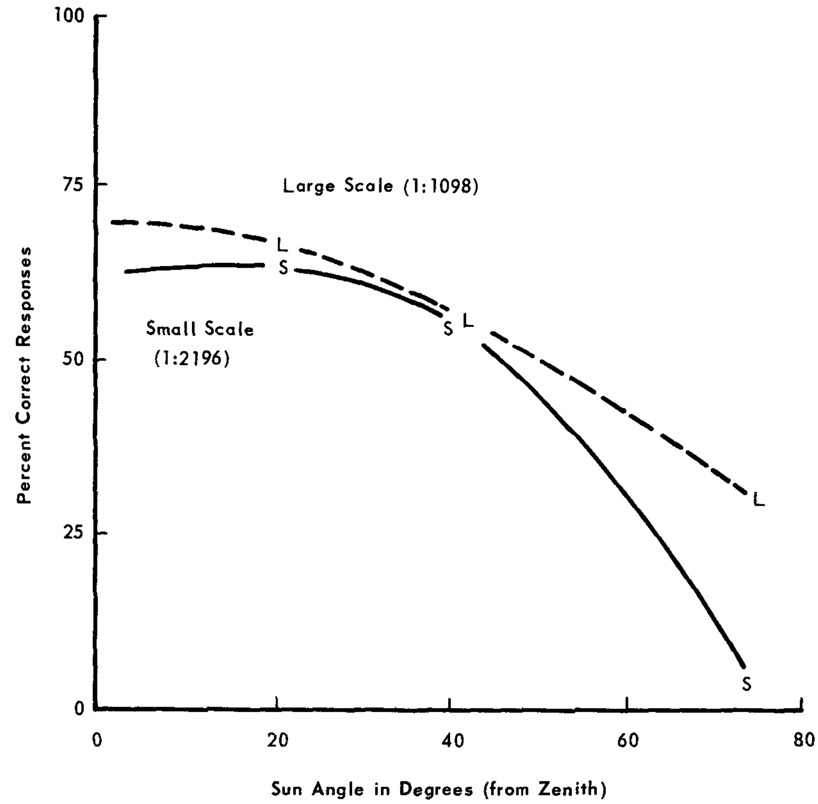

Figure 3. Detection of vehicles in nadir photographs of high ground resolution. (Bioastronautics, The Boeing Company)

2° at the target model. The prints used as the stimulus material gave a photographic scale ratio of 1:1500 (print distance to actual ground distance).

Figure 5 shows detection and identification as a function of viewing angle for the two Sun angles when only a single view was presented. Observers found 82 percent of the targets with the 20° Sun angle and 54 percent with the 80° Sun angle. Identification responses were 31 percent correct with the simulated Sun near zenith (20°) and 20 percent correct with it was "low on the horizon." There were no consistent differences in performance for either Sun angle due to changes in viewing angle. Out of every eight targets detected, three were identified, regardless of Sun angle.

Figure 6 is a bar graph giving the detection data for the subjects who saw the target area from multiple view angles. The stimulus material was the same as that used for the single-view presentations. Except for the simultaneous presentation of multiple views of the same scene, the procedure was the same as for the presentation of single views. The detection performance with multiple views for the 20° Sun angle varies little with the number of views and is closely equivalent to that obtained with single views. Detections with the 80° Sun angle are lower with the multiple views (45 percent) than with the single views (54 percent).

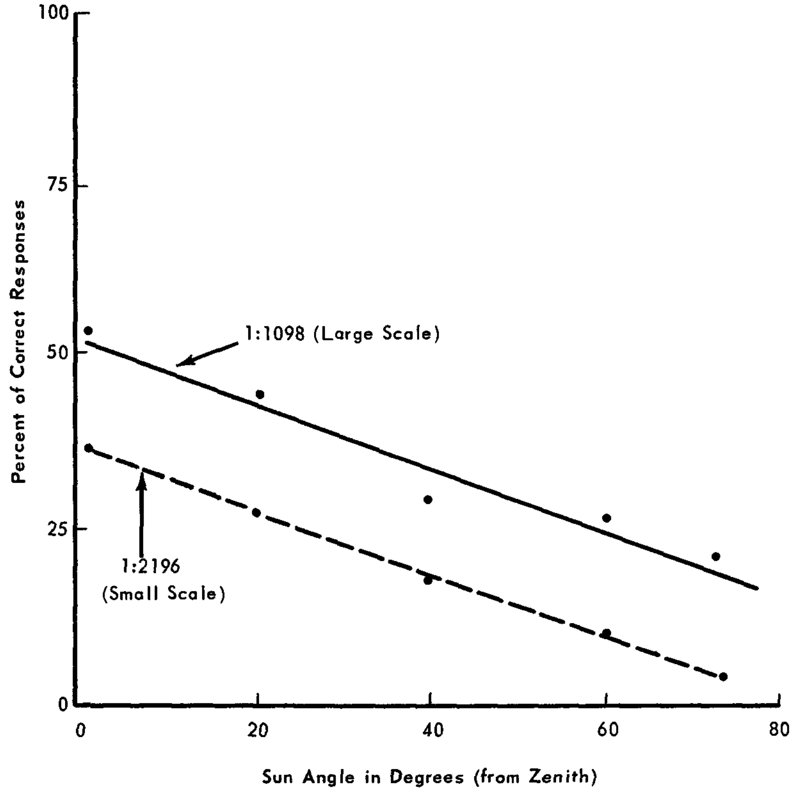

Figure 4. Identification of vehicles in nadir photographs of high ground resolution. (Bioastronautics, The Boeing Company)

Identification performance appears to be aided by the use of more than one viewing angle for both the 20° and the 80° Sun angles, running better than four identifications for every eight detections.

In summary, the influence of natural and engineering variables such as Sun angle, angle of view, and display scale are sufficiently important that in combination they can cause the information extracted from the high-quality photographs to vary by a ratio of more than 2 to 1. It therefore is important to scientific endeavors in this field that we make optimal use of those variables which can be controlled by engineering design, technique, and utilization of man.

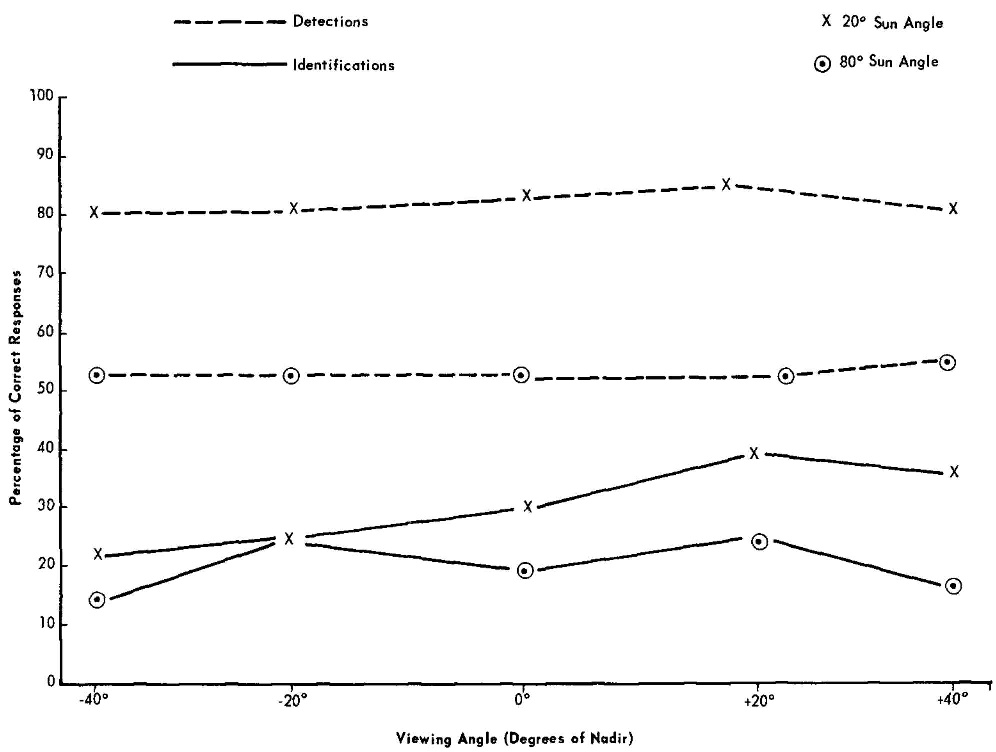

Figure 5. Single view detection and identification performance with high ground resolution photographs. (Bioastronautics and I.R. and Optics, The Boeing Company)

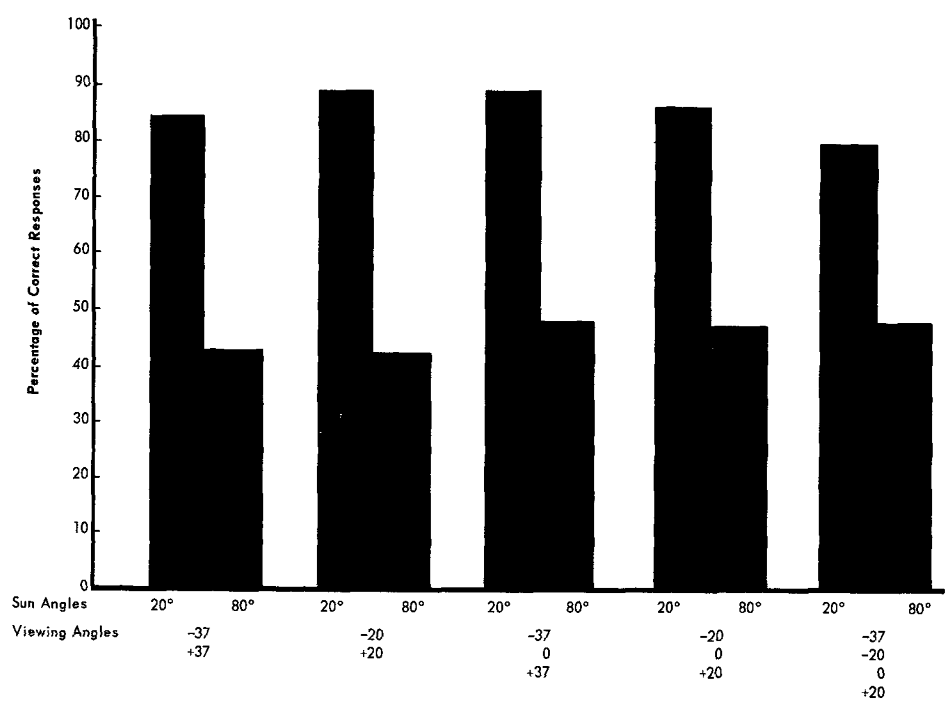

Figure 6. Multiple-view detection performance with two Sun angles and five viewing angles. (Bioastronautics and I.R. and Optics, The Boeing Company)

4. EXTRAVEHICULAR SPACE OPERATIONS

Major Tasks

Assembly of large structures. Assembly of large structures may be one of man's major contributions to scientific space missions. Optical telescopes and antennas appear to be high-priority items. The optical telescope operating without atmospheric limitations may prove to be such a valuable tool that the installation of many such telescopes and the redesigns necessary to approach the defraction limits may become a major enterprise.

Man, if provided with adequate life-support equipment, will be able to assemble large structures in space. Lack of physical strength will not be a limitation if the structure is properly designed, as weight does not impose the lifting problems found in the 1 g environment. On the other hand, length and mass of a structure could require heavy physical work if assembly were not done correctly. For example, if the assembler put into motion a long heavy structure from one of its extremities, he might have difficulty stopping its induced motion.

The keys to successful extravehicular assembly appear to rest in:

(a) The design of the structure
Nesting designs of parts for storage and transportation must include features that hold subassemblies, allowing the removal of one free part at a time. Joint mechanisms should have snap-in characteristics and locking devices that allow easy alignment and require a minimum number and specialization of tools. To avoid entanglement, minimum use should be made of guy wires.

(b) The design of the extravehicular work space
A structure in near-Earth orbit will pass through the Earth's shadow for a portion of its orbit. As a result, both structure and man will be exposed to extreme changes in temperature and illumination. One solution would be to surround the detachable storage portion of the vehicle with a large, inflatable, Echo-type balloon equipped with an "air-lock" hatch and tethered to the mother vehicle. The plastic sphere would be translucent enough to allow adequate internal illumination and yet reduce solar radiation. It would be inflated to a degree that would permit man to work unpressurized. (However, he would still wear his pressure suit for safety reasons.) Such a work area would have these advantages:

Illumination. With the balloon structure a large area source would be provided that would have light of sufficient intensity and of lower contrast than direct sunlight. The balloon would also diffuse or reflect light to fill shadow areas. The internal surface of the sphere would have high reflecting quality so that an internal low-power illumination could be used when the structure was within the Earth's shadow. Some detail would have to be painted on the inside of the sphere to give visual orientation and distance discrimination, thus avoiding any "whiteout" effects. The painted pattern should be of a minimal configuration to avoid the "Houston Astro-Stadium effect."

Temperature control. A structure in space would have a temperature approximating 250°F on the sunlit side and would be extremely cold when in shadow. The work-space sphere would narrow the range of temperature that would affect the assembler and would provide a measure of temperature control of the working environment.

Safety and efficiency. Such a surrounding balloon structure would eliminate the need for tethering the assembler or portions of the unassembled structure. Tools and structural members released without eliminating relative motion would not be lost as they could drift only to the edge of the sphere. Work-rest cycles would not have to be planned according to the time parameters of the orbit but, rather, could be planned around such factors as work-rest cycles, duration of the portable life-support system, and task-oriented work cycles.

Communication between the astronaut assembling or repairing a structure within the sphere and his associates in the space vehicle would be by radio. A redundant ratio system might be provided since the sphere would eliminate visual communication.

Economy and Applicability. Though the sphere should be useful during assembly of structures such as telescopes and antennas it would be even more useful during their operation. The sphere would reduce the effect of temperature extremes on the structures, thus reducing structural fatigue and misalignment. The sphere's usefulness in the operation of a telescope would be limited by the quality of a translucent window and the frequency with which direct Sun rays could impinge on the telescope hardware through this transparent aperture. The optical window configuration would apply to both types of structures if their attitude-stabilization systems were anchored on star references. It should be quite useful to maintenance men.

The influence of zero g on a pressure-suited man's performance in assembling a large structure has been studied using the neutral-buoyancy technique (Chaffee, in preparation). A Mark IV full-pressure suit adapted for pressurization with water to 3.5 psi was used. The faceplate area was pressurized at the same level with bottled air contained in a backpack of the same dimensions as an early design of the astronaut-maneuvering unit (AMU). The structure to be assembled consisted of telescoping modular truss-work. Each of the 6-ft modules was composed of a triangular truss and a member connected at its apexes by pins to three telescoping booms. The assembly was made rigid by three turnbuckle stiffeners. Three tasks, representative of typical modular assembly, were chosen for the study. The first required extension and securing of three parallel booms of module 'B' from their nested positions in module 'A' which had been assembled in advance. The worker then had to place the end truss within extended booms and secure them with 3/8-in. diameter pins 4 in. in length. Finally, the worker had to inspect the assembled structure and test it for rigidity. This required two transverses and two circumnavigations of the assembly.

The subject commented especially on the effortlessness of moving about from one part of the job to another and the mobility, which was estimated to be greater than that for a person in shirt sleeves in a 1 g environment. The anxiety induced by the possibility of having the faceplate accidentally flooded was estimated to be roughly equal to the fear of having a suit rupture in space.

The simulated space illumination (single area source, 5000°K, non-collimated) provided high-contrast lighting. The subject positioned himself so that the light was generally behind him. Illumination on the faceplate did provide a veiling glare, particularly if condensation or bubbles formed on either the interior or exterior surface. Whole body rotations were frequently used for better utilization of the illumination and to overcome the limited superior-inferior visual field imposed by the helmet and faceplate. The locking pins required trial and error procedures to insert them in the holes on the far side of the tubular members, and the triangular trusses were noninterchangeable as to position or orientation. These two design features increased the assembly time more than any other factors in the study. More than ever before, in the design of equipment for space use cognizance must be taken of the man-equipment interrelationship. The results of this exploratory study strongly suggest that man can contribute effectively to the assembly of large structures in space.*

Alignment and calibration. Man can contribute both to reliability and to better systems capability if he is used in alignment and calibration tasks. A modified Cassegrain telescope can be aligned within a half-hour if all the alignment controls are available to the operator at his viewing station. If, on the other hand, adjustments must be made remotely and verified each time by a return trip to the eye-piece, alignment might take 10 to 20 hr.

Calibration of a large dish antenna could be checked by man utilizing an optical aid supporting a reticle pattern. The reticle pattern, representative of the desired shape would be made visible to the scientist-astronaut alternately with a view of the antenna dish. Painted on the surface of the antenna would be reference lines duplicating those appearing on the reticle. The device would alternately present these two patterns in registry at 1.5 to 10 cps, and any apparent motion that could not be nulled out would represent an error in the curvature of the antenna dish. This apparent-motion comparator would display the information to man in such a manner that he could use his sensory and central capabilities to best advantage (see Section 2; Figure 6; and Webb, 1964).

Maintenance and Repair. Maintenance and repair involve, generally speaking, a repetition of the calibration and check-out procedures of the assembly task. They are somewhat different, however, in that they require analytical skills for fault location and place heavy demands on memory and training for the skills required to repair equipment. The efficiency of maintenance and repair rests in (i) design that is "planned for maintenance," (ii) the design, display, and ready availability of reference materials, (iii) the proper design and ready availability of tools, and, last but not least, (iv) man's skill.

*Scientists at the Valley Forge division of the General Electric Company have come to similar conclusions as a result of their neutral-buoyancy studies. Unfortunately, their studies have not yet been written up and came to our attention too late to be included in this discussion.

Inspection. Inspection can be divided into two meaningful categories. The first involves the inspection of one's own vehicle for its internal and external integrity. Man can compare how things should look with their actual appearance and thus detect incipient failures. Most automatic fault-finding systems respond after the failure has occurred. Materials that release an odor as a function of temperature have been suggested as a warning device inside the vehicle. However, an important consideration is the rate at which the respiratory supply system can remove the odor. Purging of the system would be critically necessary as olfactory adaptation is rapid, and if the warning odor were sustained within the system, it would be perceived only once.

The second category includes the inspection of other vehicles in space. For example, a detailed description of all external sensors, antennas, and solar power sources might be desirable, as would estimates of length, diameter, width, and volume.

Man's ability to estimate size at different distances has been studied extensively. The influence of illuminance, color of object, shape, method of matching, operating instructions, and body orientation are a few of the variables that have been considered. Judgments of volume, which appear to be important in space operations, have received much less attention. While man is not exact in such estimates, his constant and variable errors can be measured. In one study, 18 subjects, including 12 pilots, were asked to compare the volume of two very different solids displayed on a two-dimensional screen like a television image or a photographic projection (Kraft, 1950). Each slide presented one pair of images (from a total of 10 possible pairs), and the observers were asked to designate the image of the object having the larger volume. The subjects estimated volumes of the equal-volume geometric solids from least to greatest as sphere, cone, solid rectangle, cylinder, and cube. Figure 7 graphically illustrates his progression of estimates. If estimated volume were to follow actual volume, all bars would be of equal height at the 50 percent line. The dash lines at 39 percent and 61 percent represent the zone range within which the results could be explained by chance more often then once in 100.

These results indicate that a correction factor would have to be applied to volume estimates when the comparative standard was not a replica of the vehicle whose volume was being matched. The variable error is not as yet available, nor is man's performance in volume estimation relative to other methods of matching.

Summarizing this section, we believe that man can contribute to the assembly, alignment, calibration, maintenance, and repair of structures outside a space vehicle. He can contribute notably to the reliability and efficiency of the systems. In inspection, his cognitive surveillance will locate many failures before they occur. After more research, we can reduce his constant errors and be aware of his variable errors.

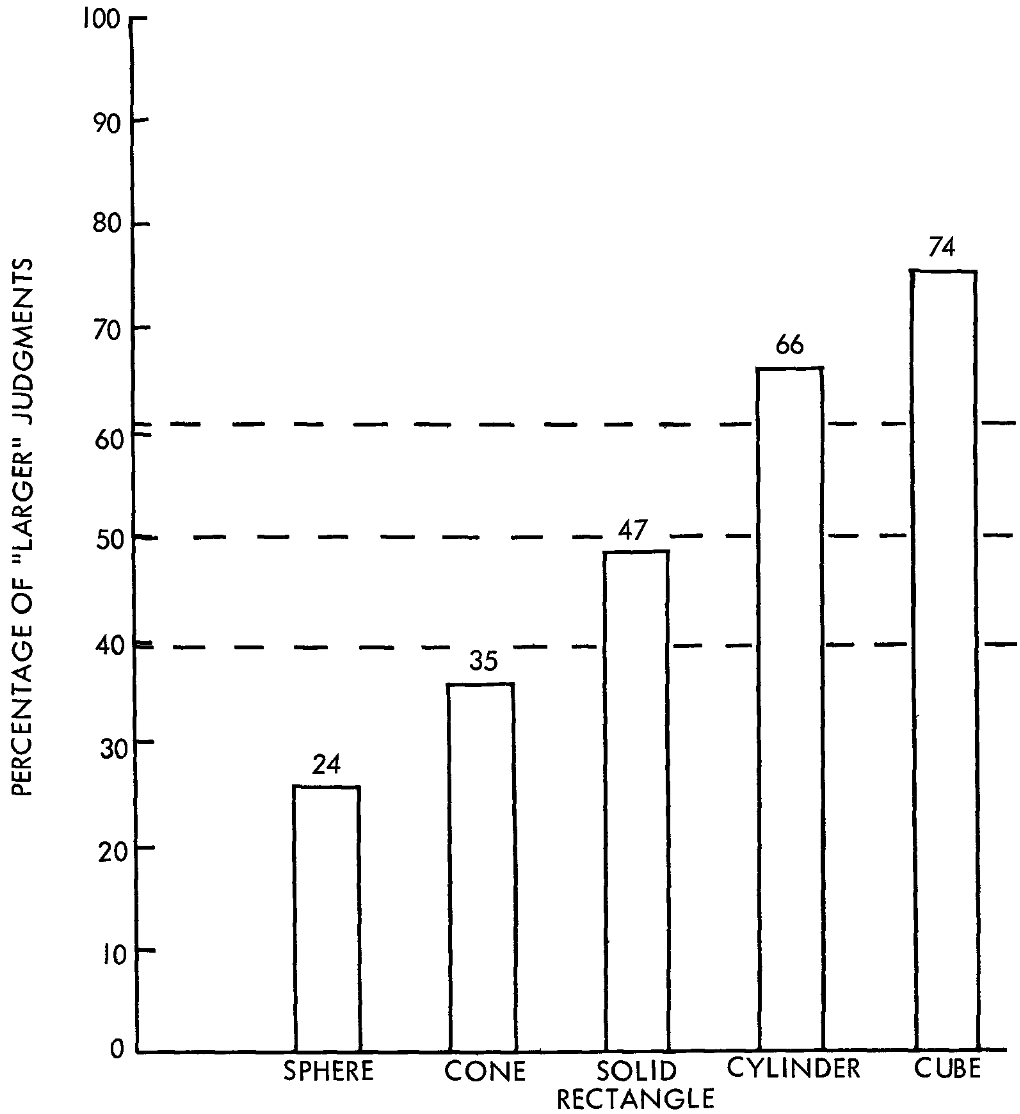

Figure 7. Paired comparison estimates of volume of five common geometrical solids. (Bioastronautics, The Boeing Company)

Locomotion and Tethering

Tether lines. Unless the astronauts are in a bubble structure or some other protective device, they must be tethered when working outside the space vehicle in order not to drift dangerously far from the vehicle. It might seem that a simple tether line could be used to "reel in" the astronaut should his personal propulsion system fail. However, an analysis by Mueller (1962) showed that this method is based on a dangerous oversimplification. Unless angular momentum in inertial space is reduced almost to zero for the isolated man, the vehicle and the man would collide with horrible consequences for the man. At this time we recommend that tether lines for retrieval purposes not be used if the distance between vehicle and man is greater than a few feet. In cases where a personal propulsion system has failed, retrieval would probably best be accomplished by maneuvering the parent vehicle toward the isolated astronaut.

Personal propulsion systems. Unfortunately, personal propulsion systems probably will not be the simple rocket-gun devices that served Buck Rogers so well. Because of the difficulty of locating the center of gravity of the body and because the center of gravity of an active human is constantly shifting, single-point propulsion generally results in uncontrollable tumbling (Mueller and Simons, 1962).

Simons and Gardner (1960) have developed requirements for a personal propulsion system, and Bell Aircraft, Chance-Vought, and Marquardt are working on devices that should eventually result in a personal propulsion system useful under a wide varity of circumstances.

Kasten (1962) has compared two possible systems for man to propel himself over relatively great distances. Test subjects propelled themselves quite successfully with one of the systems—success was measured in terms of accuracy, time, and fuel consumption. This system had two control sticks, the left controlling fore-aft thrust and the right controlling vertical thrust. We see no reason why thrust direction could not be determined by one combined control stick.

Centers of mass, moments of inertia, and principal axes. There have been few studies to determine the centers of mass of the human body and of the body's major elements. Investigators at North American Aviation, under contract to the Aerospace Medical Research Laboratories, have conducted studies with 66 nude male subjects to determine moments of inertia and centers of mass. The subjects were chosen as representative of the Air Force population in stature and weight (Santschi, et al., 1963). The procedure involved the use of the compound pendulum. It had a theoretical accuracy of between ±2-8 percent.

DuBois and others of North American Aviation repeated the study using 19 male subjects in pressure suits (DuBois et al., 1964). The moments of inertia were found to vary significantly with different body positions and between nude and suited subjects.

Hanavan (1962) designed a mathematical model to represent the human body. With this model he was able to predict the inertial properties of the human body in any fixed position. His model made use of simple geometric solids—right circular ellipsoids of revolution, right elliptical cylinders, spheres, and frusta of right circular cones—which were representative of 15 body segments. Hanavan prepared a design guide for the 31 major body positions that can be assumed in current full pressure suits. His procedures enabled him generally to predict known centers of gravity within 0.7 in. and moments of inertia within 10 percent. Perhaps more important to designers of astronaut maneuvering units, Hanavan could calculate accurately the principal axes of the body, showing, for example, that with different body positions principal vertical axis can deviate from the longitudinal axis of the body by as much as 50°. Such information will enable engineers to design maneuvering units that would exert their force through the principal axes of a body in virtually any position, thus avoiding cross-coupling effects.

Despinning the astronaut. There is a possibility that an astronaut working outside the space vehicle may find himself in an uncontrolled spin of high angular momentum. For example, one of the exhaust ports on his propulsion unit might stick in the open position, causing him to rotate very rapidly. Aume (in press) has suggested that a mass be attached to the astronaut's suit and that this mass be spring-released if the astronaut gets into an uncontrollable spin. Since the angular momentum is constant and the radius would be increased, the rate of rotation would decrease. For example, a 10-lb weight and a 30-ft line will reduce rotation from 100 rpm to approximately 5 rpm for a man and his equipment weighing a total of 300 lb.

Egress-ingress diameters. In an emergency it may be important that the astronaut be able to leave or re-enter the mother vehicle quickly and safely. Thus, an aperture is needed that is large enough not to impede an astronaut's progress through it, is completely free of sharp edges or protuberances, and yet is no larger than necessary, since additional diameter requires a disproportionate increase in the supporting structure of the vehicle.

A study conducted under weightlessness by Simons et al. (in press) is instructive regarding certain body motions under zero and lunar gravity and regarding some of the differences in time and error that might be expected under zero or lunar g. Each subject was required to leave one station, go through an iris, and seat himself at another station. The following are some of the more significant findings from that study. Time-score findings for the motions involved in egress and ingress include: (a) The mean time for all motions for subjects while in pressurized suits (2 1/2 psi) was approximately 25 percent greater under lunar gravity and approximately 40 percent greater under zero g than when subjects were not in pressurized suits. (b) The mean time for all motions required during egress and ingress was approximately 35 percent more under zero g than under the lunar-gravity condition (presumably because of poorer body control under zero g). (c) Approach to an aperture, progress through an aperture, and landing on the other side were completed faster when soaring was done headfirst than when it was done feet first. (d) Egress time was inversely related to the diameter of the iris.

Error score findings, with error defined as any contact with the side of the aperture, include: (a) Contacts were twice as frequent when subjects were suited (in a pressurized suit) as when they were not (presumably because of poorer body control in the suited condition). (b) Subjects made contact with the edge of the iris twice as often with a 1-in. clearance as with a 10-in. clearance. (c) Subjects touched the iris less frequently when they went through it with their feet first than when they went through headfirst (presumably because they could see their feet and thus control the lower half of their bodies better).

Experience and observation suggest that man, with practice, probably will perform certain tasks better under zero g than under 1 g—soaring from one place to another rather than walking or climbing, for example. A similar comment can be made regarding tasks under lunar gravity.

Donning and doffing the pressure suit. At best, it is very uncomfortable to wear currently available full pressure suits for extended periods of time. It would be better if men could wear them only during emergencies or while performing extravehicular activities. Therefore, it is important to know how long it takes to don a pressure suit under emergency conditions. Lt. Sasaki of the Aerospace Medical Research Laboratories has found this time to be approximately 2 1/2 to 3 min. Doffing time was only 1 1/2 min (Sasaki, 1964). This should be satisfactory for all except catastrophic penetrations of the vehicle, and this time should improve as additional attention is given to this facet of pressure-suit design. Although no measures of energy expenditure were made, it is important to note that the subject, an excellent athlete in prime condition, was extremely fatigued at the end of each trial.

Nonanthropomorphic maneuvering units. As early as 1960, Bell Aircraft Corporation proposed the REMORA system, which combines direct and remote manipulation. The REMORA concept (see Figure 8) appears to offer several advantages. First, if necessary, it could be shielded to protect the extravehicular worker in zones of high radiation. Second, a variety of arms may be used, each designed to serve a special type of operation. For example, one pair of arms might be of the gauntlet type for use on jobs requiring delicate manipulation. One arm might hold and provide power for tools such as drills; other arms might simply hold the work, leaving the operator's gauntleted hands free for productive work. (Industrial engineers tell us that the "hold" operation is probably the greatest source of inefficiency in assembly and maintenance operations.) The possibilities are almost infinite. Third, REMORA is pressurized, requiring the operator to inflate his suit only in emergencies. This feature would greatly reduce fatigue and extend useful time of work.

Tools and Scientific Instruments

Functional considerations on tools and instruments. Force-cancelling tools have stirred the imagination of many engineers and designers, and a number of companies have produced ingenious force-cancelling devices. Questions arise, however, regarding their usefulness in view of their relatively larger size and weight, the confined spaces where they would be employed, and, since they are complex, their reliability. It is expected that, within the vehicle, the number of surfaces against which a man can brace himself will be adequate for the counterforces demanded by hand-tool operation. Whether this will hold true outside the vehicle is not certain.

The use and control of tools when operating extravehicularly present three special problems—storage, recovery when loose, and need for special restraints. Unless being used in a bubble, tools must be contained and stored in special receivers or holders, for example, in a Velcro-lined

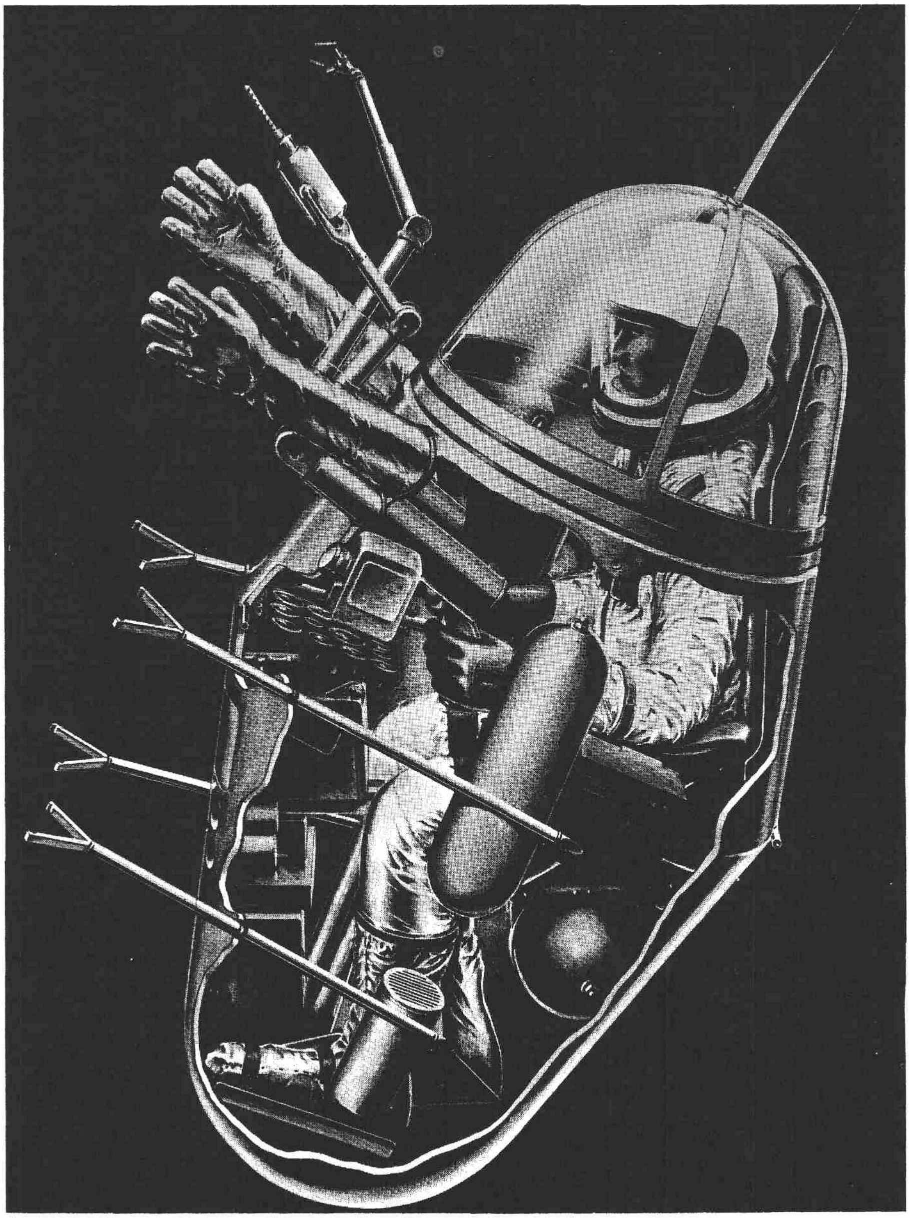

Figure 8. Cutaway view showing man in REMORA. Report No. D7052-953001, Bell Aircraft Corporation; reprinted by permission.) Note the man working in uninflated suit, the variety of manipulator arms, and the Gauntlets for delicate operations.

tool box contoured to the astronaut's body as suggested by Mueller and reviewed by Kristy and Roth (1964). Mueller also proposed a special nut/bolt retainer clip designed to hold those small parts that are most difficult to manipulate with a pressurized gloved hand. Perhaps force-cancelling tools outside of the vehicle should be battery-powered, since power lines would increase the risk of entanglement of the astronaut. Force-cancelling tools would appear to be advantageous only when the astronaut had minimal restraints or none at all.

In order to meet strict size and weight criteria, traditional scientific instruments must be redesigned for use in space, and scientist-astronauts must learn how to use the redesigned instruments. Another requirement, of course, is that no aspect of the instruments' operation can depend upon gravity.

Evaluation of Common Hand Tools. The relative usefulness of tools in zero g operations and in terrestrial gravity operations may differ widely. Mabry et al. (in preparation) studied the relative effectiveness of different hand tools in 1 g environment and in a neutrally buoyant simulation of zero g. Removing fasteners was chosen because of its broad applicability. Four types of fasteners, three sizes of fasteners, and seven different tools were studied in both land-based and neutrally buoyant situations. Three subjects performed 19 experimental removals under both conditions. The two major considerations were the time required to complete the tasks and the inability to perform the task (go, no go). Secondary aspects, such as damage to equipment and ease of using tools, were also studied. The tools were: crescent wrench (6-in.), open-end wrenches, box-end wrenches, nut drivers, Allen wrenches, Phillips screwdrivers, and blade screwdrivers. (The blade screwdriver data were complete only for the 5/16-in. size, and therefore do not appear in the analysis.) The fastener sizes were 3/16, 1/4, and 5/16 in., and the torque-down levels were the minimums/recommended for each size.

The results indicated a reliable difference between 1 g and neutrally buoyant environments, the mean times required for fastener removal being 44 sec and 61 sec, respectively. The difference due to sizes of fasteners could have been due to chance. The difference between tools was a reliable difference; Figure 9 illustrates the mean times for the tools arranged in order of rank for the zero g environment. Interaction between tools and environment was significant. It is illustrated by the different rank order (as to efficiency) that would hold for the 1 g environment. On the other hand, the environment versus size interaction was not significant. Also, the tools differed as to whether they chipped metal from the panels and fasteners, the crescent wrench being particularly poor in this respect.

The results of this study, if valid for zero g, suggest that similar maintenance tasks will take longer under zero g, tools will not have the same relative value in zero g that they have in 1 g situations, and fastener size does not affect performance time in these environments. The subjects encountered no situation in these tasks that would require force-cancelling tools. Breakaway forces were obtained by a rapid motion, a finger brace, or a toe-under-wall edge brace.

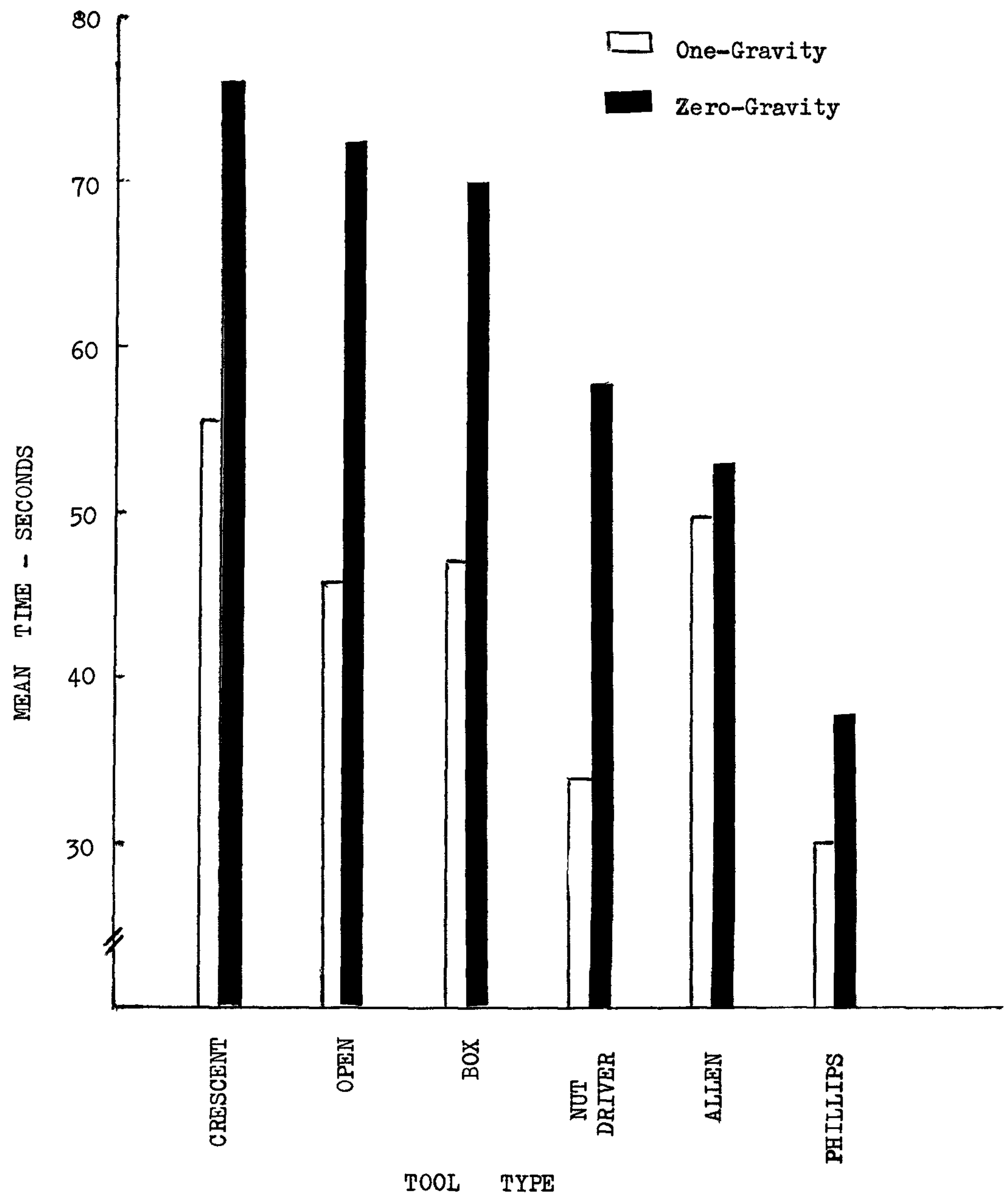

Figure 9. Environment and tool interaction for the fastener removal task. Prediction of tool efficiency is different for a 1 g environment and a zero g environment. (Bioastronautics, The Boeing Company).

5. LUNAR AND PLANETARY OPERATIONS

Major Tasks and Auxiliary Equipment

Exploration of extraterrestrial bodies probably will be preceded by the assembly of habitable structures on their surfaces. With the exception of weightlessness, the pressure-suited astronaut outside his vehicle, or later his habitat, is faced with problems similar to those involved in extravehicular operations in space.

The suit designed for lunar and planetary exploration will have to have a more extensive respiratory supply system to compensate for the existence of partial gravity and the increase in work load. The suit will also need the flexibility necessary to permit the astronaut to regain his feet if he inadvertently falls prone. It will have to be resistant to the possible accumulation of dust, which may be abrasive and electrostatic, and it will need a very effective cooling system.

Auxiliary equipment, especially designed to assist in lifting, maneuvering, and holding structures, should be available. Such equipment would be required for single-man operations and the assembly of objects beyond the reach of the suited astronaut.

Requirements will vary for scientific exploration as contrasted with experimentation. Man will need mobility to assure that samples are representative and sufficient. He may need manual dexterity to grasp and manipulate small particles. His qualifications, his equipment, and his program plan should enable him to perform preliminary analyses, to draw tentative conclusions, and to explore new hypotheses that are suggested in the course of his activities. He must be provided with sufficient auxiliary illumination to do detailed work in shadows and darkness.

He may require special training as to what to expect in the way of visual stimuli. The high degree of similarity between craters of all sizes, as well as other features, will make difficult the judgment of distance and the distinctions between areas. The short wavelength excitation of some surfaces (Kopal, 1965) has, through photography, been shown to emit extensive energy in the green and red portions of the visible spectrum. Especially at dawn, dusk, and night, therefore, the luminance in these particular areas will have a different color temperature than that of the Sun. Apparent size and distance relationships have been shown (Kraft, 1950) to shift with color temperature. For example, a 30 x 30 cm square at a distance of 48 m will be judged to have twice the area under 5500°K luminance than when seen under the lower color temperature of 2360°K. Or, conversely, an object of known size and 48 m away positioned among objects of unknown size (as on a lunar plateau) would be judged to be 36 m away under 2360°K luminance and 48 m under 5500°K. Such factors may add to man's difficulty in judging spatial relations on the lunar surface.

Man on lunar or planetary surfaces can study the scientific data obtained, selecting and organizing information to be transmitted to Earth. Communication loads will thus be reduced while the transmission rate of valuable information will be increased. Consultation with fellow scientists on Earth will be most valuable to the astronaut.

The successful prediction of man's behavior on the Moon or on another planet involves much more than mere weightlessness. We can simulate weightlessness and the equivalent gravities of the Moon and other planets for short periods of time, but we still do not know whether the Moon, for example, will sustain the weight of a man and his equipment. Consequently, it is impossible to discuss authoritatively the design and use of small vehicles. However, assuming that their surfaces are reasonably solid, we venture some comments concerning man's self-locomotion on such terrain. The ability to walk, at least with the gait employed on Earth, appears marginal. Kasten (1962) has conducted studies in aircraft at various levels of gravity and found that approximately 0.20 Earth g is required for walking. Theoretical calculations by Margaria and Cavagna (1964) seem independently to confirm these results. The addition of weight to the man enables him to walk under 0.165 g. The weight should be added near the feet, perhaps on the calves, since adding weight higher on the body tends to increase swaying motions.

Much has been made of the fact that man will be able to handle much greater masses on the Moon than on Earth, since weights will be approximately one sixth of what they are on Earth. However, since their mass will not vary, stopping with a large mass, manipulating it, changing directions, etc. will present problems.

It seems obvious that assembly and erection of structures on other bodies should be simple, direct, and capable of accomplishment with the simplest of tools or, better, with no tools at all.

6. SELECTION AND TRAINING*

Selection

The procedures used to select the first seven United States astronauts have been reviewed rather thoroughly elsewhere (Mitchell, 1962; Voas, 1961) and will only be summarized here. The seven were all military test pilots less than 6 ft tall. All were of superior intelligence, exceptionally stable emotionally, and capable of withstanding unusually severe physical stresses. We believe that the flights to date have vindicated the judgment of the physicians, psychiatrists, and psychologists who helped select the original seven. It should be recognized, however, that in the interests of safety and because of a lack of knowledge regarding job requirements, the selection committee probably established standards that were unduly stringent. These standards probably should be relaxed somewhat if we hope eventually to send into space large numbers of men with widely varying scientific and technical backgrounds. A definite need exists for

*Portions of this section taken or adapted from material in Psychological Aspects of Extended Manned Space Flight (Christensen, 1963).

the establishment of minimum criteria for the physical, mental, and emotional characteristics of astronauts—criteria that would curtail as little as possible the number of personnel otherwise qualified.

Training and Skill Retention

The training of the original seven astronauts also has been reported extensively elsewhere (Grether, 1961). It was designed to acquaint them with as many of the unfamiliar circumstances and sensations as could be foreseen and simulated on Earth; to keep them in excellent physical shape so that they could withstand the rigors of blast-off and re-entry and could endure the discomforts of many hours at their work stations; to familiarize them with virtually every engineering detail of their space vehicle and its booster; and to assure complete knowledge of the details of the job they were to perform. Familiarization with new situations and the overlearning of their jobs appear to have been particularly appreciated by the astronauts (NASA, 1962a, 1962b, 1962c). The soundness of overlearning from a psychological point of view is clear; in preparing for a space mission it is apparently better, for example, to have experienced weightlessness for even a few minutes than never to have experienced it at all. There is evidence that deterioration of performance under stress can be partially prevented by overtraining the incumbents in their jobs and by training them under conditions that approach those under which the jobs actually will be performed. The introduction of stress during learning enables one better to adapt to stress in actual situations (Klingberg et al., 1963).

There is a close interrelationship between selection, training, and engineering psychology. One can often simplify a job to such an extent that the incumbent need not have either special talents or training. On the other hand, proper training often will enable operators to compensate for poor engineering or will simplify the job of the design engineer and increase systems reliability. The secret, of course, is to strike a balance among such factors as quality and quantity of candidates; amount, expense, and type of training; amount of maintenance associated with various designs; effect on resources; time schedules in design and manufacture; and the myriad other factors that must be considered in the design and employment of a complex system.

A question arises as to whether the planners of scientific studies should take trained astronauts and teach them to carry out the duties of scientists or take trained scientists and teach them to perform certain operational tasks associated with the mission. We feel that the first alternative is impractical as a rule and that astronauts should restrict themselves to operation and maintenance of the vehicle. It may be practical to cross-train the scientist so that he can perform selected emergency operations; at least he could then guide the vehicle to a position where it could be controlled from the ground. However, complete cross-training of all crew members is impossible and should not be attempted.

During scientific missions of extended duration an acute problem could be maintaining adequate proficiency in all the skills required. Unfortunately, there has not been sufficient research on the long-term retention of skills. Dr. Morgan of the Aerospace Medical Research Laboratories, however, has given us a few suggestions based on the available information in this important area. For verbal/symbolic tasks (intellectual, procedural, etc.), any factor that influences learning is likely to have a similar effect on retention. If a factor facilitates learning, it generally will facilitate retention. Tasks that have a high degree of motor involvement as compared to verbal/symbolic involvement tend to be retained better. (This may be because such tasks generally are learned more thoroughly. In fact, extensive overlearning may be the best single safeguard against deterioration of skills due either to duration or to stress.)

Rehearsal of highly skilled tasks probably should occur on at least a daily basis. If provision cannot be made for rehearsal of a highly skilled task, then perhaps consideration should be given to redesigning the task so that it requires less skill.

It is unlikely that designers will permit the installation of any significant amount of training equipment in the space vehicles of the near future. However, practice may be possible by means of synthetic inputs into the real equipment. Even "imaginative rehearsal," as Morgan puts it, is probably effective. Techniques such as these—"dry runs" with actual controls, verbal rehearsal, and the like, should be used liberally. If any small simulators are required, their programming and scoring problems can probably be handled by one of the computers that almost certainly will be on board for other purposes.

Finally, we often have a choice as to whether to store selected information in the operator's central nervous system or in performance aids like checklists and manuals. We recommend heavy reliance on the latter; they do not deteriorate significantly with time and they are not affected by stressful situations. The weight of such materials could be reduced significantly by microfilming.

7. SUMMARY AND CONCLUSIONS

We realize that many of the statements in this paper lack the decisiveness and specificity that might be desired. There are at least two reasons for this: First, there is much still to be learned about human behavior generally and about human behavior in space specifically. Second, we believe that decisiveness and specificity will emerge only when specific requirements for specific missions have been developed and analyzed carefully. There is no quick or simple answer to the question, "What can man do in space?" We can only suggest (1) that extremely careful, detailed analyses of proposed missions be made, in which man's capabilities are constantly borne in mind, (2) that a fair amount of information already exists regarding man's capabilities in space and space-like environments, and (3) that suitable methodologies exist to test specific abilities that are not as yet elucidated.

Thus, requirements should be developed first and then the means available (man and machine) should be examined to meet those requirements. The examiners should consider carefully what a man has to offer as a functional subsystem and a scientific observer in support of the planner's attempts to meet mission requirements. After studying man for many years and considering carefully what others have learned about him, we feel that he has enormous potential for contributing to the success of complex systems, whether these systems be in space or elsewhere. Some of man's potential, in general terms, includes:

(1) His contribution to systems reliability by serving as a source of redundancy or backup for electro-mechanical systems.
(2) His contribution to systems reliability through his ability to diagnose and repair other subsystems.
(3) His ability to handle unexpected or low-probability events.
(4) His ability to evaluate and discard incorrect or inadequate hypotheses and, if indicated, to generate new hypotheses.
(5) His ability to interpret complex materials and events.
(6) The possibility of reducing the complexity of equipment because of his presence.
(7) The earlier availability of certain systems because man can do things that equipment cannot yet do at all, or cannot do with sufficient reliability.

We have not described man's abilities in detail, nor have we enumerated the principles of engineering design that should be followed if man is to operate effectively in space systems designed for scientific investigations. Such information already exists in comprehensive handbooks. There are, however, some considerations peculiar to space operations, which we have tried to bring into focus. Some of the more important with respect to performance under weightlessness and in space-suits are:

(1) The quality of man's performance under short-term weightlessness is decreased very little if he is properly tethered and if he is given appropriate tools.
(2) The quality of man's performance in a pressurized suit decreases significantly. Increases in time scores of 30 to 200 percent for the same task are not at all uncommon. The greatest increases are found in the fine manipulative tasks. The design engineer would be well advised to reduce to an absolute minimum the number of fine manipulative tasks that might need to be performed in a pressurized suit.
(3) Although to our knowledge it has not been accurately measured for a wide variety of tasks, there is no doubt that much more effort is required to do any job in a pressurized suit. An additional load is therefore placed not only on the operator but also on his environmental control system.
(4) If required, men who are properly motivated and part of a well-integrated crew can work productively for 12 to 14 hr a day for up to 30 days. If necessary, they can do so on a 4-hour-on, 4-hour-off schedule.
(5) Attention to the details of equipment and workplace design will decrease errors, save time, and significantly increase man's effectiveness (see also item 2, above).
(6) More consideration should be given to the possible uses of remote manipulators for certain tasks in space.

(7) Soaring appears to be a very effective means of traversing short distances in space (up to, say, 50 ft). It is one of what we hope will eventually be many examples of operations that can be performed better under weightlessness than under Earth gravity. Handling of large masses, for example, might be another (see also items 9 and 10).

(8) Equipment should be designed so that diagnosis of malfunctions is easy and repair can be carried out with a minimum of tools and, if possible, with no special tools. If the equipment must be repaired by a man in a pressurized suit, then the comment under item 2, above, applies.

(9) While some actions take more time under zero g, this time is expected to decrease as man gains experience under weightlessness. For some activities (e.g., those requiring coverage of a very large set of consoles) efficiency may be increased under weightlessness.

(10) While larger masses theoretically can be handled under weightlessness, the procedures for doing so are, as yet, not adequately developed.

(11) A new look at selection procedures for astronauts is needed. Undoubtedly some standards can be relaxed, but, of more importance, very careful consideration should be given to the exact education and experience backgrounds required for each crew member for each type of scientific mission. We do not recommend complete cross-training of all crew members because we feel that it obviates extraordinary competence in specific fields. We favor only enough cross-training to handle absolutely essential activities.

(12) The training of teams for scientific missions should include instruction in techniques to maintain requisite levels of skill in each of the tasks for which each member is responsible. Rather than use extravagant means to maintain skills that deteriorate rapidly, we recommend that, wherever possible, tasks and equipment be designed so that only a minimum of such skills is required.

(13) Check lists, taped instructions, and the like should be used as memory aids, particularly for those activities that occur infrequently.

(14) From the conceptual stage through the operational stage, extreme care must be given to the proper integration of man and machine in terms of established criteria of systems effectiveness. Mission profiles, related to a time line and showing tasks in as much detail as possible, should be initiated at the conceptual stage of development and should be continually refined as requirements and the operational activities needed to fulfill them become progressively more definitive.

(15) Finally, all systems, even the so-called "automatic" systems, are man-machine systems. It is axiomatic that environment plays a large part in determining man's behavior. In the scientific enterprises that we are considering, the space vehicle and its equipment are the environment, at least for the duration of the mission. If we design this environment properly, man's behavior will be exceedingly effective; if we design this environment improperly, man's behavior will suffer.

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APPENDIX A. HUMAN PERFORMANCE FUNCTIONS

Melvin J. Warrick

1. Introduction

To aid in considering man's role in space vehicles, it was felt that it would be useful to prepare reference lists of basic psychological functions or capabilities fundamental to mission accomplishment, safety, or personal well-being. Such listings would serve many purposes: specifying the details of man-machine-mission interaction, "flagging" critical human functions, suggesting abilities that may not have received adequate consideration in mission planning, etc.

Since these lists were prepared early in 1964 it has come to our attention that other organizations have found similar listings helpful. Our listing is somewhat more detailed, which may be both an advantage and a hazard, a hazard not only because it is more vulnerable to criticism but, more importantly, because it implies consensus and completeness. We claim neither; we claim only that it is a functional beginning.

For convenience we have classified the basic functions into three sub-groups: Sensory Input, Motor Response, and Central Processing. The distribution is arbitrary and even artificial. The input function of "visual acuity", for example, is of importance only as a part of a total process involving, perhaps, recognition, decision, and action.

Tests and techniques for measuring many of the functions are available; normative data about many of the functions are published. But the measurement tests or techniques are not uniformly valid and reliable, and the normative data are not equally comprehensive across the spectrum of functions. Much is known about visual acuity, little about target recognition. But even where much is known, extrapolating to the space environment must be made cautiously. Depth perception has been studied for a hundred years, but in space the conspicuous on-the-ground cues of linear perspective, texture gradient, aerial haze, etc., are no longer available.

It will be noted that we have not detailed such important terms as emotion, motivation, personality and/or behavioral disorders. This is not an oversight. In a sense these considerations overlie all listed functions, but we do not believe that they will be or will create problems for carefully selected crews.

2. Sensory Input Functions

The sensory inputs, both internal (coming from within the body) and external, are listed without particular regard to the human sensors—the sense organs—involved. Only basic inputs—those for which the corresponding sensations are innate or at least learned early in life—are included in this list. Interpretations (e.g., red means "stop") are included under "Central Functions."

For most, but not all, of the "sensory inputs," data are available concerning the range over which representative humans can operate, their absolute sensitivity, and their ability to detect changes. For many of these inputs, standard tests or techniques are available to assess human capabilities and limitations and the influence of environmental, motivational, training and other effects. The following are typical general characteristics of sensory inputs;

Intensity } absolute level and rate of change
Area and/or extent }
Location
Duration
Spatial and/or temporal patterning

Absolute identification (i.e., without comparison), absolute thresholds (minimum detectable), differential thresholds (noticeable differences), and simultaneous and/or successive discriminations (comparison) may be involved.

Visual (both central [foveal] and peripheral)
- Acuity
- Size-extent-area
- Intensity
- Brightness
- Contrast
- Form
- Location
- Distance
- Depth
- Spatial sequence and pattern
- Temporal sequence and pattern
- Motion
- Color (hue, saturation, brightness)
- Flicker
- Apparent movement

Auditory
- Loudness
- Pitch
- Complexity (purity)
- Concision
- Temporal sequence and patterning
- Location (direction and distance)

Whole Body Position. Under weightlessness and without body contact, vision will provide the major source of information. However, kinesthetic feedback systems and other sensory mechanisms in the viscera and loose body tissue may serve as a source of information while the body is being accelerated. While in movement from place to place within the vehicle, both linear and angular acceleration would be sensed through the otolith and canals, respectively. Auditory, temperature, tactile (touch), and other inputs may provide additional information.

Whole Body Movement
- Linear
- Rotational

Whole Body Accelerations. These may be sensed directly but not too precisely without visual cues. Body movement and position may be inferred from accelerations, but not accurately or reliably.

Linear
Rotational

Body Component (e.g., limb) Position
Body Component Movement
Body Component Accelerations
Static Muscular Tensions
Direction of Vehicle Acceleration (g)
Temperature
- Whole Body / Localized } { Hot / Cold } { Intensity / Localization

Touch and/or Pressure
- Intensity
- Frequency
- Duration
- Direction
- Direction of travel
- Localization
- Angularity
- Area
- Shape
- Roughness

Vibration
- Whole Body / Localization } { Intensity / Duration

Smell
- Type (specific odor)
- Intensity
- (Neither duration nor complexity are reliable inputs)

Time
Pain (external and/or internal)
- Intensity
- Location
- Duration
- Area

Other
- Moisture and Humidity
- Thirst
- Hunger
- Bladder
- Colon
- Somesthetic (visceral)
- Respiratory irritation
- Electric shock
- Air movement
- Itch - tickle
- Emotion (fear, apprehension, elation, euphoria—probably autonomic)

3. Central Processing Functions

There is presented below a listing of central (i.e., central nervous system-intellectual) functions. The naming of these functions is more a matter of convenience than of reality of function or of structure. It is doubtful, for example, that there is a faculty of "memory"; rather the "ability to remember" is, at least somewhat, specific: "I remember faces but not names." Furthermore, "functions" are seldom if ever exercised independently. One cannot, for example, compute unless he can discriminate one symbol from another. Thus, the distinction between the listed functions may in many, if not most, cases be more apparent than real.

It is unlikely that any two people would generate identical lists of functions, nor is it likely that one list would be appreciably more defensible than another. However, it would appear that virtually any list would contain, among the more important "central functions," learning, memory, and ability to use symbols as in "thinking" and communicating.

The purpose of listing these functions is to serve as an aid in recognizing, identifying, and describing the details of a man-machine mission, to aid in "flagging" those "functions" that may be most critical, and, possibly to motivate and direct the development of measures of the functions as they may be influenced by the mission and environmental demands. (At this time no particular effort has been made to rank-order or group the listed functions.) To use the list the reader will need, for accuracy and completeness, to add his own specific "of what - for what."

- Learning and
- Remembering
    - Procedures (SOP's), sequences, hazards, instruction
    - Actions
    - Techniques
    - Plans and/or profiles
    - Prior experiences of self and others (event - action - consequences)
    - Timing - what to do when
    - What leads to what - (if this, then that)
    - What to look for under certain conditions (e.g., symptoms and likely causes)
    - Vocabulary - reading-listening-speaking
    - Symbols - what stands for what
    - Standards
    - Normal behavior of instruments, controls, etc.
    - Spatial and temporal Relationships
    - Location of controls, displays, storeables, etc.
    - Dynamics of control
    - Motor coordination (manual skills)
    - Normal environment and normal body conditions
    - Mathematical operations, formulae, etc.
    - "Things to look for, or look out for"
    - Past conditions
    - Emotion - fear, elation

- Reading and Comprehension (dials, printed materials)
- Identifying of "targets" etc. (perceiving form, size, context, etc.)
- Body and Vehicular Orientation and Motion
- Detecting signals, objects
- Recognizing and categorizing patterns

Monitoring
Vigilance
Discriminating, comparing and matching
Detecting, Identifying or Inferring
Classifying (by size, shape, color, duration, intensity, etc.)
Spatial Relationship
Temporal Relationship
Generalizing
Associating (e.g., action and consequences, cause and effect)
Choosing, executing, and monitoring appropriate action
Verifying appropriateness of action
Auditory Comprehension (speech, auditory display)
Comprehending through noise (S/N not necessarily restricted and audition)
Vocalizing (selecting and generating appropriate spoken words)
Counting
Estimating
Computing
Assessing
Analyzing
Problem Solving
Synthesizing - Integrating (present)
(present and past)

Generalizing and Concept Formation
Hypothesis formation and testing
Guessing - approximating
Planning
Predicting - Extrapolating
Evaluating (present)
(consequences)

Timing
Ordering - Sequencing
Coordinating input and output (e.g., tracking, visual and auditory)
Other "Functions" or characteristics
- Improvising
- Exploring (curiosity)
- Flexibility - reprogramming
- Self awareness and monitoring
- Operating on incomplete information

4. Motor Response Functions

Voluntary. Overlaid on most of these are:
- Static (holding)
- Dynamic
- Reaction latency
- Precision and control
- Speed of motion
- Extent of motion
- Duration of motion

Strength and/or force
Complexity
Coordination
Agility
Energy expenditure
Feedback (e.g., location, identification, etc.; see "Sensory Input Functions")

Arm, Wrist, Hand, Fingers and Thumb

Transporting Push - pull, etc. Grasping Rotating Holding	} Self, materials, controls, tools

Leg, Ankle, Feet

Hold Push Rotate	} Self, materials, etc.

Body - gross segments (as in orientation)
Tracking - Input-output coordination
Vocalization
Eye Movements
Others
Head
Mouth - chin

Involuntary
Sweat
Eye blink
Cross extension
Withdrawal - protective (e.g., from pain)
Startle
Righting
Pupillary
Cardiovascular and respiratory (e.g., cough)
Gastro-intestinal
Swallowing - peristalsis - vomit
Internal duct glands (e.g., salivary)
Homeostatic

APPENDIX B

PRESENT KNOWLEDGE AND METHODS OF OBTAINING OR SIMULATING ZERO GRAVITY

James E. Mabry

Devices for Producing or *Simulating zero g*	Description	Results	Advantages	Disadvantages
"Pretend," i.e., No zero g effects	Ground based simulation studies. No zero g effects	Conflicting. Depends on conjecture.	Cheap; fast	Design requirements conclusions often not valid when tested in Mercury or Keplerian trajectory; e.g., tool use, O_2 consumption, Gemini capsule. Materials show no zero g effect.
Keplerian trajectory	Parabolic aircraft flights. Zero g for 14-60 sec	Primarily qualitative man's capability greater than in 1 g on some tasks, less than in others; different in all gross motor tasks. No up or down. Workspace design should not be based on 1 g concepts.	Actual zero g. Man and materials exhibit real zero g effects.	Very brief duration. 2-1/2 g at insertion and pullout. Costly. Difficult to schedule due to high maintenance down time. Limited size. Extensive support personnel training required.

Devices for Producing or Simulating zero g	Description	Results	Advantages	Disadvantages
Swing	Suspension at 1 g in "Peter Pan" harness or in seat by cable or rod from traversing support (balloon or trolley)	Conflicting. Capability and requirements depend on suspension. Man's capability and requirements in tractionless environment different from 1 g.	May be cheap—not necessarily	Body suspension strongly influences results. High inertia. G/mass imbalance cause unrealistic problems. Materials show no zero g effects.
Water immersion (Orientation)	Tilt table and/or instrument reading in darkened and/or rotating tank.	Difficult to tell up from down in poor lighting. No reports from rotating tank studies.	Partially simulates perceptual zero g effects reported in Keplerian trajectory Mercury, Vostok, etc. No time limit. Cheaper than Keplerian or Mercury program.	Underwater breathing equipment requirement. No studies of tasks and situations representative of systems design/operations/maintenance problems.
Air bearing	3 to 6 deg of freedom, low friction body suspension (at 1 g)	Limited and conflicting. Capability and requirements depend on body support design. Shows greater instability in some tasks than Keplerian or Mercury; much less in other tasks—strong biases.	Low friction relative to "pretend." Cheaper than Keplerian or Mercury. No time limit.	Large inertial due to mass of mechanical body suspension. Unstable due to g/body mass couples. Limited design study variety. Only Mercury and Keplerian more expensive. Materials and test components show no zero g effects.

Devices for Producing or Simulating zero g	Description	Results	Advantages	Disadvantages
Mercury, Vostok, etc.	Vehicles in space	Man's capability for operation much greater than previously thought. Long term physiological problems confirmed; e.g., cardiovascular. Definition of tasks much different at zero g than 1 g. Basic difference in design philosophy required.	Real thing—not a simulation	Costly. Time consuming. Must have high "first time" reliability. Study programs inflexible once started. Extensive support personnel training required.
Neutral buoyancy	Controlled environment water tank and submersible full scale systems mock-ups.	Limited at present. Qualitative effect on task performance (e.g., hatch traverse) very similar to Keplerian studies. Quantitative measures show increased capability with less logistic cost (e.g., air consumption).	6 deg of freedom from g effects. No artificial effects from mechanical body suspension. No time limit. Materials can show zero g effects. Great flexibility in test modifications—rapid, easy. Relatively inexpensive. Relatively little danger. Unlimited space for full-sized mock-ups. Little training needed for support personnel.	Viscosity of medium. Underwater breathing system. Modification required for underwater operations for certain items. Minor visual adaptation (mask-water interface). Slightly hazardous.

XI
Role of Man in Space Research

INTRODUCTION AND CONCLUSIONS

The Working Group on the Role of Man in Space Research was drawn from participants in each of the other Working Groups of the 1965 Summer Study at Woods Hole. Meeting on July 6-7, it discussed, in broad terms, the limitations and advantages of man as an experimenter in space, the nature of the role or roles to be played, the relationship of manned and unmanned space research, and the selection and training of flight personnel. Also discussed were the scientific experiments planned for the current Gemini and Apollo programs, and the scientific importance of large manned orbital laboratories capable of flights of extended duration. The Working Group was not designed to study these questions exhaustively but rather to assess generally and informally, on the basis of the disciplines and experience represented by those participating, the scientific role of man in space research.

The general conclusions of the Working Group are as follows:*

1. Scientifically satisfying studies of the planets will require the presence of scientists, preferably on the planetary surface where they can make direct observations. If that is not feasible they should at least be in a spacecraft orbiting closely enough to the planet so that communication time delay and power bandwidth considerations will not seriously limit the performance of remotely controlled instrumented vehicles on the planet. It is clear that here man is essential.

2. There are many ways in which men can be usefully employed in space for scientific research: as observers; for the assembly, placement, repair, and operation of scientific instruments; for preliminary analysis, screening, sampling, data collection, storage, and retrieval.

*Each of the Working Groups of the 1965 Summer Study gave consideration to man's role in space research; specific comments may be found in their individual chapters.

3. Few if any scientists would attempt to justify the entire cost of developing manned space flight solely on the basis of its "scientific value;" however, most scientists would agree that this capability, when developed, should be utilized for scientific purposes whenever it seems possible to do so.

4. There appear to be no a priori reasons why the physiological and behavioral stresses imposed on man by the space environment should prevent him from surviving and functioning effectively in space during missions at least of Apollo duration.* Such was the judgment of the Summer Study's Working Group on Medicine and Physiology (see Chapter X). The Working Group on the Role of Man in Space Research also believes that most physiological constraints will yield to work now in progress, and that man may in the future embark on longer missions with reasonable confidence that he can perform meaningful scientific studies. It was clearly recognized by all participants, however, that ground-based research must be carried out on those human systems likely to be affected by the space environment (e.g., cardiovascular, respiratory, renal, and digestive).

The experience of the United States and the Soviet Union in increasingly longer manned flights, and in extravehicular activity, together with the progress that has been made in preparation for manned lunar landings, has stimulated increased interest in directly using man's abilities to explore space—from within spacecraft, in appropriate pressure suits outside the spacecraft, and on the surface of the Moon or planets. Certainly many questions regarding man's adaptability to the space environment remain unanswered, but given the opportunity to progress in a scientific and orderly way to successively longer and more complex missions, it is expected that spacecraft and life support developments will emerge that would enable him to live in space for protracted periods and to accomplish almost any desired task. As a result, scientists will become progressively interested in the ways that men can be used to advance scientific research in space—as scientific observers, as highly skilled technicians, and as the subjects of experiments themselves.

The Working Group visualized four immediate types of programs utilizing man for scientific studies in space: (1) The current Gemini and Apollo programs, which can carry modest scientific experiments to be performed by the astronauts, but where scientific objectives are secondary to engineering and human survival; (2) later manned orbiting laboratory flights, such as the Manned Orbiting Laboratory (MOL), Apollo Extension Systems (AES), or the Large Orbiting Research Laboratory (LORL), and their modifications, where an important objective of the mission is to carry out relatively demanding scientific measurements and observations; (3) large, complex, special-purpose space assemblies, such as optical or

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*Note added in proof: Astronauts Borman and Lovell in GT-7 recently completed a 2-week orbital flight, with no apparent ill effects.

radio-astronomical observatories that are designed specifically to be assembled, aligned, serviced, or operated by men in space; and (4) scientific operations on lunar or planetary surfaces.

ROLE OF MAN IN SPACE RESEARCH

In order that the greatest scientific returns be achieved by including man in space research, those capacities that make him superior to automatic or remotely operated equipment must be fully utilized.* For example, man can make substantial contributions in tasks that require scientific judgment, e.g., studying data and evaluating preliminary results; making a complete record of an experiment or group of experiments being done simultaneously; making decisions as to which scientific results are significant and should therefore receive further study; planning a program of observations on the basis of results as they are obtained; operating complicated instruments and equipment so that maximum and most significant results are obtained; utilizing his ability to reflect, theorize, and make and test hypotheses in order that full advantage may be taken of every opportunity; surveying a large field of view from an orbiter in order to select specific areas and times of interest for photographic recording and to supplement such records with descriptions of significant phenomena.

While the foregoing deals with the role of man as a scientist, there are other roles that require a high level of intelligence, ingenuity, and technical skill. In this category may be included such tasks as the return of samples or data in physical form; alignment and adjustment of precision equipment; placement of complex apparatus, especially that which requires protective cover during launch and flight; removal of worn-out or nonfunctioning subsystems; and equipment maintenance and servicing. (One has only to reflect on the extreme difficulty of maintaining complex precision equipment on Earth to realize the importance of maintenance in space.)

RELATIONSHIP OF MANNED AND UNMANNED RESEARCH

In deciding whether to use man as part of a data collection and analysis system, or to have it completely automated, scientists would certainly

*For a discussion of this subject, see Appendix 8 of Chapter X.

favor using man if (1) it is the only way to obtain the necessary data or (2) it is the most economical way in the broader sense, i.e., in terms of expenditure of time and effort per unit result. Finally, if the presence of man in the system were already considered essential, the scientist would assign many scientific tasks to man because of the greater reliability and flexibility he would bring to the system.

An unmanned system has only an estimated statistical probability that it will continue to operate reliably for its designed lifetime; manned intervention increases reliability through the possibility of extending the lifetime of spaceborne equipment almost indefinitely by means of repair and replacement. Intervention by man greatly increases the flexibility of the system, for he can decide not only the most interesting or profitable way to use an instrument at a particular time but also can make alterations and improvements in the instrument itself. Data transmission can, of course, be virtually eliminated for manned experiments, and the design of an instrument can be simplified if every operation does not have to be automated. Furthermore, several lines of investigation may be carried on simultaneously. Most importantly, man can select significant items from a large field of view and notice relationships among phenomena that no preprogrammed instrument can discern.

Since such abilities make the presence of man very attractive for most investigations, the Working Group felt it necessary to address itself to the questions: (1) At what point does it become justifiable to utilize man if he is not essential? (2) Is such use ever justified when the economic tradeoff is not clear-cut? This section will summarize some of the Working Group discussions that occurred.

MONITORING

Unmanned spacecraft should, in general, be used for the monitoring, continuous measurement, or sampling of physical variables (e.g., solar radiations of various kinds, particle fluxes and magnetic fields, components of the neutral and ionized atmosphere). The physical variables of interest in such researches are known; the data are routine and in themsleves not very exciting from minute to minute; the data gathering process requires no exercise of judgment and there is generally no reason why a man should be present while it is going on. However, when it is necessary to monitor simultaneously a large number of critical variables in order to gain a complete picture of some typical process, a man can be useful. An example can be found in the study of the physics and large-scale behavior of the upper atmosphere of a planet—meteorology, in the broad sense of the word. In such a situation each instrument can be automated easily enough and have a statistical probability of working for the required interval, but it may be much more difficult to achieve satisfactory over-all reliability for the complete system.

SEQUENCE OF MANNED AND UNMANNED INVESTIGATIONS

Manned and unmanned investigations can and should be arranged so that each complements the other most effectively. Unmanned investigation of hostile environments is obviously desirable in advance of manned exploration. Thus, the programs of unmanned lunar investigations—the Rangers, Surveyors, and Lunar Orbiters—in preparation for Apollo are wise, and unmanned exploration of Mars should be done in the same way.

Examples of projects which should be manned before being completely automated can be drawn from other chapters of the Summer Study report, for example, in Chapter II. Here, the Working Group on Optical Astronomy cites justifiable misgivings on whether a very large diffraction-limited telescope mirror can be figured on the ground under normal gravity and still be made to retain its figure under weightless conditions. That Working Group also expresses concern with the technical problem of exact optical alignment after the telescope has suffered the stresses of launch. There is a desire to see whether these problems can be solved with a man present to carry out the trial-and-error approach that has always been found essential on the ground. A comparable procedure will doubtless be proposed for the Apollo Extension Systems. The current Gemini and Apollo programs can conduct simple photographic surveys to "see what is there" in order to optimize the design of, and to plan observing programs for, larger and more complex instruments.

PREPARATION FOR LONG-DURATION MANNED SPACE MISSIONS

If manned expeditions are to go someday to a planet (e.g., Mars), a manned space station clearly becomes essential for the gathering of the medical, physiological, and behavioral data needed to determine how man may survive in space in reasonable comfort for periods of 2 or 3 years.

COMPLEX SCIENTIFIC RESEARCH PROGRAMS

An orbiting station will probably be necessary to carry out the biological experiments proposed by life scientists, such as those requiring very detailed and selective manipulation of biological specimens, examination and recording of selected events under a microscope, the rearing in space of generations of advanced life-forms and their use in experiments, and surgical or similar procedures using plant forms. Large systems like the large orbiting telescopes proposed by the astronomers (Chapters II-V) are somewhat less complex: it is possible to contemplate automating all the steps in their operation so that such systems could be kept running efficiently between occasional manned visits for maintenance. A manned space station within easy reach of a large orbiting astronomical facility would permit the design and management of that facility to be greatly simplified. Astronomers have been considering the advantages of a system comprising a telescope and a space station (near by or very loosely coupled to the telescope) so that man can get aboard quickly and frequently when needed but not be present while the telescope is operating in the fine-pointing mode.

SELECTION AND TRAINING OF SCIENTISTS FOR SPACE MISSIONS

The Working Group examined the relative merits of training of scientists as astronauts or training astronauts as scientists. For some tasks—those requiring scientific insight—it would seem better to have a scientist possessing judgment, experience, and imagination and to train him as an astronaut to the extent required. However, for duties other than these, it would probably be preferable to train astronauts to carry out scientific activities or at least to work as technicians and observers. Thus, an experienced field geologist would almost certainly have greater grasp of and insight into what he observed and sampled on the lunar surface than would an astronaut, regardless of high motivation or a technical background sufficient to learn basic geology in short order. But it also seems clear that for maintenance, repair, or almost any task where manipulation rather than scientific observation is important, an astronaut is probably to be preferred to a scientist, who may or may not be familiar with such tasks. Furthermore, in a situation where a variety of straightforward scientific tasks in quite different fields must be performed, there is no reason to believe that the competent scientific specialist will be more expert outside his own field than a highly motivated and well-trained astronaut starting with a good technical or general scientific background.

The Working Group further agreed that (1) it is timely to consider ways in which "scientist-passengers"* can be incorporated into space crews. As scientist-astronauts, older, more experienced scientists might be disqualified on age or medical grounds; these requirements should now be re-examined. For example, in situations requiring visual scientific observation, experience in interpretation is much to be preferred over acute naked eye vision. It would appear that medical requirements for the

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*See A Review of Space Research, National Academy of Sciences—National Research Council Publication 1079 (1962), Chapter 11.

scientist-passenger should be progressively relaxed to the point where a candidate would be disqualified only for defects that would make him a hazard to the mission. (2) Although the pilot-astronaut will be essential for a long time to come, training of a scientist-astronaut as a jet pilot might be reduced and perhaps finally eliminated. Experience in simulators suggests that the control of a spacecraft (attitude control, rendezvous, docking, re-entry maneuvers) is not intrinsically too difficult for a reasonably well-coordinated person to learn. However, jet and test pilot training contributes one valuable ingredient: it cultivates a continual, almost subconscious awareness of the possibility that an emergency may occur at any moment and, by constant practice in dealing with simulated and real emergencies, gives confidence in one's ability to deal with them.* Ways should be sought by which this process can be accelerated and done more simply.

In summary, the Working Group suggests that the astronaut selection and training program take into account progress in manned space flight. Successes so far may be viewed as evidence that it may be possible to relax the present high physical standards at a pace faster than has yet been contemplated.

GEMINI, APOLLO, AND SCIENTIFIC FLEXIBILITY

In reviewing the presently planned Gemini and Apollo scientific program (see Appendix 2), the Working Group affirmed its value as an exploratory program and commended the use of Gemini and Apollo to try out simple ideas and experiments, especially those that might be regarded as forerunners of more ambitious projects for the Apollo Extension Systems and later systems. This may be the most valuable opportunity for science in these early manned flights.

The Working Group recognized that engineering and mission constraints prevent the derivation of maximum potential scientific benefit from the Gemini and Apollo programs. The primary purpose of these flights is to develop systems able to support man in space for limited durations and to assure his safety, within reasonable limits, during such flights; scientific activities must be carried out on a noninterference basis, subject to cancellation. Despite these constraints, it is likely that numerous opportunities will exist, up to within a few weeks of launching, to place modest

*The performance of the crew during the abort of the October 25, 1965, launch of GT-6 provides a good illustration.

experiments on manned space flights. The Working Group suggests that no such opportunity be lost and encourages NASA to examine ways to exploit them.

While the Working Group subscribed to the existing goals of the Gemini and Apollo programs, and recognized that the predominance of scientific objectives must be deferred until those goals are met, it wished to emphasize the following point: In order to attract many competent research scientists to the type of space experimentation that involves man, it is desirable to create a reasonable level of assurance that the time and effort they invest in planning and preparing a manned flight experiment will lead to the accomplishment of that experiment. Some risk must, of course, be accepted, but it is important that an effort be made to minimize it. Such considerations may suggest that some adjustments in procedures and attitude toward the experimenter may be necessary to encourage the scientific community to use this new capability in space research.

APPENDIX 1: LIST OF PARTICIPANTS

WORKING GROUP ON ROLE OF MAN IN SPACE RESEARCH

Porter, R. W., Chairman	General Electric Company
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APPENDIX 2

THE ROLE OF MAN IN THE GEMINI AND APOLLO SCIENCE PROGRAMS

Willis B. Foster

It should be stated at the outset and be clearly understood that the Gemini and Apollo programs are aimed, as was Mercury, at the development of man's capability to operate in space. The development of this capability has been an evolutionary one—starting with one man in a ballistic trajectory, one man in extended orbital flight—two men in orbital flight, extravehicular activity—eventually rendezvous and docking, lunar landing, and long-term space flight.

The science program in manned flights has been similarly evolutionary, starting with simple experiments, involving the astronaut as a sensor—leading eventually to long-duration flight during which the astronaut will carry out complicated investigations, possibly even of his own design. The scientific exploration of the lunar surface will involve astronauts thoroughly trained in the geosciences (geology, geophysics, geochemistry, volcanology, etc.). Not only do we hope to explore the Moon but possibly even exploit it as a base for scientific investigations other than those in the geosciences.

The Gemini flights are very restricted in weight, volume, and choice of orbit for scientific investigations. The experiments are, by and large, being carried on a "space-available" basis. In spite of these restrictions, however, each Gemini flight between now and 1967 (8 are planned) will be carrying anywhere from 6 to 21 experiments.

There are five types of experiments on each of the flights and much competition for the available space and weight carrying capability. The competing groups are: (1) engineering experiments needed by the Manned Spacecraft Center to assure the safety of future flights; (2) experiments proposed by the Department of Defense; (3) medical experiments; (4) scientific experiments; (5) technological experiments proposed by the Office of Advanced Research and Technology, on advanced techniques.

Gemini. As of June 16, 1965, the following were approved for Gemini flights: 10 experiments from engineering, 16 from DOD, 12 from the Office of Manned Space Science, 3 from OART, 11 medical experiments. Many of them will be repeated, since we shall be looking for as much information as we can get from these early flights.

It is an evolutionary program in terms of use of man in the system either as the investigator or investigatee. The photography experiments (zodiacal light, terrain, weather, cloud-top, airglow, and ultraviolet sources) all use the astronaut as a sensor—he must also use some judgment in his role as an evaluator—in selecting the targets for his photography and in choosing the correct films, filters, shutter settings, and exposure times. Man is also useful in assuring proper storage of the film after exposure and in bringing it back after the flight.

This picture is of course oversimplified. In each of the six experiments involving photography the astronaut is required to have training in the science involved in order to know how to handle the unexpected; he must execute complicated spacecraft maneuvers in order to optimize the exposure required by the principal investigator. Ney's zodiacal light experiment, for example, requires a roll, pitch, and yaw maneuver, a 1-min exposure with the spacecraft held steady followed by a 30-sec wait, then a repetition of the entire sequence.

Another aspect of the astronaut's role in space science is being demonstrated by another five of the scientific experiments in the Gemini program: the sea urchin eggs, the frog eggs, the blood cells, the nuclear emulsions, and micrometeorite collecting experiments all require him to become involved as a manipulator. As in the case of the photographic experiments, the astronaut also acts as an evaluator, the extent to which he acts depends on the degree of his training in the particular field of science in question and the amount of time invested by the principal investigator in working with the astronaut. These experiments also require special spacecraft maneuvering and attitude control, in addition to actuation by the astronaut.

Two other scientific experiments scheduled for the Gemini flights involve the astronaut even more: these are Duntley's visual acuity investigation and Medved's (EOS) investigation of the ion density in the wake of the Agena just before completion of the docking maneuver. The first of these involves the astronaut, himself, as a subject; he will be asked to identify and photograph prepared optical targets on the ground. The second of these involves very careful handling of the spacecraft during the rendezvous and docking operation.

Apollo. Apollo is two separate programs insofar as science in concerned: the in-flight program and the lunar landing program. The in-flight program makes use of the Saturn I-B and Saturn V in circumterrestrial orbit and possibly also circumlunar orbit before landing on the Moon. Science is somewhat better off here than it is in the Gemini program: the same problems exist but they are not quite so acute. There is a wider choice of places to locate experiments and, if recovery is not essential, the Service Module provides one whole bay and several thousand pounds of payload.

NASA had identified and approved 25 experiments for these flights and 50 more have been proposed and are awaiting disposition. They are, in some cases, repetitious of the experiments flown on Gemini, with minor modifications; many, however, are bigger and more complex experiments. Even more significant than the experiments themselves has been the development and the installation of an airlock for the Command Module, and the development of an independent pallet which can be fitted into the Service

Module. NASA has a very exciting proposal from Ball Brothers for the construction of a pallet which could be ideally suited for a solar astronomy experiment. NASA will press to fly this experiment as early as possible.

Other concepts being pushed to make best use of the Apollo flights are the subsatellite and the independent life-support system for small animals. The subsatellite is being developed for those experiments that should be carried out as far from the manned spacecraft as possible, e.g., experiments needing true zero-gravity, or a very weak magnetic field. The independent life-support system is being developed to accommodate small-animal experiments. The only one approved to date is a study of the otolith function in frogs; this one is scheduled for one of the early Apollo flights. Man's function in all of these types of experiments is as a laboratory technician.

Another major group of experiments being rushed to completion for the Apollo circumterrestrial and circumlunar flights includes those involving the use of remote sensors (infrared, ultraviolet, radar, and photography) for the study of the terrestrial and lunar surfaces. These experiments constitute the basis for a program in the application of space technology to agriculture, forestry, hydrology, oceanography, and the mapping of mineral resources. All these experiments require the astronaut to select appropriate targets, to operate the equipment, to control the spacecraft throughout the duration of the experiment, and to store and preserve the film, etc., properly.

The second important part of the Apollo science program is the part to be done on the lunar surface. It is here that optimum use of the astronaut as a scientist and explorer is made. The three major aspects to this program are:

(1) Geological exploration of the Moon on foot in the immediate vicinity of the landed lunar excursion module, with special hand tools now being developed. This activity requires that the astronaut be a well-trained observer, possessing that rare quality required of our scientist-astronauts, perspicacity.

(2) The emplacement on the lunar surface of a package of experiments which the astronaut will emplace, actuate, and leave behind. The instruments will be designed to function, collect, and transmit data for 6 to 12 months. The package as now conceived will contain 4 to 8 experiments, including, for example: active and passive seismometry and measurements of gravity, magnetic fields, trace atmosphere (if any), heat flow, radiation, radio activity, and meteoroids and micrometeoroids.

(3) The selection, collection, packaging, return, unpacking, distribution, and analysis of biological, geological, and atmospheric samples from the Moon. Here again the astronaut must be a highly trained observer and at the same time an accomplished field worker.

NASA is planning the construction of a lunar sample receiving laboratory which will test for short-lived radioactive nuclides in a low-level counting facility, collect and analyze any occluded gases in the sample, and provide a biological barrier behind which any viable organisms which turn up in the lunar samples may be identified and isolated.

Manned Space Flight Experiments Board
Approved Gemini and Apollo Experiments

Summary (Rev) June 16, 1965

No.		G, Gemini A, Apollo
Center Engineering		
MSC1	Electrostatic Charge	
MSC2	Proton-Electron Spectrometer	G
MSC3	Tri-Axis Magnetometer	
MSC4	Optical Communication	G
MSC5	Lunar UV Spectral Reflectance	
MSC6	Beta Spectrometer	G
MSC7	Bremsstrahlung Spectrometer	
MSC8	Color-Patch Photography	G
MSC10	Two-Color Earth's Limb Photos	
MSC12	Landmark Contrast Measurements	G
MSC13	Subcritical Cryogenic Storage	
MSFC1	Dielectric Materials Evaluation	A
MSFC3	Propellant Mass	
MSFC4	Liquid Interface Stability	
DOD Technological		
DI	Basic Object Photography	
D2	Nearby Object Photography	G
D3	Mass Determination	
D4	Celestial Radiometry	G
D5	Star Occultation Navigation	
D6	Surface Photography	G
D7	Space Object Radiometry	
D8	Radiation in Spacecraft	G
D9	Simple Navigation	
D10	Ion-Sensing Attitude Control	G

D12	Astronaut Maneuvering Unit	
D13	Astronaut Visibility	G
D16	Power-Tool Evaluation	
D17	Carbon Dioxide Reduction	G

Scientific

S1 S	Zodiacal Light Photography	
S2 M	Sea Urchin Egg Growth	G
S3 M	Frog Egg Growth	
S4 M	Radiation and Zero G on Blood	G
S5 S	Synoptic Terrain Photography	
S6 S	Synoptic Weather Photograph	G, A
S7 S	Cloud-Top Spectrometer	
S8 S	Visual Acuity	G
S9 M	Nuclear Emulsion	
S11 S	Airglow Horizon Photography	G
S12 M	Micrometeorite Collection	
S13 S	UV Astronomical Camera	G
S14	Frog Otolith Function	
S15	Zero G - Single Human Cells	A
S16	Trapped-Particles Assymetry	
S17	X-Ray Astronomy	A
S18	Micrometeorite Collection	
S19	UV Stellar Astronomy	A
S20	UV/X-Ray Solar Photography	
S26	Ion Wake Measurement	G

Technological

T1	Re-entry Communication	
T2	Manual Navigation Sightings	G
T3	In-Flight Nephelometer	

Medical

M1	Cardiovascular Conditioning	G, A
M3	In-Flight Exerciser	
M4	In-Flight Phonocardiogram	G, A
M5	Bioassays Body Fluids	
M6	Bone Demineralization	G, A

M7	Calcium Balance Study	
M8	In-Flight Sleep Analysis	G, A
M9	Human Otolith Function	
M11	Cytogenetic Blood Studies	G, A
M12	Exercise Ergometer	
M17	Thoracic Blood Flow	A
M18	Vectorcardiogram	
M19	Metabolic Rate Measurement	A
M20	Pulmonary Function	
M21	Semicircular Canal Function	A
M22	Red-Blood-Cell Survival	
M23	Lower-Body Negative Pressure	