A Review of Space Research

Space Science Board, National Academy of Sciences,
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THE NATIONAL ACADEMIES
Advisers to the Nation on Science, Engineering, and Medicine

SPACE SCIENCE BOARD

A REVIEW
OF
SPACE RESEARCH

The Report of the Summer Study
conducted under the auspices of the
Space Science Board
of the
National Academy of Sciences

at the

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FOREWORD

The Space Science Summer Study was conducted under the auspices of the Space Science Board of the National Academy of Sciences at the State University of Iowa during the period June 17 to August 10, 1962. The Study was undertaken, in response to a request and supported by a grant from the National Aeronautics and Space Administration (NASA), to examine the current national program of basic research in space and its future objectives. Most of the Study, of course, was concerned with NASA's scientific effort, including that conducted through public and private laboratories as authorized by Congress. Other federal agencies — including the National Science Foundation and the Department of Defense — conduct or support space and space-related research. Accordingly, consideration was given to the total national scientific effort.

The discussions and findings of the Summer Study are presented in the following chapters. Chapter One attempts a general survey, although the large number of specific topics examined in the Study makes an adequate summary difficult. Thus, Chapter One perhaps serves only as an annotated index to the following chapters. Appendices to Chapter One provide some description of the organization of the Study, lists of participants, contributors, and working groups, and the agenda for the Study. It should be noted that the results of the Study do not always represent the views of each individual taking part in the examination of a specific topic, and the listing of names of participants, who contributed generously in time and effort to the discussions, should not be interpreted in this way: rather, the Summer Study and the Space Science Board sought a consensus. In this sense we believe that the report is true to the spirit and substance of the deliberations.

Two general remarks are in order at this point: First, this report is addressed by the participants of the Space Science Summer Study to NASA; accordingly, in many passages it is taken for granted that references to the existing or proposed program will be understood without detailed descriptions. Other readers will find the NASA briefings on its space science program, printed as a supplement to this report, helpful in this regard. Very detailed information is also contained in other public documents; for example, in the NASA Authorization Hearings before the Senate Committee on Aeronautical and Space Sciences (June 13, 14, and 15, 1962). Second, the Summer Study enthusiastically endorsed the NASA space science program on the whole. Recommendations singling out specific segments of NASA's program for approval or improvement should be read in the light of this over-all general endorsement.

The concept of the Summer Study emerged in discussions at the tenth meeting of the Space Science Board. Preliminary planning began early in 1962. General plans were outlined by Lloyd V. Berkner (then chairman of the Board), Homer E. Newell of NASA, James A. Van Allen of the State University of Iowa, and Hugh Odishaw and R. C. Peavey of the SSB secretariat. Following discussions with the President of the Academy, then Detlev W. Bronk, Van Allen agreed to serve as chairman of the Summer Study and William W. Kellogg as vice-chairman. Further discussions, in person and by mail, were conducted by these individuals with several scores of scientists; from these discussions the agenda for the Study were arrived at. These agenda, in turn, were discussed and modified by the participants in the Study during its opening days.

The Academy and the Board are indebted to James E. Webb, Administrator of NASA, Hugh L. Dryden, Deputy Administrator of NASA, Homer E. Newell, Director of NASA's Office of Space Sciences, and to the large number of NASA officials and scientists, from both Headquarters and the field centers, for their personal contributions to the Study as well as their support of it. The Academy and the Board are also indebted to many other federal agencies — in particular the Department of Defense, the National Science Foundation, the National Bureau of Standards, the Atomic Energy Commission, and the U. S. Weather Bureau — whose representatives assisted in the program.

Grateful appreciation is acknowledged to President Virgil M. Hancher and other officials of the State University of Iowa for their gracious hospitality as hosts to the Space Science Summer Study, and to the many members of the University staff who assisted in its direction: J. R. Jordan, Director of University Relations, who assumed the primary University role in management of the Study, J. W. Dickey, K. D. Donelson, L. W. Dunlap, M. J. Finnegan, M. S. Hawkins, R. B. Mossman, Ada M. Stoflet, and G. B. Strayer; also Pearle Connell, Maxine Gibson, Beth Hawkins, Deanna Homstad, T. Jordan, Nancy Larson, L. Liedtke, Mary Jane Madsen, Helen Martin, Connie McBurney, D. Seki, and Janet Walker, as well as numerous other individuals who assisted on special occasions.

Acknowledgement is due also to the Board's secretariat, under the direction of Hugh Odishaw, for their assistance: R. C. Peavey, who served as Executive Director for the Summer Study, G. A. Derbyshire, E. R. Dyer, T. Gikas, J. Orlen, J. P. T. Pearman, J. Roshal, J. C. Truesdale, Aelese L. Carlson, Arlene K. Grittner, and Eva F. Tully.

Finally the Academy and the Board wish to record here their large debt to all the participants in the Study. Their names appear in the lists of members of the various working groups at the ends of chapters. Their hard and devoted work during the Study made this report possible. Of these participants, especial note must be taken of J. A. Van Allen and W. W. Kellogg, chairman and vice-chairman of the Summer Study; aside from their own contributions, they carried the appreciable burden of conducting the Study.

H. H. Hess
Chairman
Space Science Board
National Academy of Sciences

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Chapter One

INTRODUCTION AND SUMMARY

I. Introduction

Toward the end of 1961, both the National Aeronautics and Space Administration and the Space Science Board recognized the timeliness of an evaluation of the national research program in space and its future objectives. Some five years of expanding space research activity provided a background of experience and discovery for an inquiry into the problems and opportunities before the scientific community. The same period had also seen an appreciable development in the nation's technological capabilities for space research: new vehicles had been brought to operational status and others were rapidly being developed, techniques of spacecraft maneuverability and orientation had advanced substantially, allowable payload weights no longer imposed such severe restrictions on the design of scientific instruments, and a world-wide net of tracking and telemetry stations had been established to assure ready acquisition of scientific data. In November 1961, NASA had established the Office of Space Sciences as one of its four primary divisions. And during this time NASA had acquired a useful body of administrative and management experience in the conduct and support of research in this complex new field.

Considering these factors, the Board and NASA concluded that the time was appropriate for a review of the primary problems in space research in each major scientific area and of the policies and procedures which would shape the national effort for the years ahead. It was recognized that such a review required an extended period of deliberation by specialists in various fields. Accordingly, plans were made to conduct an eight-week study of these matters under Board direction during the summer of 1962.

Boundaries of the subject matter for discussion by the Space Science Summer Study were set early in the planning. The Study would direct its attention to the objectives of basic research in space: the status of present achievement in each scientific field, current NASA programs in these disciplines, the goals toward which each scientific program should be directed during coming years, and related administrative and policy questions. On the other hand, the Study was not to be concerned with certain other aspects of the NASA program, such as the development of new propulsion systems, applied technology (e.g., communications and navigation satellites), etc., except as developments in these programs had direct relevance to the program of basic research in space. The man-in-space program, culminating with the Apollo manned mission to the Moon, while primarily a program of technological development in its present stages, was considered in terms of its scientific potentials, because it will certainly eventually lead to a greater capacity for science in space.

The objectives of the Space Science Summer Study were outlined by Dr. Lloyd V. Berkner, then Chairman of the Space Science Board, to Summer Study participants at the opening session:

> "The first task is carefully to consider the future course of our nation's scientific program in space, and to help the government's planners to chart the way. This is a grave responsibility, and you have been chosen because you as a group represent a broad coverage of the disciplines involved in space research, and we have enough time this summer to think carefully about all facets. In particular, the backup research that underlies a comprehensive program must be fully elaborated.
>
> "The second task is similar, and involves aiding the government in its conduct of the space research program in such a way that maximum benefit will come from it. We all recognize the many opportunities opened by the space age for education, stimulation of industry and the nation's economy, research in many allied fields, collaboration and exchange of ideas with scientists in other countries, etc. These many extra benefits from our space activities can only be fostered if the program is wisely administered, and here, again, your advice has been sought and—we are assured—will be carefully heeded. If I may identify one of these aspects that deserves especial attention, it is education for space research and engineering. The burden of carrying out the education and training of new scientists and engineers rests with our universities, and it is not entirely clear how NASA, the National Science Foundation, the Department of Defense, Congress, the President, and the university community can best work together in our changing world to do this job. The training of young people is a major national responsibility. We will consider this matter carefully.
>
> "The third task is not so much a task as an inevitable consequence of our Summer Study. You, as spokesmen for the scientific and industrial community, will be privy to the problems being faced by our government administrators and scientists, and they in turn, will hear your views. The strength of our program will depend on mutual respect and understanding between the various interests, and it is most important for the university and industry people to comprehend the many broad problems and decisions that must be faced by our government people. The two-way exchange of ideas may perhaps, in the long run, be one of the most enduring benefits to come from our efforts. It is with this in mind that many of the key people from the government have agreed to spend time with us, and to be a part of our Study. Let us be careful at all times to listen to each other."

More than one hundred scientists participated, both full and part time, in the Summer Study. (For a list of participants, see Appendix II.) Many of these scientists received, for the first time, an opportunity for close association with scientific and administrative personnel of NASA and of other government agencies with space interests. This association was valuable for a number of reasons.

First, it permitted the exploration and clarification of many of NASA's policies and procedures which of necessity deal with the complex nature of space missions: e.g., the technological framework necessarily surrounding the contributions of experimenters, the scheduling and launching problems associated with expensive and complicated space rocket systems, the many factors involved in tracking and data acquisition by telemetry that call for extensive networks of stations in many parts of the world. Just as a better appreciation of these problems was attained by the scientific community, so the NASA staff became more aware of the interests and problems of experimenters throughout the nation, particularly those involved in the conduct of research at universities.

Second, this association led to a growing appreciation of the quality of NASA's scientific staff and of the general excellence of its scientific program planning and execution.

Third, and very probably most important of all, this association, because it was of sufficient duration to afford ample time for exploring attitudes and views, and because it was characterized by candor and openness, provided a basis for a satisfactory examination of space science—achievements so far, current status, and plans for the years immediately ahead. The validity of the findings of the Summer Study rests significantly upon the qualities of this association.

The material from which this report is compiled comes from the reports of more than twenty different working groups and subgroups. It thus reflects varied approaches. Preliminary reports from each group were submitted to the entire Summer Study for review and comment before being prepared in final form for inclusion in this report. Opinions, even within each working group, were not always unanimous. The report thus does not pretend to reflect every shade of opinion: it is intended to be a consensus of the Summer Study participants. At the end of each chapter will be found an appendix listing the persons who participated in the discussion of that subject; not all those whose names appear necessarily subscribe personally to every opinion recorded in that chapter.

The scientific content of the space program has been treated very differently by the different groups of specialists participating in the Summer Study. For instance, the astronomers have, on the whole rightly, assumed that every astronomer would know what scientific data of value can be deduced from a given line of experimental development, and so have not elucidated the science in detail. On the other hand, the chapter on particles and fields summarizes what we know today and what we need to know; in this case the specialists have assumed that the experimental lines of attack will be obvious.

Some of the science reviewed in this report is similar to, or in elaboration of, that in earlier reports of the Space Science Board or of NASA committees and consultants. In particular, one can refer to the following three documents as supplementary reading, in which the scientific goals of space research are described in greater detail: two reports by the Space Science Board—Science in Space (L. V. Berkner and H. Odishaw, Eds., McGraw-Hill Book Company, New York, 1961) and The Atmospheres of Mars and Venus (W. W. Kellogg and C. Sagan, Eds., NAS-NRC Publication 944, 1961)—and NASA's Long-Range Thinking Document.

Although some of the scientific discussion in this report necessarily represents a review and elaboration of earlier thinking, some new topics are given consideration. The chapter on biological researches is an example. Progress made in arriving at a policy on sterilization is reported, and also a plan for deriving the maximum scientific return from the manned exploration of space. In sterilization, enough experience has now been accumulated for setting up procedures realistic enough to be incorporated into the preparation of lunar and planetary probes, which will minimize the risk of contaminating extraterrestrial bodies. A series of recommendations for the man-in-space program sketches the scientific tasks that a man might carry out, especially in the exploration of the Moon, and outlines what we believe is a workable plan to select and train scientists as astronauts, and vice versa.

NASA's relationships with the outside academic and research world were reviewed. These relationships can be grouped under three broad headings: (1) the real significance of the long-range scientific potential of the man-in-space program, and the necessity for making this clear to the scientific public without extravagant claims; (2) NASA's responsibility to support academic institutions in the development and replenishment of the supply of educated manpower, on which NASA and other scientific and technological activities will impose a heavy drain during the foreseeable future; (3) planning the national space science program, as far as compatible with engineering and scheduling constraints, so as to encourage maximum freedom and flexibility for the individual working scientist and thus to produce an atmosphere conducive to scientific originality and initiative. (Participants referred to this last area of concern as "scientific elbowroom.") All three of these are large and complicated subjects. Although the Study reached very definite positions, these topics will require continuous review in the future.

With regard to the second of these areas—NASA's support of academic institutions as sources of scientific manpower—NASA's program of fellowships, training and facilities grants, and research contracts is a most encouraging sign that this set of problems is on its way to being solved. With regard to the other two areas—science in the man-in-space program, and maximum attainable freedom for scientific experimenters—the situation is not so clear-cut. For example, in its efforts to promote freedom and flexibility, the Study strongly recommends the allocation of blocks of payload space in a satellite or a series of satellites to scientists of demonstrated competence. The requirement for an exact description of the experiments as a condition for the assignment of payload space would be waived to allow a choice of experiments flexible enough to meet the change in circumstances that can take place between the first granting of payload space and the launch, perhaps two years later. The Study also strongly advocated the use of "small satellites," defined in this context not so much as having less than a certain size or weight, but as being suitable to carry a single experiment or single integrated set of related experiments under the direction of a single experimenter or group of experimenters. The Study also emphasized that the launching schedule should be flexible enough to allow the launching of such satellites on short notice, to take advantage of particular circumstances. It was pointed out, for example, that if such an arrangement had been in force during the summer and fall of 1962, much more could have been learned about the artificial radiation belts of Project Starfish.

Proposals like those just discussed—the block allocation of payload space and the use of satellites under the control of single experimenters—obviously presuppose a very high degree of skill in space technology on the part of the experimenter. Not many scientists can at present meet these conditions, but the Study felt that many have the potential to do so, and would develop that potential if the scientific atmosphere were more nearly in accord with their desires for freedom of action, perhaps approaching what they have in their laboratories. The Study realizes that implementation of these proposals can result in a certain amount of dislocation of the present system of budgeting, payload space assignment, and flight scheduling, and that it will take considerable time, effort, and perhaps additional funds, to accommodate the system to these ideas.

A number of other important questions were on the agenda which need further study, because the participants did not feel themselves to be sufficiently expert, or because their views were too divergent to formulate a consensus, or because they were unable to find the time to treat the subject thoroughly, or because they did not or could not secure the precise information needed. Subjects that have not been adequately treated and that require further attention are noted in the following paragraphs.

The assignment of relative priorities to scientific programs and experiments was not adequately discussed for the space science program as a whole: their relative scientific importance, the proper time sequence for optimizing the scientific return, or the equitable and efficient distribution of funds from a limited budget. Thus, it is almost impossible to decide on rational grounds many questions that cut across the entire program as long as a variety of special interests are represented. A number of recommendations were made, however, dealing with priorities in specific, narrower fields of activity. For example, astronomers would postpone putting instruments on satellites to observe extraterrestrial objects in the infrared until the potentialities of observation from the ground or from balloons are more fully exploited. Lunar specialists emphasized that certain kinds of scientific data about the Moon must be obtained relatively early, not because they are of greater scientific importance but because they are required for the proper execution of the Apollo mission. A comprehensive recommendation cutting across all other interests is that, in the early exploration of Mars, biological and biochemical studies must have the right-of-way until we find either that there is no life on Mars or that the risk of irrevocably destroying it by the introduction of terrestrial organisms is negligibly small. These examples do not by any means exhaust the list of specific priorities; nevertheless the subject as a whole is still largely open.

Questions connected with processing, distributing, and storing data were discussed. The Study is on the whole well pleased with NASA's current policy concerning the exclusive rights of an experimenter to his data for a time specifically agreed upon in each case, with flexible provisions for their later release. The mathematical problems of extracting the essential information from a vastly redundant quantity of data, technical and managerial problems of getting the data from the satellite to the experimenter, the filing and storage of data in a compact and easily accessible form, and the necessity for greater attention to these problems were all remarked; no definitive conclusions were drawn on these topics and they are recommended for further study.

Another question on which NASA asked the Summer Study for guidance concerns the scope of the ground-based researches that NASA should support; that is, is there some way to distinguish categorically between researches that are closely enough related to NASA's mission to justify its support, and those that are not? Although this question was discussed by many of the working groups in many different contexts, no one succeeded in drawing a clear dividing line. In fact, the Study concluded that no such sharp line can or should be drawn. Some of the findings are rather obvious. For instance, certain types of ground-based research, such as that relating to the development of techniques and instrumentation or the acquisition of physical data about the space environment, are indispensable just to make the equipment function properly; other ground-based researches are necessary in order to derive the maximum scientific return on a given investment (for instance, the acquisition of astronomical and physical data that are complementary to the space results or that assist in the interpretation of space results, or the theoretical and interpretative studies themselves). Several lines of research in optical, radio, and radar astronomy or radar physics of the circumterrestrial medium (which could be considered classic examples of ground-based activities) were specifically recommended for NASA support. The Study was careful to make clear, however, that these recommendations were not meant to imply an obligation by NASA to support all of classical astronomy or aeronomy, including those programs which would more normally be within the purview of the National Science Foundation, for instance. On the other hand, it was suggested that NASA should be liberal, rather than strict, in its definition of what is relevant, when considering researches commonly thought of as part of a classical ground-based discipline; that NASA should feel free to support anything of interest to NASA itself, especially researches of an exploratory nature, without relying on some strict rule for justification. In these fringe areas, it is assumed that NASA will coordinate its activities and interests with those of the other federal funding agencies.

This subject has been incompletely treated, then, chiefly because the border line between space research (defined primarily in terms of the technique used) and the classical disciplines (defined in terms of subject matter, which happen to be identical with the scientific aims of space research) must, by its very nature, be ill-defined. The ground-based ramifications of space research can be circumscribed only by drawing artificial and arbitrary distinctions.

One working group considered some of the social implications of the national space program. This group consisted of specialists representing several disciplines within the social sciences. They met for four days in consultation with several physical scientists participating in the Summer Study. Our culture, economy, and society obviously already have been modified in a number of ways by the impact of the space program. Even greater changes may be foreseen as the latent effects of the growing space program become more evident. A number of provocative questions were raised and a number of significant problems were identified which are suggested for appropriate study. The report of this working group makes clear, however, that the four days available for their sessions were not sufficient for a satisfactory review of the complex and diverse social implications of the national space program. This report is transmitted with full recognition that the majority of the Summer Study participants are not experts in the disciplines relevant for a competent evaluation of its findings.

Clearly this report should be regarded neither as a comprehensive master plan for the guidance of the space science program nor as a complete encyclopedia of all the science that is being performed or that should be performed. It is at best a compendium of ideas which we believe are sound and will be useful to NASA in the conduct of its scientific program over the next few years.

II. Summary

This section summarizes the findings and recommendations of the various working groups, which were later presented to the entire Study for their comment. For the full text and discussion accompanying each finding or recommendation, see the chapter and page number in parentheses inserted in the text at appropriate points. The reader is cautioned to consult the full text and discussion, and to regard this summary only as an index with explanatory comment. The findings range from very general to quite specific; in this summary they have been grouped under several major headings:

Flight Program: Satellites and Space Probes

Flight Program: Rockets, Balloons, and High-Flying Aircraft

Ground-Based Research Activities

Science in the Manned Space Flight Program

Administrative and Policy Matters

International Cooperation

Social Implications of Space Activities

A. Flight Program: Satellites and Space Probes

Astronomy. In broad outline we endorse the present NASA Astronomy Program (2-1).* We recommend that, as far as possible, NASA schedule launchings of orbiting astronomical observatories (OAO) so that during the next ten years at least one OAO is operational at any given time (2-10). Decisions on what specific experiments to schedule for particular future OAO's must be kept as flexible as possible within the limitations of the long lead-times imposed by both budgeting and engineering development. The existence of large space observatories of this type must not, however, be allowed to prejudice the use of small satellites, which we believe may provide more freedom and flexibility for the experimental programs of some experienced space scientists (2-5).

We believe that the next step beyond the present OAO program should be a considerably larger and more versatile space telescope (2-13). We suggest that a small study group be organized for the summer of 1963 to explore this subject, and to prepare a report delineating the relevant technical problems, together with such scientific objectives as can be foreseen.

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*Numbers in parentheses refer to the chapter and page in the body of the report; i.e., (2-1) refers to Chapter Two, page 1.

Experimental Celestial Mechanics. In connection with experimental celestial mechanics, we recommend the launching of artificial planets with periods of approximately one year, in order to determine the length of the astronomical unit (3-3); we assume that this technique is more practicable than that of tracking, say, Venus clear through a synodic period by means of radar. We advocate the establishment of artificial orbiters around the Moon and Venus (3-2) to determine the gravitational fields of those bodies. The observed motion of satellites placed near the Lagrangian triangular libration points in the Earth/Moon system would also enable us to measure the mass of the Moon precisely (3-3). The tracking of deep space probes would be improved by the further development of the Azusa-type system and the establishment of additional tracking sites (3-3).

Solar Physics. We endorse NASA's plan to launch orbiting solar observatories of the S-16 type at the rate of about two per year, both for continued explorations of the solar spectrum and other phenomena (2-8), and for monitoring those solar radiations that interact with our upper atmosphere (5-2). We recommend that NASA develop, for solar observations, a spacecraft more advanced than the present OSO, to be ready for use in three or four years (2-9); such an advanced OSO should have resolving power and pointing and stabilization capabilities sufficient to record solar features with an angular diameter of 1/10 of a second of arc (2-7).

Radio and Infrared Astronomy. Detailed experimental objectives for radio astronomy are given in Table 1 (page 2-16) and Table 2 (page 2-17). The fact that radio-astronomical techniques are better developed for frequencies corresponding to the long-wavelength edge of the radio window than they are for the short-wavelength edge suggests that observations at the long wavelengths should be emphasized in the first radio-astronomical exploration in space (2-14). At the short-wavelength end (millimeter- and submillimeter-wave) and in the infrared, techniques are still being perfected, and we believe that extensive space explorations should await the outcome of ground-based observing programs just now getting under way (2-25).

The Lunar Program, with Special Emphasis on Support for the Apollo Mission. Although we are primarily interested in data of scientific importance, many of the data about the lunar environment and surface must be obtained in order to make responsible engineering decisions in time for a successful manned lunar landing and return by 1970 or earlier. To this end, NASA should step up both the number and the information-gathering scope of unmanned flights and make maximum use of Earth-based observations (4-2). Although some of the data in the next paragraph are possibly of greater basic scientific interest, priority must be assigned to the early and reliable return of information important to the success of the Apollo mission, such as the roughness of the surface, the depth of the suspected dust layer, the physical properties of the dust layer, the distribution in size and momentum of meteorites impacting the Moon and their shrapnel-like secondaries, and the detailed topography and gravitational field of the Moon (4-3). We recommend greater emphasis on unmanned lunar impactors, soft landers, and orbiters of the Ranger and Surveyor series to obtain topographical, mechanical, chemical, seismic, meteoritic, and gravity data (4-4). Generous extensions and overlaps of the Ranger, Surveyor, and early part of the Apollo program should be scheduled, to enhance the reliability of data acquisition and to decrease the cost per launch (4-10). Satellites should be placed in orbit around the Moon to determine its gravitational field and thus to reduce the uncertainty of circumlunar trajectories and to obtain high-resolution pictures of potential landing areas (3-2).

Other experiments of equal, if not greater, scientific importance (but of less importance to the success of the Apollo mission) include: the return of a sample or samples of lunar material, both from the surface itself and underground; measurements of the magnetic field of the Moon, if any; tracking capsules in a variety of orbits around the Moon, in order to obtain reliable moments of inertia and thus an idea of the mass distribution within the Moon (4-7); and the erection of a three-axis telescope system on the surface of the Moon in order to determine its physical librations (3-2). Satellites in the neighborhood of the triangular libration points (mentioned above) or an already scheduled interplanetary probe directed through the neighborhood of one of these points, if equipped with appropriate detectors, would tell us something of the nature, distribution, and origin of the debris now suspected to be accumulating there (4-7). In the collection of lunar and planetary samples, care should be taken to conserve carbonaceous compounds on account of their biochemical interest (9-10).

Planetary Program. On pages 4-9 and 9-6 ff. will be found a number of remarks on the planetary program too detailed to elaborate on here. Information about the surfaces and environs of the planets, specifically through greater emphasis and extension of the Mariner program, should be obtained, not only on account of their intrinsic scientific interest, but also in anticipation of future manned visits (4-7). In particular, manned exploration of Mars represents an extraordinary scientific opportunity; preliminary planning, especially of long lead-time items, should begin at an early date (11-5). Other comments dealing with biological studies on Mars and other extraterrestrial bodies will be found below, under Sterilization and Biology.

The acquisition of chemical information about the atmosphere and surface composition of Mars should receive high priority (9-8). Of special biological interest are: O_2, CO_2, CO, CH_4, H_2S, NH_3, H_2O (including water of hydration), and organic compounds in general. Physical parameters such as temperature, pressure, and the flux of ultraviolet and ionizing radiation are of great interest, not only intrinsically, but also biologically. The collection (and enrichment) of samples of the Martian surface for microscopic examination and the transmission of vidicon images (with or without data reduction) are areas that warrant accelerated effort (9-8). As a relatively simple device for the detection of macroscopic life forms, a microphone should be included on vehicles designed to land on Mars (9-10).

We believe that NASA can pursue, at an early date, its current plans to investigate planetary atmospheres (5-13). Planetary observations and experiments of the kinds listed on pages 58 and 59 of "The Atmospheres of Mars and Venus," NAS-NRC Publ. No. 944, 1961, should be carried out (5-11).

Planetary-Interplanetary Programs. NASA should consider the broad applications of bistatic radar astronomy for the study of planetary surfaces and atmospheres and of the interplanetary medium, and of the supporting role of monostatic radar astronomy, using the same ground-based facilities (5-15, 6-4). The first steps to develop and erect more powerful ground-based equipment for such systems should be taken as soon as possible. In due course instrumentation of space probes for use in conjunction with the ground-based research radars should be funded and developed (5-15).

Within the foreseeable future, all lunar, planetary, and interplanetary probes should carry appropriate magnetometers whenever possible. Only by widespread and frequent measurements can we expect to unravel the complicated temporal and spatial variations in the interplanetary magnetic field that Pioneer V revealed (7-9). The biological exploration of interplanetary space should also be seriously attempted (9-10).

Sterilization. Sterilization of spacecraft which might intentionally or accidentally land on the Moon or a planet continues to be a problem. The present NASA policy of requiring sterilization of such spacecraft is heartily endorsed (9-12). Efforts to develop adequate sterilization procedures (such as those at JPL) should be continued and intensified (9-12).

The chief results accomplished by the lunar spacecraft sterilization program up to now seem to have been development of capability and acquisition of experience in the procedures for spacecraft sterilization (10-4). With regard to the lunar program, it appears that the time schedule imposed by nonscientific considerations will probably not permit the full development and incorporation of certain advanced sterilization procedures just now being developed (10-5). There is some evidence that certain sterilization procedures reduce the reliability of spacecraft systems and some of their components; it has not yet been possible to carry out quantitative measurements of the degree of degradation, so the matter is still largely a topic for speculation (10-5). Until now, waivers have been issued for critical components which could not be sterilized internally without serious degradation of their performance; such waivers should continue to be required in order not to delay the over-all program. However, this recommendation should not be interpreted as encouraging a general relaxation of tolerances. The lunar probe decontamination program should continue in force in order to keep contamination of the Moon as low as possible and thus to interfere as little as possible with future biological and biochemical studies (10-5). Meanwhile, we find that the JPL spacecraft sterilization program has resulted in substantial technological advances: problem areas and critical components have been identified; in some cases, intensified efforts toward redesign have produced components that can now withstand specified sterilization treatments, thus removing such components from the critical list (10-5).

We believe that biological and biochemical experiments should be carried out with the *first* probe, while pollution from the Earth is still minimal (10-7). We therefore recommend that, among the samples of Moon stuff which are first collected, some be sealed off under sterile conditions and retained as minimally disturbed and uncontaminated samples of lunar material for future use; it is quite possible that, in the light of information and techniques which we do not how have, we might wish to carry out experiments on uncontaminated lunar material only to find we do not have any and have no hope of getting any (10-7). In particular, uncontaminated samples should be collected during early unmanned missions, since the risk of contamination during manned flight to the Moon (or the planets) is relatively great (10-7). Uncontaminated samples such as we are suggesting need not necessarily be returned to Earth immediately, but may be left at the collection site to be picked up and returned even many years later (10-7).

Planning for manned landings on the Moon and planets must be based on the assumption that sterility precautions will still be required during the phase of manned explorations (10-7). Further information on the lunar surface and environment is needed in order to plan sound sterilization procedures prior to the first lunar landings (11-3). Whatever the circumstances, jettisoning of human wastes under conditions which could contaminate the lunar surface, perhaps even before the first collection of lunar material, is inconsistent with a responsible attitude toward exobiological objectives (10-6). In order to reduce the risk that man will contaminate lunar and planetary environments by his presence, work on the development of space suits which will function as more effective biological barriers than those presently available should be intensified (10-7).

Since conditions on Mars are much more favorable for life than on the Moon, it follows that even greater precautions must be taken against the contamination of that body. One of the most serious impediments to the maintenance of sterility—namely, last-minute adjustments before launch, which have often been required heretofore, but which, if performed, require that the spacecraft be sterilized again—can probably be avoided in the case of vehicles landing on the planets. Since such spacecraft will be designed to operate for many months without maintenance, it should be possible to make them work a few days longer without human intervention—that is, from the time of last sterilization instead of the time of launch (10-6). We should also be cautious about contaminating Venus—or any of the other planets, for that matter—until we are certain that no life is present, or at least that physical conditions are everywhere so hostile that no terrestrial forms of life could survive. Furthermore, besides being careful not to transport any viable organisms to extraterrestrial bodies, we should avoid introducing nonliving organic compounds which might complicate later cosmobiological research, even though that research might be limited to a study of the planet's organic chemistry. Such complications will be materially lessened by maintaining a complete inventory of the organic constituents of each spacecraft in a form for convenient reference, in accordance with the directive of 13 October 1959 (Appendix IV of Chapter Ten, 10-6).

As sterilization techniques are perfected and become more routine, the concept of "certified sterility" for spacecraft and components should be developed and standardized, along lines similar to present-day medical practice. The present direct cost of the sterilization of lunar probes is hard to estimate, but may be as high as five or ten per cent of the total cost; as techniques are perfected, however, this "overhead" can be written off as a very small fraction of the future cost of missions to the Moon and planets (10-6).

The beginnings of a good working relationship seem to have developed between biologists and spacecraft engineers cognizant of the sterilization program. This is highly commendable, for the continued improvement of the program depends very much on the maintenance of such rapport (10-4).

The problem of keeping extraterrestrial bodies from being contaminated by terrestrial organisms is obviously international in scope (10-7). International cooperation in the field of space probe sterilization is highly desirable (9-13, 10-7, and 15-14). We must make arrangements for the exchange of information about sterilization technologies if possible. If the exchange of information is not completely possible, even the unilateral transmission of information would be helpful (9-13). In particular, the planet Mars is at present our most important biological objective.

We urge that it be a matter of national policy that Mars becomes a "biological preserve" (10-9), and that steps be taken to obtain international agreements to this end (15-13).

Particles and Fields. The NASA space science program in particles and fields has on the whole already yielded substantial results; we endorse the existing plans for the continuation of this work, which we assume are going forward (7-1). There are, however, many vital experiments which need a geostationary (24-hour period) satellite, and we hope that one can be scheduled at an early date, without waiting for the development of the Centaur booster (7-1). Other specific experiments require a satellite in an accurately circular orbit at an altitude of about 300 km; for instance: the further investigation of the recently discovered low-energy electrons at low altitudes over the Central Pacific (7-20), comprehensive observations of the aurorae all made at the same altitude, and measurements of the neutral atmosphere (7-1, 7-19, 5-5). Such a satellite should be gravitationally oriented, but its lifetime need not be very long.

A much more complete study of the spatial and spectral variations of particle fluxes is needed than we now possess, in order to develop a conclusive theoretical understanding of the essential physical processes involved (7-17). The controlled injection by artificial means of a known number of particles with known energy spectra and at known positions in space is a technique of considerable technical value in clarifying the dynamics of trapped electrons as a function of L value, geomagnetic perturbations, and the nature of the outer part of the geomagnetic field. International cooperation in the development of ingenious injection mechanisms, experimental methods, and the analysis of results could contribute significantly to this end. An adequate system of satellites (at least one in a high-inclination orbit) is required for observing the effects (7-18 and -19). It might be necessary, however, to delay such studies until more nearly definitive measurements of the natural radiation belts can be carried out.

Magnetic surveys could be conducted with considerably greater precision with more precise tracking and vehicle orientation; for missions subsequent to POGO, the use of the Transit Doppler system for tracking and the OAO orientation system should be considered (7-3). The structure and boundaries of the geomagnetic cavity should be explored with a variety of plasma probes and magnetometers (7-8). Vector magnetometers are desirable. For monitoring the location of the boundary, only moderate sensitivity (a resolution of 10 gammas) is adequate; but the investigation of the irregular magnetic field just beyond the boundary, and of the low-intensity interplanetary fields still farther out, call for instruments capable of detecting 1-gamma fluctuations (7-8).

Aurorae. Our understanding of the aurora and related mechanisms is still very poor. The following questions still await a theoretical explanation: How are the rapid variations in color, intensity, and structure connected with electron bombardment or other causal mechanisms? How do the bombarding electrons acquire their energy, and how are they precipitated into the atmosphere? How can the electron fluxes be as extended in longitude and as compressed in latitude as is sometimes observed, and how do the fluxes achieve such twisted shapes? What controls the gross variation in latitude in the frequency of occurrence? How is the frequency controlled in diurnal and solar cycle effects? It had been thought earlier that the trapped radiation might be the direct source of particles producing the aurora, but this now appears very unlikely and the answers will probably not be forthcoming until more observational data are acquired by satellites passing through the source regions (7-22).

Measurements of the ultraviolet luminescence made from satellites in the auroral zone would also be exceedingly helpful; such measurements should be made from the same polar-orbiting satellite that is used for detecting the particle fluxes associated with aurorae (5-5).

Aeronomy. Measurements of the concentration of various atmospheric constituents (with the highest priority being assigned to neutral and ionized helium and atomic hydrogen) and the ionic and electronic temperature are needed. The recommended method is the molecular-beam technique, using an oriented satellite in a circular polar orbit a few hundred kilometers high, similar to or identical with that needed for measurements of the magnetic field, trapped particles, and aurorae (see above; also 5-10).

The Earth's Gravitational Field; Time. An Earth-satellite program designed specifically to improve our knowledge of the Earth's gravitational field and hence its detailed mass density distribution should be considered (3-2). Representatives of the fields of geodesy and celestial mechanics should participate in the planning of this program and the interpretation of these observational data, since their interests overlap. We also endorse experiments concerned with the investigation of the nature of time, such as those outlined in the NASA Long-Range Sciences Thinking Document, which require a satellite program similar in many respects to that needed for gravitational field studies.

Biology. The search for extraterrestrial life has an obvious and compelling fascination for peoples of all nations. In a few short years this topic—at one time merely science fiction—has been lifted from the category of escape reading and whimsical speculation and now stands as a serious objective of the international space race. If life does indeed exist on another planet and we or the Russians find it, that discovery will have an enormous and lasting impact on people of every race and culture the world over, whether they are scientists or not. One does not need to be a biologist to understand in great measure the significance of finding extraterrestrial life. As a national objective it is indisputably nonmilitary. Whether it can match in importance such achievements as the discovery of belts of trapped radiation, the more precise determination of the astronomical unit or the decision as to whether the Moon has a liquid core may become the meat of philosophical arguments, but for 99 per cent of the world's population such questions have little meaning. Of all the discoveries which have come from or can now be anticipated from man's efforts in space science none more easily captures the imagination or is more likely to focus interest and acclaim than the empirical proof that there is in this universe a biota other than our own. On solid scientific grounds, on the basis of popular appeal, and in the interests of our prestige as a peace-loving nation capable of great scientific enterprise, exobiology's goal of finding and exploring extraterrestrial life should be acclaimed as the top-priority scientific goal of our space program (9-6).

Detailed suggestions for experiments dealing with the search for extraterrestrial life, fossil evidence therefor, and associated organic compounds will be found in Chapter Nine; these should be energetically pursued.

In the field of environmental biology, orbiting space vehicles uniquely provide the possibility of studying the long-term biological effects of a zero-gravity state (9-15). Besides having major responsibility in this area of environmental biology, we believe that NASA should concurrently support ground-based studies on the biological effects of gravity fields above normal. Studies of biological rhythms in plants and animals (including man), both on the ground and in space where it appears that the last vestiges of periodicities impressed by the terrestrial environment will be removed, should also form an important part of NASA's program in environmental biology (9-17).

There is some evidence that biological specimens react differently to a given environmental factor in the two cases: when that factor occurs in conjunction with other variable factors, and when it is isolated. Carefully designed experiments, with suitable controls, should be conducted to explore the effect of more than one environmental stimulus acting concurrently (9-19).

B. Flight Program: Rockets, Balloons, and High-Flying Aircraft

In general, we agree that the sounding-rocket program will continue to be an essential part of the over-all space program; it will be indispensable for exploratory measurements, for observations of brief duration, for measurements requiring physical recovery of data, for observations below the altitudes for which satellites, are suitable, and for training in space techniques (2-3).

Astronomy. The astronomical sounding-rocket program should continue to receive full support. We encourage NASA's efforts to make available at the earliest possible time the inertially guided Aerobee with fine pointing, not only at selected stars, but also at the Sun under the control of a suitable optical sensor (2-3).

Airglow. Observations of the distribution of the airglow from above should be made on appropriate high-level rocket flights, particularly at the wavelengths 6300-6364A and 3911A (5-5).

Upper Atmosphere. Measurements of the upper atmosphere ionic temperature, electron temperature, and atomic and molecular constituents, both neutral and ionized, should be carried out with rockets in the range of heights from 85 km to about 200 km (5-7). In contrast to the satellite measurements at greater heights mentioned above, in this region first priority should be given to measurements of atomic and molecular oxygen and nitrogen. Both the mass spectrometer and methods based on the optical absorption of sunlight should be used. We believe that NASA should anticipate the requirement, in the long run, for aeronomical rockets that can be fired to a height of around 200 km from sites with modest range facilities (5-16). This requirement should be borne in mind during the more immediate development of meteorological rockets (see below) and of sites from which to fire them. Such sites, if suitably distributed, might also contribute to the solution of a number of physical and organizational problems, including relations with regional groups of universities.

Meteorology. We have reviewed an excellent NASA survey of meteorological rockets and have gathered information from other sources on the state of the art in small-rocket systems (8-2). We note especially the following features of the existing situation (8-3 and 4). The operating ceiling imposed by both rockets (Arcas and Loki II) and payloads is about 60 km. Rockets with their payloads are only 50-75 per cent reliable, although statistics on this point are hard to evaluate. The most common method of observing high-altitude wind, i.e., with falling chaff, does not give satisfactory results at sites where no big radars exist. The high-altitude vapor trail method has been used only at twilight under clear weather conditions, and the grenade method requires very accurate tracking, an array of microphones on the ground, and complex data reduction. Winds have occasionally also been measured from stabilized rockets to heights over 100 km by observing the differential pressure on the sides of the rocket, which gives the transverse component of the relative wind on the rocket. The direct measurement of temperature with an exposed sensing element begins to break down above 45 or 55 km because of radiation effects, so that above these heights a temperature derived from the pressure and density, both of which can be reliably measured, is probably more trustworthy.

Although the current meteorological rocket systems, together with a network of well-equipped firing ranges, has given meteorology its first chance to enter the region above 30 km on a synoptic basis, the situation is not yet satisfactory (8-3 and 4). We therefore recommend that NASA and the Department of Defense pursue a joint development program to provide a first-generation meteorological rocket system, capable of reliably probing the atmosphere between 30 and 60 km, to measure wind, and pressure or temperature or density versus height. Such a system should be self-contained, capable of operating without the help of a large radar. Development programs already under way at the Department of Defense, given adequate financial support and direction, can probably produce such a system in time for the IQSY.

We further recommend that NASA initiate a program to develop a second-generation meteorological rocket system capable of reliably probing the atmosphere between 30 and 100 km or even higher, to measure, in addition to the parameters listed above, the electron density at heights above 60 km. This system should also dispense with the support of large radars, if possible. For widest and most versatile use, the new rocket should cost less than $2000, be able to carry 10 kg to over 100 km, be stable in flight, and have a 3 σ dispersion on impact of less than 10 km. Although the techniques to make the individual measurements listed above are probably at hand, a new and imaginative approach to the design of sensors will probably be necessary.

Procedures for the publication and storing of meteorological data have been worked out for the North American Rocket Network; but as other countries begin to use meteorological rockets and data, exchange agreements will become important. We suggest that a group of experts be convened by the Joint Meteorological Rocket Network Steering Committee to study the best methods of modifying or extending present procedures for data storage and publications for the anticipated international exchange through WMO.

Biological Sampling. We believe that NASA should undertake the responsibility for exploring the vertical extent of the terrestrial biosphere by collecting biological samples with high-altitude balloon flights and rockets (9-11). Besides defining the boundaries of our biosphere, such flights could prove out sampling techniques to be used on space probes later. It would also appear possible to put biological sampling devices aboard the X-15.

Other Uses of the X-15 and Balloons. The X-15, which can go to heights of more than 80 km, also offers to astronomers the opportunity for certain types of observations, especially those of an exploratory nature (2-2). One field of investigation which astronomers feel has not yet been adequately explored from the ground or subsatellite heights is infrared astronomy (2-25). For example, plans to secure planetary observations in the infrared from space vehicles should await the results of forthcoming balloon-borne experiments (except in those cases where planetary probes offer the obvious advantage of nearness to a planet).

C. Ground-Based Research Activities

One of the important questions presented to the Summer Study might be described somewhat as follows: There are many classes of ground-based research that are closely related to the flight program or in some obvious way back it up, which everyone agrees should be supported and encouraged by NASA. At the other extreme, there are ground-based researches that obviously have nothing to do with the space science program. In the spectrum of ground-based researches, from those that are obviously and vitally relevant, which NASA should support, to those that are obviously irrelevant, which NASA should not support, is there an easily identifiable boundary where NASA should draw the line? Although this question was discussed by several of the working groups and their findings were presented to the entire Study, no one succeeded in drawing such a boundary line; in fact the consensus was that no sharp line ought to be drawn.

The responsibility of other federal agencies to foster programs of research in support of the primary program of space research was also considered. Clearly, the National Science Foundation should assume an active role in this area. Many other federal agencies are supporting scientific programs which will contribute to the national space research effort. The Departments of the Air Force and the Navy are conducting operational programs, including research in space in support of agency missions. Various elements of the programs supported by these other federal agencies were considered by the Summer Study and there was substantial evidence of the need for closer integration of these efforts with those of NASA.

In the preparation of this report, various attempts were made to list a number of classes of ground-based researches deserving support, ranging from "indispensable" (e.g., development of techniques and instrumentation, or the acquisition of physical data about the space environment), through "necessary for the maximum scientific return on a given investment" (e.g., the acquisition of astronomical or physical laboratory data that assist in the interpretation of space results, or theoretical and interpretative studies themselves), to "exploratory studies" (e.g., in fields which may or may not turn out to be important in future space researches, but about which so little is now known that no one can be sure of their eventual value to the science program as conducted with space vehicles). Among the ground-based programs specifically recommended for NASA support are several of a classical nature, which probably would have been carried out even if the space program did not exist; for instance, the solar flare patrol (2-29), radar soundings of the magnetosphere (5-10); or studies in organic chemistry throwing light on the possible origins of living matter. This was not meant to imply that all ground-based astronomy or radar physics or organic chemistry should automatically receive NASA support. All concerned are agreed on the studies that are essential to the success of the space science program; we assume that NASA will support them automatically. In researches of an exploratory nature, whose relevance is somewhat less direct, NASA should have the freedom to examine each proposal in the light of several rather flexible grounds—on the merits of the proposal itself; in the light of NASA's interest or possible future interest in the result; whether or not another funding agency will take it in hand; or to follow up interesting leads unearthed in NASA's earlier programs, even though the follow-up may take place entirely on the ground. (See, for example, pp. 2-24 ff. for an elaboration of these ideas.)

There follow a number of specific recommendations on ground-based research adopted by the Summer Study.

Development of Techniques and Instrumentation. Many technological problems still remain to be solved to make the most effective use of future sophisticated spacecraft. To draw an example from the field of astronomy, we call attention to the technological research at the Kitt Peak National Observatory, looking to future orbiting astronomical observatories, on metal mirrors, space optical systems, remotely operated telescopes, and stabilization devices. In astronomy, the following techniques are among those requiring work: more sensitive image tubes with 1000-line resolution, ultraviolet reflection optics with good reflectivities, X-ray optics, closed-cycle cryostats, large radio antennas that can be erected in space, millimeter-wave antennas, and space-borne radiometers (2-15, 2-23). Still others remain to be identified. Furthermore, the future growth of space radio astronomy will depend on the prior growth of ground-based radio astronomy, a fact which we believe justifies NASA participation in its support, even though chief responsibility may lie with other agencies (2-16).

Technological developments for biological studies that are needed, or to whose possibilities NASA should at least remain alert, include: electron-optical techniques adaptable to the space environment (9-11), laser techniques to provide planetary data (9-7), life-detection equipment based on the assimilation or the metabolic generation of CO_2 or on enzyme activity (9-9), and the collecting and storing of lunar and planetary samples under aseptic conditions (9-9). We endorse NASA's already existing efforts along these lines. In fact, NASA should support projects of high quality in biotechnology, even though their application may be somewhat indirect (9-20). Some of the foregoing would be applicable equally well to the study of inanimate matter, e.g., on a microscopic scale.

With regard to techniques in support of manned space flight, the Summer Study noted the following. Regenerative life-support systems, including bioregenerative systems based on photosynthesis, should be developed (9-9). Research efforts in chemical protection against radiation should receive low priority, at least for the time being, unless other facts come to light.

The various ways in which a man can fit into an extended sensing and control loop reaching into space came under discussion: he can remain on the Earth, he can be sent to the far end of the loop (say, on the Moon), or he might be integrated into the system at various intermediate points. This subject is obviously very complex, and requires detailed study by experienced specialists, including for instance people from the AEC who have experience with complicated slave automata (11-8). Such a study should include consideration of various classes of versatile remotely controlled automata (dubbed "telepuppets" by the group, following a suggestion by Whipple) in order to extend the range of man's capabilities in exploring, e.g., the surface of the Moon, or regions likely to be inaccessible for the indefinite future because they are too hostile. One sensing and control loop which need not wait for technological developments, and which can therefore be explored immediately, would consist of a trained observer in the field connected by a two-way communication system to a scientist at a base, with the observer reporting to the scientist and following his directions as far as he is able (11-4).

In connection with sterilization, besides urging the continued study of techniques along the several lines now in progress (e.g., treatment of spacecraft and their components with ethylene oxide), we wish to note the following specific points. "Glove-box" procedures such as those used in handling sterile biological specimens are applicable to the handling of components, e.g., in the assembly of spacecraft, and should be exploited as far as possible (10-4). As noted above, space suits that will function as more effective biological barriers than do those presently available should be developed, in order to prevent man as far as possible from contaminating the extraterrestrial bodies that he visits. Finally, appropriate quarantine procedures should be established for equipment and personnel returning to Earth in order to avoid contamination of our environment by hostile organisms, in case this turns out to be a danger (9-13).

Data on the Space Environment and Its Effects. Astronomical ground-based activities and laboratory work in direct support of NASA scientific missions (2-23 and 24) include solar patrols; observations and studies of Moon (described in slightly more detail in the next paragraphs); the optical, infrared, and radio study of planetary atmospheres and surfaces; studies of meteors, and the systematic collection and analysis of fallen meteorites (see also 9-11); and so on. Much information along these lines can be obtained relatively soon and economically from Earth-based observations and experiments (4-5). We strongly approve NASA's current support of such work, and recommend that this policy be continued and expanded (2-24).

Astronomers believe that infrared techniques have not yet been sufficiently exploited on the ground.

In regard to the Moon in particular, we note that essential contributions to the knowledge that we should like to have on scientific grounds, but which in any case we must have soon to ensure the success of the Apollo mission (4-10), can be obtained from: (i) the exploitation and further development of optical, infrared, radio, radar, and laser techniques to observe the Moon; (ii) intensified mapping and study of the lunar topography, as exemplified by the Air Force LAC lunar map series and the U. S. Geological Survey photogeologic analyses; (iii) laboratory studies simulating lunar conditions, like the investigation of the behavior of fine dust in a high vacuum as pioneered by Gold (4-5 and 6).

We believe that further work is required on the physiological and psychological effects of the space environment on man for the Apollo program (11-3), making full use of ground studies as well as the results of Gemini and early Apollo orbiters to strengthen this work.

Information Needed to Interpret Space Results. Information needed to derive the maximum scientific results from data acquired in spaceborne missions falls naturally into several classes (2-22): (i) synoptic or monitoring data describing physical conditions while the space data were being acquired; (ii) data complementary to the space data, for instance, in another part of the spectrum; (iii) physical data on the properties of matter acquired in the laboratory, etc. The specific recommendations summarized in this section fall into one of the foregoing groups.

Ground-based activities in astronomy complementary to the space program should receive NASA support almost as a matter of course, although the detailed decisions on priorities for funding would naturally rest on the degree of urgency of a given investigation for the more immediate space program (2-22). In solar physics, we recommend specifically that NASA assist in the improvement of the international photographic flare patrol by supporting efforts to reduce the gaps in the present coverage and to improve the quality of the existing patrol stations and their output (2-29). Proposals contributing to a broad study of the mechanism underlying solar flares should also be encouraged (2-29). We recommend that NASA support proposals to conduct laboratory studies of astrophysical interest, similar in content to those being carried out by the National Bureau of Standards (2-29).

The numerical value of the constants of the solar system (e.g., the masses and mass ratios of the solar system bodies, the length of the astronomical unit, etc.) are very important for the calculation of trajectories. Furthermore, the accuracy with which these constants can be determined from observations is ultimately limited by the accuracy of the catalogue positions of the stars that serve as reference points in the sky. There is a clear and present need to improve these stellar positions, both for individual stars and for separate catalogues with respect to a common system of reference. This is especially true of the southern celestial hemisphere (3-4).

Either existing balloon telescopes should be flown more frequently to observe the planets, or duplicates should be built so that more flights can be made (5-14). NASA should support ground-based optical, radio, and radar observations of the planets at or near those institutions where active research in planetary science is going on, especially if the geographical location of the observing site would contribute to a world-wide or international cooperative scheme for continuous observational coverage of the planets. Recommendations like the foregoing should not, however, be construed as urging NASA to support all kinds of ground-based astronomical studies, without regard to their content and potential.

Because of the relevance of data on the magnetosphere to NASA's main mission, we recommend that NASA support (by transfer of funds to other agencies, if appropriate) the development of magnetospheric radio sounding techniques and the installation of additional sounders at suitable locations (5-10). As before, NASA should not feel that this specific action constitutes a recommendation to support all ground-based sounding activities.

We endorse support by NASA of high-quality studies related to the origins of life which will obviously become most important for the interpretation of the origins of extraterrestrial living forms, in case these are discovered (9-5). Even if no such forms are discovered, the results of studies in abiogenesis will nevertheless be of immense scientific value, so that the support will by no means have been wasted.

Theoretical, Mathematical, Exploratory, and Interpretative Studies. In meteorological studies, the full potential of satellite observing stations may not be realized until new ways of observing the atmosphere have been developed; therefore imaginative new approaches to this question should be encouraged (8-6). For instance, passive and active methods for use in all regions of the electromagnetic spectrum, and ways to relate these novel measurements to the behavior of the atmosphere, should be considered.

There is an enormous redundancy in meteorological satellite observations, and methods for automatically extracting the most useful information from these data and transmitting it to the ground in simplified form should be encouraged (8-6). Other fields of science are beset by this same problem.

Analytical researches in celestial mechanics should be emphasized and supported, not only because of their general interest for science and mathematics but also because of their importance for practical engineering decisions connected with flight (3-4).

In the field of biology, we believe that NASA should encourage new theoretical approaches to biological problems, and should consider establishing a group of theoreticians in space biology, at either a university or a NASA center, like Ames (9-22).

Bistatic Radar Astronomy. In contrast to monostatic (i.e., conventional) radar, a bistatic radar system has a transmitter and receiver at different locations. Such a system, with large transmitting equipment on the Earth and small receivers in space probes, would allow valuable studies to be made of: (1) the surface of the Moon; (2) any residual atmosphere or ionosphere of the Moon; (3) the surfaces of the planets; (4) the atmospheres and ionospheres of the planets; (5) the solar corona; and (6) the interplanetary plasma. As will appear below and as indicated in much greater detail in Chapter Six, such a system is an example *par excellence* of a large and special enterprise with strong ground-based applications which NASA should develop and support.

The characteristics (intensity, polarization, etc.) of the reflected signal as a function of the angle of incidence yield important information about the properties of reflecting surfaces or refracting media. For reflection studies or occultation studies (which are important for plasmas) the angle of incidence should range from perpendicular to glancing, and the placement of the receiver corresponding to these angles can be provided only by a bistatic radar system. Reception at the receiver of both the directly transmitted ray and the ray reflected from the body under study affords automatic calibration. Naturally, the measurement of the reflection characteristics from both solid surfaces and plasmas should be performed for a wide range of frequencies. For the plasma studies one should use as low a frequency as possible because our present estimates of the plasma density lead us to expect low plasma frequencies.

Bistatic radar astronomy involves an unusually flexible approach to space science. Not only can the ground equipment be used for many different unique probe missions, but also when no vehicles are in flight the ground equipment can be used alone as a monostatic radar to obtain strong echoes from the Sun, the Moon, and the planets, and as a steerable magnetospheric radio sounder. The system would thus be active at all times as a research instrument and as a means of involving graduate students in space science.

Initial radar studies of the nearest planets at nearest approach can be conducted with existing ground equipment, but more powerful equipment is required for more detailed measurements and for greater distances. The need for a more powerful transmitter and a large antenna with a steerable beam is particularly great at the low frequencies required for space plasma studies. It is difficult to obtain high sensitivity at low frequencies, because the competing cosmic noise is greater and antenna gain per unit aperture is less at low frequencies. Such equipment would not be cheap, but would nevertheless not require the expensive precision involved in higher-frequency installations.

Possibly there has not been a general appreciation of the broad range of applications of this technique for research in space sciences, and of the importance of supporting research which could be conducted using the same ground equipment as a monostatic radar. We therefore recommend that: (1) NASA include in its thinking and planning the broad applications of bistatic radar astronomy for space science research, and the supporting role of monostatic radar astronomy using the same ground-based facilities; (2) NASA take the initial steps aimed at developing and building more powerful ground-based equipment for these applications as soon as possible; (3) NASA in due course fund instrumentation of space probes for use in conjunction with the ground-based radar research equipment.

D. Science in the Manned Spaceflight Program

The Apollo program was acknowledged as an integral part of the NASA effort, based upon the President's decision that this undertaking be established as a national goal. The objectives of the Apollo mission and its interrelationships with fundamental scientific research were discussed on many occasions during the Space Science Summer Study, and a number of working groups gave consideration to the opportunities for carrying out scientific research which the Apollo program would afford. These discussions also formed the basis for a more detailed consideration of the role of man in space exploration by a working group convened specifically to consider this topic.

Discussions of this topic revealed that there is considerable confusion about the Apollo mission and its proper justifications. Because of this confusion it is pertinent to present here the conclusions of the Summer Study on this subject.

In the first place, the Apollo program is related to man's innate drive to explore unknown regions, to national prestige, and to national security. These elements are of concern not only to scientists but also to all other segments of our society.

In the second place, there are important scientific objectives in the Apollo program, and it is in terms of science and scientific opportunities that the program appears to have been most widely misunderstood. It is in this context that the Summer Study conclusions concerning the Apollo program deserve elaboration.

Put in the simplest terms, the objective of the Apollo program is to place a man on the Moon and return him safely. Thus the current program is primarily a technological and engineering effort, and this fact ought to be generally recognized. When it becomes clear, however, that these ends will be achieved, a strong scientific validity immediately follows. By his presence, man will contribute critical capacities for scientific judgment, discrimination, and analysis (especially of a total situation) which can never be accomplished by his instruments, however complex and sophisticated they become. Hence, manned exploration of space <u>is</u> science in space, for man will go with the instruments that he has designed to supplement his capacities—to observe what is there, and to measure and describe phenomena in terms that his scientific colleagues will clearly understand. A scientifically trained and oriented man will be essential for this purpose.

In this context the Apollo program indeed acquires scientific validity. Scientists should recognize that Apollo is the first phase in a continuing engineering enterprise that will ultimately enable man to move about in space and provide him with the capacity for conducting his scientific investigations. It must always be remembered that as the earlier phases of the Apollo program proceed, engineering for the craft and for man will always assume the highest priority and the engineers must be protected in their ability to do their jobs. As the engineering tasks are accomplished, however, scientific investigations and missions will also be phased into the program; and, as flexibility and sophistication are achieved, scientific investigations will become the primary goals.

Appreciation of these concepts is of critical importance to the acceptance of the current Apollo program by scientists throughout the country. The proper exposition of these concepts by the federal government should go far toward allaying misunderstandings of the Apollo program which are currently prevalent among many members of the scientific community.

<u>Lunar Exploration</u>. The most important <u>scientific tasks</u> foreseeable for manned lunar explorations are: (i) observations of natural phenomena, including micro- and macro-structure and composition; (ii) collection of representative samples; and (iii) emplacement of monitoring equipment. In this assessment, we agree with the findings of the NASA committee which examined these same questions (11-4).

We believe that it is extremely important for at least one crew member of each Apollo lunar mission to possess the maximum scientific ability and training consistent with his required contribution to spacecraft operations (11-4). In this report we have designated such a man a "scientist-astronaut." His scientific activities should be continued throughout his period of training in spacecraft operations. Such a man should participate in the earliest possible lunar missions. Furthermore, in the lunar orbital rendezvous mode now planned for the Apollo mission, the maximum scientific return will be achieved only if the scientist himself lands on the Moon.

A number of investigations, primarily astronomical, have been suggested which make use of the Moon as a space platform (Chapters II, XI). In situ experiments, investigations, and explorations of the Moon itself, which by their nature require man's presence on the Moon, should have priority before those merely making use of the Moon as a space platform (2-19). Facilities for investigations of the latter type should, however, be included, if possible, in a manned lunar laboratory to be built after the initial explorations. We definitely foresee a need for such a laboratory (11-5).

Earth-Orbiting Manned Laboratories. There was some diversity of opinion on the scientific need for a manned orbiting laboratory beyond Gemini (see below). Such a laboratory will be useful primarily for biological studies, but the time phasing and form for such a laboratory require further detailed study (11-5).

The means whereby a man can reach orbiting satellites to repair, maintain, or modify them for scientific experimental purposes, as well as for mission support, should be explored (11-5). Such a capability is of particular importance for complicated and expensive permanent astronomical facilities such as astronomers are now contemplating (2-21). Astronomers believe that a man is not required to operate an orbiting observatory—that, in fact, his presence would be positively detrimental; but they welcome the prospect that man can intervene in the maintenance or modification of such equipment. This capability will naturally influence the design of such a system very strongly. With such possibilities in mind, we urge NASA to make possible, in the near future, personal contacts between interested astronomers and some of the astronauts who have been in space (2-21). The purpose of these contacts would not be briefings or debriefings of imminent or recent flights, but exchange of ideas for later activities and the establishment of effective working relations for the future.

Gemini. We recommend that a meteorologist be made a member of the crew of a future manned orbiting space observatory, in the first instance, Gemini (11-17). A small number of the Gemini pilots should be given special training in meteorology and in the use of meteorological satellite data to prepare them as competent observers, with the expectation that they will be able to make good use of their time in orbit to make valuable meteorological observations and inferences (11-17).

Crew Selection and Training. We urge the maximum possible participation of scientists in all space missions: on board vehicles in flight, at extraterrestrial stations, and on the ground. Participating scientific personnel would fall into the following categories:

(i) Scientist-astronauts—men who combine the experience and resourcefulness of trained scientist and trained astronaut;

(ii) Scientist-passengers—experienced, mature scientists with adequate training in critical and emergency spacecraft operations;

(iii) Ground scientists—leading scientists in pertinent fields who collaborate with spacecraft personnel in the accomplishment of the scientific mission;

(iv) Astronaut-observers—astronauts with varying degrees of special training in making scientific observations.

The recruiting of qualified personnel for training should begin now. Recruitment procedures should assure that all qualified individuals in the appropriate scientific disciplines have an opportunity to apply, regardless of their current employment status. Astronauts already in the program should be given scientific training appropriate to their early collaboration with ground scientists in coming missions. The National Academy of Sciences should be requested to devise and operate a mechanism by which: (i) the bases for selection of scientist-astronauts and scientist-passengers can be established; (ii) the nomination of successful candidates can be accomplished. Graduate fellowship support in the sciences will be required in order to build up an adequate pool of qualified candidates for the foregoing program (11-15).

It is probably not too early to recommend that biologists be included in the proposed NASA program of scientist-astronaut training, to ensure the availability of adequate personnel for the first Mars missions (11-6).

NASA must, of course, retain full control and responsibility for the flight qualification of all classes of personnel. During the remaining Mercury program, the Gemini program, and the Earth-orbiting phase of the Apollo program, a vigorous research program should be concentrated on human physiological and psychological reactions in the space environment, with the special object of determining those conditions necessary to make man an effective observer in space (11-15).

In connection with the foregoing selection and training program, we recommend that an institute of space sciences be established immediately adjacent to the major NASA training facility for astronauts. The proposed institute must be of the very highest scientific caliber in staff, laboratory, and library facilities, in order to support the continued scientific activities of scientist-astronauts and to facilitate major researches in the space-related sciences. It should maintain liaison with the major centers of research activity in these fields, both in the United States and abroad. It might be administered either under contract with a major university or by the office in NASA responsible for scientific research and planning (11-16, also 11-6).

E. Administrative and Policy Matters

The Life Sciences and Man in Space. We recommend that NASA continue to try to achieve greater coordination, information exchange, and the sharing of program responsibilities between the personnel in its manned program and unmanned program (4-10). In particular, early planning in the Apollo program should give greater emphasis to early scientific and engineering design information, and should make greater use both of existing unmanned spacecraft programs and of Earth-based observations and experiments as sources of information (4-10).

Further, we believe that the Gemini and Apollo programs afford significant opportunities for obtaining needed physiological and psychological information about man which can be exploited more fully, perhaps by coordination within NASA at the divisional level (9-14).

The Block Assignment and Use of Payload Space. There are now in this country investigators or groups of investigators who have proven their competence in the conduct of space experiment programs. In some of these programs—for example, those involving the measurement of magnetic fields and particle fluxes—coordinated systems of many detectors carefully planned by a single investigator or by several working together may be required to produce the most significant results. Sometimes a series of similar experiments in successive satellites will be required, but the exact nature of the later experiments in such a series may well depend on the result of an earlier experiment. In order to give this kind of investigation the flexibility that nurtures the free pursuit of scientific knowledge, we endorse the general concept of the block allocation of payload space to investigators of proven competence (7-1, 13-4). Such investigators may be allotted a certain volume, weight, power supply, telemetry schedule, etc., without requiring them to submit in advance a detailed description of what they propose to fly.

Small Satellites. It appears that the use of small satellites will also permit the same kind of flexibility (14-4; 2-4, -5). The existence in NASA's program of space observatories with payloads made up of many diverse experiments should not be allowed to prejudice the use of small satellites, limited essentially to single experiments or related sets of experiments under the control of single experimenters or collaborating groups (2-5). NASA should actively sponsor the construction of such small satellites, should provide the vehicles appropriate to launch them, and should obtain from its own tracking net or from that of the Department of Defense the orbital data needed by the experimenters (13-4; see also below).

Non-NASA Spacecraft. We believe that opportunities for basic scientific research aboard military spacecraft can be exploited more than at present by scientists outside the service complex with benefits to all concerned (14-4). The Navy and Air Force should make more generally known to the scientific community the opportunities that exist for the use of payload space on their spacecraft (14-4). These Services should pay careful attention to the review of experiments and the selection procedures, and to the establishment of sound and consistent policies with regard to testing payloads, the release of scientific data acquired on military flights, and other matters affecting the relationship between experimenters and launching agency (14-4).

Information provided to the Study indicates that a relatively large number of reliable vehicles (e.g., Thor, Atlas, Polaris) will be obsolescent for military programs in the near future and could be made available for space research programs. These opportunities should be investigated promptly and, if possible, plans should be developed to utilize the vehicles for scientific experiments.

Telemetry, Tracking, and Orbital Data. NASA should extend its net of telemetry stations to parts of the world not covered at present, and should make sure that the coverage is adequate for the expected increase in the number of future satellites, including those launched by the Department of Defense (14-4). NASA should make known to the scientific community the availability of orbital elements for unclassified spacecraft, as provided to NASA by DOD tracking systems, as well as for its own spacecraft (13-4, 14-5).

Release of Data. We approve the spirit of the NASA policy on the release and publication of satellite data (2-30). This policy protects the experimenter in his right to use his data ahead of all others for a reasonable length of time, but provides for eventual release of the data in the event of unusual delays, with these provisions being made in consultation with the experimenter. It may be that later experience will require modification of this policy in minor respects, but it does not seem possible to improve on it now.

Sharing of Observing Time. All the spacecraft of the currently planned series of orbiting astronomical observatories (except the first) will consist of a principal experiment at the prime focus of a 38-inch mirror, together with some subsidiary experiments. The third satellite of the series will have a stellar ultraviolet spectrograph, designed and constructed by the Goddard Space Flight Center, which can be programmed to obtain ultraviolet spectrograms of stars at the rate of approximately one spectrogram per orbital revolution. NASA has proposed a tentative plan by which this potentially very large observing program can be shared with scientists outside of NASA. We are enthusiastic about the intent of the plan and believe it may have wider applicability. In arranging the observing program of an orbiting observatory whose time is to be shared in this way, we agree that the prime investigator should have the first priority; prime investigators on earlier or succeeding observatories, second priority; and guest observers, lowest priority (2-30). It may be safely assumed, we believe, that those in control of the scheduling of observations will avoid any tendency on the part of the group of active observers to become a closed shop against applicants new to the field, that each request will be judged on its merits, and that foreign astronomers will receive the same consideration given to American astronomers (2-31).

Fuller Exploitation of Meteorological Data. The proper exploitation of the vast quantity of satellite meteorological data that is beginning to accumulate presents a problem. It would help if scientists potentially interested in satellite meteorology could be identified at an early date and encouraged to assume a responsibility for exchanging ideas and instigating research in their areas of competence. In this way, nuclei of scientists will naturally and easily evolve that can serve as foci for research activities in limited areas by making certain kinds of data more easily available, holding conferences on special subjects, and in general assuming the role of leaders in the development of this newest school of meteorology (8-7). The success of this evolutionary process will be promoted by a coordinated plan of action and the wholehearted support of the government program offices.

Communications, Including Distribution of Data. The communication of scientific plans requiring coordination, exchange of scientific data and results among nations, among scientists, between scientists and government agencies, between NASA and other federal agencies, and within NASA itself, all present problems which are becoming increasingly complex (9-20). This is not a single problem, but several interrelated ones, warranting continuing study at appropriate levels. These problems include: reducing delay times in providing the data to experimenters in useful form; finding means for coping with the ever-increasing quantities of data, including consideration of the possibilities of partial on-board data reduction; providing mechanisms whereby scientists throughout the world can have ready access to and make optimum use of the data for the advancement of human knowledge; and publication problems, currently an increasing concern because of the large and growing quantity of literature in the field.

NASA's Relationship to Education and Training. We recognize the necessity for developing a vigorous academic program in all aspects of the space endeavor (12-7). We recommend that NASA pursue its present policy toward university research at the graduate level and extend application of this policy to as many additional universities as possible, both large and small. We accord full approval to the programs of research grants, research contracts, training grants, and facilities grants. Broadly conceived research grants not aimed at specific projects are regarded as the lifeblood of a vigorous university research program (12-7).

We have several specific suggestions which we believe will improve the existing program. We recommend that NASA extend the scope of its summer programs for selected undergraduates, and provide funds through research grants and contracts for undergraduate research assistantships (12-7). We recommend that NASA look with favor on research proposals that encourage faculty members, particularly assistant professors, to spend a period on full-time research in space science (12-9). We believe that NASA should develop a program of postdoctoral fellowships subject to safeguards that will keep the program from being used to replace the original research which the candidates should have performed for their Ph.D. degrees, or to stretch out still further the number of years required for a candidate's scientific apprenticeship (12-9).

In view of the importance to NASA of understanding the behavior of large systems, particularly engineering systems, NASA should support appropriate aspects of university research in systems engineering (12-10). In this whole field of activity, ways should be sought to break down the present partition between engineering and the physical sciences.

We believe that NASA should familiarize itself with the problem of technician education and training and its potential solutions (12-10). NASA should stimulate industry-education cooperation in metropolitan areas where such cooperation does not now exist, and should support existing cooperative efforts where appropriate (12-10).

Space Projects Potentially Harmful to Science. We realize that there are many space activities outside the control of NASA; nevertheless we ask NASA to take the lead by watching its own projects for potential sources of interference with other scientific researches. Further, we urge that parallel actions be taken in the Department of Defense and the Atomic Energy Commission, and that final over-all responsibility for identifying and making decisions concerning such potentially harmful operations be placed in the office of the National Aeronautics and Space Council (2-31).

We specifically recommend that NASA designate a person whose duty it will be to monitor NASA operations in order to identify space projects potentially detrimental to scientific research (2-32). The designated person should maintain liaison with the Space Science Board.

F. International Cooperation

The present level of emphasis by the United States on programs of international cooperation in space science seems to be amply justified by the results to date and by those expected in the future (15-6). Increased emphasis is recommended, but only to the extent that the projects involved shall continue to have a sound scientific, technological, and economic basis. NASA's existing program is imaginative and effective in achieving scientific objectives by international cooperation and should be continued under its present policies and guide lines (15-8).

The United States should continue to use its present multipronged approach to international cooperation. Ideally, we should strive wherever possible to secure multilateral programs, but we recognize that bilateral programs are often desirable and necessary, and that the two types of programs are complementary (15-9). Cooperation through government organizations (UN, bilateral agreements) will continue to be necessary and should be encouraged, but the importance of nongovernmental scientific organizations (COSPAR, the international scientific unions, etc.) should continue to be recognized (15-9). U. S. scientists should continue to support and strengthen COSPAR as the forum for nonofficial scientific contacts between national space committees and as the focal point for the space interests of the international scientific unions associated with ICSU (15-11).

The avoidance of contamination of celestial bodies, Mars in particular, is a matter of great importance to future space research, and should be considered in scientific discussions by the international scientific community. The recent establishment by COSPAR of a Consultative Group on Potentially Harmful Effects of Space Experiments may provide a new mechanism for stimulating discussion of this topic (9-13, 10-7, 15-14).

As a means of encouraging the involvement of foreign scientists in space research at the working level, NASA is urged to consider the use of continuous telemetry, together with cooperative agreements with interested foreign scientists to read out and reduce the data themselves for some of the solar monitoring satellites or similar monitoring experiments (15-15). To this end, NASA is urged to review carefully the possible use of continuous telemetry on satellite projects of intermediate size and complexity.

The National Academy of Sciences should recommend to the IAU that observatories in other countries be encouraged to cooperate in maintaining a continuous patrol of the planets, and in observing the Moon to gather the detailed optical data necessary for lunar chart construction (15-15 and 16; see also Chapter Four).

G. Some Social Implications of Space Activities

The national space program obviously has economic, social, and political consequences. The deliberations of the working group concerned with these consequences indicate that interesting studies could be undertaken on a number of topics of which the following are examples (see Chapter Sixteen):

1. Impact on the national economy in terms of proportion of the federal budget required to support the program and the possible contribution to growth of the gross national product.

2. Impact on the economy of local communities with major space installations, such as New Orleans (Michoud) and Houston, where new NASA activities will be a major economic factor.
3. Impact of transfers of space technology to the general economy or to specialized industries serving the general economy.
4. Impact on existing manpower resources and on training facilities for scientific and technical personnel.
5. Factors determining the degree of public support for the large expenditures required for space endeavors.
6. Organization to maximize scientific productivity in the context of a large-scale and complex program.
7. Balanced use of national intellectual resources, in view of the large proportion of professional personnel diverted to space activities, including the impact on other scientific activities and on the social sciences and humanities.
8. Opportunities and problems in foreign policy created by the space program, particularly in terms of relations with the Soviet Union, control of armaments, and space experiments affecting the environment of the Earth.
9. International implications of the need for overseas ground facilities, operational communications and meteorological satellites, and future development of satellite launching capabilities in other countries.

Appendix I

Organization and Conduct of the Space Science Summer Study

Planning for the program was undertaken by the secretariat of the Space Science Board in collaboration with J. A. Van Allen as general chairman and with W. W. Kellogg as vice-chairman. Representatives of the NASA Office of Space Sciences collaborated closely with the secretariat in developing the plans. Guidance was also provided by Lloyd V. Berkner (then Chairman of the Space Science Board) and by the Board's Executive Committee through periodic reviews of the planning.

Initial planning for the Study focused on the development of a broad outline for the summer's program and the enlistment of U. S. scientists to undertake the review of the NASA space sciences program. A score of scientists, representing all scientific areas comprising the NASA program, was envisaged as the full-time complement required to carry out the Study program; the competence of this group would be augmented by calling in other scientific specialists on a part-time basis as specific areas of the NASA program were considered. A group of twenty-one scientists participated full time in the eight-weeks program and ninety-two others joined in the endeavor on a part-time basis (see Appendix II).

As the program of the Summer Study evolved, arrangements were also made for the assistance of key NASA scientific and administrative personnel in the program. Eighty-nine NASA representatives (Appendix II), of both Headquarters and field centers, joined with Summer Study participants during the course of the eight weeks' activity. These representatives participated fully in the discussions to inform participants concerning the details of NASA's current programs and plans in each scientific field and to describe the policies and procedures employed in the conduct of the operating program. NASA representatives did not, however, join in development of the Study conclusions and documents which constitute the body of this report.

The assistance of other government agencies was also freely given as topics relevant to space research programs in these agencies were considered by the Summer Study. Particularly, representatives of the Department of Defense, the Atomic Energy Commission, the National Science Foundation, and the National Bureau of Standards provided major contributions to the Study by informing participants concerning the space-related activities conducted in these agencies.

An unusual feature of the Summer Study was the participation of six university graduate students in the program. With NASA's endorsement, the Board invited the participation of a small number of promising graduate students in science whose careers could profit by attending the deliberations and by association with eminent space scientists. These young men acted as rapporteurs for the various working groups. Their names are given in Appendix II.

The Study Program

The agenda (Appendix III) suggests in broad outline the program developed for the Space Science Study. The Study opened with a series of plenary sessions at which briefings were presented on existing space science programs and the procedures and policies used for their implementation. Those presentations outlining various aspects of the NASA program are reproduced in full in the Supplement to this report. The next two weeks were devoted to working group sessions which considered in detail the programs in each scientific area. This period was followed by a one-day plenary session to receive preliminary reports of these working groups and to plan succeeding stages of the Summer Study. Subsequent weeks were devoted to working group sessions dealing with administrative and policy matters, followed by a second plenary session again to receive interim reports and to formulate plans for final Study documents. The summer's program closed with oral presentation of the Study's findings to key NASA officials.

The first week's briefings provided Study participants with an over-all perspective of the National Aeronautics and Space Administration program and, particularly, of the program conducted by the NASA Office of Space Sciences (see Supplement). This review of the space program was supplemented by similar program reviews presented by representatives of the Department of Defense, the Atomic Energy Commission, the National Science Foundation, and the National Bureau of Standards. Considerable discussion was engendered by these briefings, which helped to amplify participants' comprehension of the programs, and also served, in part, to identify matters that should receive more thorough examination later in the Summer Study.

Working groups were then formed to review the NASA space research program in particular scientific areas: stellar, galactic, and radio astronomy and solar physics; celestial mechanics; lunar and planetary research; particles and fields in space; atmospheres of the solar system; bistatic radar astronomy; sterilization of space probes; fundamental biology and exobiology; meteorological research.

Before the Study began, chairmen and vice-chairmen for the principal working groups had been selected and arrangements had been made for scientific specialists in each program area to join the study for this phase of the work. In each working group key NASA personnel initially reviewed in detail the elements of NASA's current program and plans for the future that were relevant to the interests of that particular group. On the basis of the status of scientific achievement and knowledge in each field, the working groups then considered scientific goals for the program in the coming years.

After some two weeks of such deliberation, the Summer Study reconvened in plenary session to receive preliminary working group findings and recommendations and to reach a consensus regarding the scientific programs. During this session plans were also formulated for subsequent working groups which would give consideration to aspects of NASA's administrative procedures and policies by which the space research program is carried out. Many of these aspects were definable from previous normal activities of the Board and the known concerns of the scientific community; others had emerged during earlier deliberations of the Summer Study.

Consequently, working groups were established in the following areas: NASA/university relationships, organization and scope of NASA scientific program, organization of life sciences in NASA, man as a scientist in space exploration, data problems, block allocations of payload space, scientific opportunities on DOD spacecraft, international programs, and social implications of the national space program.

As with previous working groups, a large number of scientists having special qualifications was again invited for the deliberations undertaken by these working groups. Again, opportunity was afforded for detailed discussions with NASA officials and scientists regarding specific NASA procedures and policies applied in the implementation of the space sciences program. Many of the working groups had the benefit of extended discussions with NASA on these matters, enabling them to evaluate carefully the import of these policies and procedures to the science program. Other groups, for a variety of reasons, were unable to conduct their deliberations in similar depth. The current space science program is so dynamic that Study participants were particularly aware of the difficulties in establishing fixed policies and procedures; it was readily apparent that many of these topics require relatively continuous examination and appraisal. As a consequence of the dynamic character of the program, several of the reports on the foregoing subjects are, in some respects, necessarily inconclusive or qualified.

Following some three weeks devoted to these matters, the Summer Study again convened in plenary session on July 30 and 31 to receive preliminary reports of the working groups and to formulate plans for the development of final reports.

The Study concluded its program with oral briefings for James E. Webb, Administrator of the National Aeronautics and Space Administration, and other senior NASA officials on August 8 and 9. During these briefings all of the major findings and recommendations developed by the Space Science Summer Study were presented. These sessions provided the opportunity to outline the considerations leading to adoption of the respective recommendations and findings, and to participants' views as to the effect which their adoption would have on the space research program.

Management and Facilities of the Study

Management of the Space Science Summer Study was undertaken by the secretariat of the Space Science Board with very substantial assistance from the State University of Iowa.

Following selection of the State University of Iowa as the site for the Study, close liaison was maintained with its representatives responsible for management planning and the provision of facilities. The University, it should be noted, responded enthusiastically to the proposal by the Space Science Board that the Study be undertaken on the Iowa campus and liberally contributed its facilities and resources to assist the program.

Appendix II

List of Participants
Space Science Summer Study

Full-Time Invited Participants

Dr. L. V. Berkner, President, Graduate Research Center of the Southwest
Dr. H. G. Booker, Center for Radiophysics & Space Research, Cornell University
Dr. A. H. Brown, Department of Botany, University of Minnesota
Dr. A. J. Dessler, Graduate Research Center of the Southwest
Dr. Von R. Eshleman, Radioscience Laboratory, Stanford University
Dr. John W. Firor, Director, High Altitude Observatory
Dr. Robert Galambos, Department of Physiology & Psychology, Yale University
Dr. A. B. Giordano, Dean, Graduate School, Polytechnic Institute of Brooklyn
Dr. Samuel Glasstone, Consultant, Los Alamos Scientific Laboratory
Dr. LeVan Griffis, Dean of Engineering, Rice University
Dr. H. D. Hedberg, Department of Geology, Princeton University
Dr. H. H. Hess, Department of Geology, Princeton University
Dr. W. W. Kellogg, Head, Planetary Sciences, The Rand Corporation
Dr. J. Ross Macdonald, Director, Physics Research Laboratory, Texas Instruments, Inc.
Dr. Gene R. Marner, Director of Research, Collins Radio Company
Dr. Norton Nelson, Chairman, Institute of Industrial Medicine, New York University
Dr. George F. Pieper, Applied Physics Laboratory, The Johns Hopkins University
Dr. Wm. E. Porter, School of Journalism, State University of Iowa
Dr. Leo Steg, Manager, Space Sciences Laboratory, General Electric Company
Dr. J. A. Van Allen, Head, Department of Physics & Astronomy, State University of Iowa
Dr. L. C. Van Atta, Director, Hughes Research Laboratories

Part-Time Invited Participants

Dr. Philip Abelson, Director, Geophysical Laboratory, Carnegie Institution of Washington
Dr. W. Ross Adey, Department of Anatomy, University of California at Los Angeles
Dr. S.-I. Akasofu, State University of Iowa
Dr. Daniel Alpert, Director, Coordinated Science Laboratory, University of Illinois
Prof. Edward Anders, Enrico Fermi Institute for Nuclear Studies, University of Chicago
Dr. Allen V. Astin, Director, National Bureau of Standards
Dr. K. C. Atwood, Department of Microbiology, University of Illinois
Dr. John C. Bellamy, College of Engineering, University of Wyoming
Dr. H. S. Bridge, Department of Physics, Massachusetts Institute of Technology
Dr. Arthur Brodbeck, Yale Law School

Dr. H. S. Brown, Division of Geological Sciences, California Institute of Technology
Dr. Carl W. Bruch, Schwartz Laboratories
Dr. B. F. Burke, Department of Terrestrial Magnetism, Carnegie Institution of Washington
Dr. L. D. Carlson, Department of Physiology, University of Kentucky
Dr. Arthur Cherkin, Don Baxter, Inc.
Dr. C. O. Chichester, Agricultural Experiment Station, University of California
Dr. Sydney Clark, Geophysical Laboratory, Carnegie Institution of Washington
Dr. E. U. Condon, Chairman, Department of Physics, Washington University
Dr. A. P. Crary, Chief Scientist, Antarctic Programs, National Science Foundation
Dr. H. J. Curtis, Chairman, Department of Biology, Brookhaven National Laboratory
Dr. Henry David, President, New School for Social Research
Dr. V. G. Dethier, Zoological Laboratory, University of Pennsylvania
Dr. A. J. Deutsch, Mt. Wilson and Palomar Observatories
Dr. F. D. Drake, National Radio Astronomy Observatory
Dr. F. K. Edmondson, Department of Astronomy, Indiana University
Dr. H. M. El-Bisi, Department of Food Technology, University of Massachusetts
Dr. R. O. Erickson, Division of Biology, University of Pennsylvania
Dr. E. B. Espenshade, Jr., Chairman, Department of Geography, Northwestern University
Dr. J. W. Findlay, Deputy Director, National Radio Astronomy Observatory
Dr. S. W. Fox, Director, Institute for Space Biosciences, Florida State University
Dr. J. D. French, Director, Brain Research Institute, University of California at Los Angeles
Dr. Herbert Friedman, Superintendent, Atmosphere and Astrophysics Division, U. S. Naval Research Laboratory
Dr. Sigmund Fritz, Chief Scientist, Meteorological Satellite Laboratory, U. S. Weather Bureau
Prof. Thomas Gold, Center for Radiophysics and Space Research, Cornell University
Dr. Leo Goldberg, Harvard College Observatory
Mr. Joseph M. Goldsen, Associate Head, Social Science Department, The Rand Corporation
Dr. S. A. Gordon, Division of Biology & Medical Research, Argonne National Laboratory
Dr. H. Keffer Hartline, Biophysics Section, The Rockefeller Institute
Dr. George Herbig, Lick Observatory
Dr. N. H. Horowitz, Division of Biology, California Institute of Technology
Dr. F. S. Johnson, Graduate Research Center of the Southwest
Dr. Joseph Kaplan, Department of Physics, University of California at Los Angeles
Dr. William Kraushaar, Laboratory for Nuclear Science, Massachusetts Institute of Technology
Dr. G. P. Kuiper, Director, Lunar and Planetary Laboratory, University of Arizona
Dr. A. E. Lilley, Harvard College Observatory
Prof. D. B. Lindsley, Chairman, Department of Psychology, University of California at Los Angeles
Dr. R. B. Livingston, Chief, Laboratory of Neurobiology, National Institutes of Health
Dr. N. U. Mayall, Director, Kitt Peak National Observatory
Mr. Alfred Mayo, Astronautics Division, Chance Vought Corporation

Dr. H. Fernandez-Moran, Head, Mixter Laboratories for Electron Microscopy, Massachusetts General Hospital
Dr. H. J. Morgenthau, Director, Center for the Study of American Foreign Policy, University of Chicago
Dr. T. O. Nevison, Department of Aerospace Medicine and Bioastronautics, Lovelace Foundation
Dr. E. P. Ney, School of Physics, University of Minnesota
Dr. David Novick, Head, Cost Analysis Department, The Rand Corporation
Dr. E. F. Oakberg, Oak Ridge National Laboratory
Dr. Herbert Pahl, Special Research Resources Program, National Institutes of Health
Dr. E. N. Parker, Enrico Fermi Institute for Nuclear Studies, University of Chicago
Dr. C. R. Phillips, U. S. Army Chemical Corps Biological Laboratories
Dr. C. S. Pittendrigh, Department of Biology, Princeton University
Dr. G. N. Plass, Manager, Radiation Physics, Aeronutronic Division, Ford Motor Company
Prof. E. C. Pollard, Department of Biophysics, Pennsylvania State University
Dr. R. W. Porter, Engineering Services, The General Electric Company
Dr. C. Ladd Prosser, Head, Department of Physiology, University of Illinois
Dr. Leonard Reiffel, Director, Physics Research, Armour Research Foundation
Dr. H. L. Richter, Jr., Manager, Advanced Electronics & Information Systems Division, Electro-Optical Systems, Inc.
Dr. M. D. Ross, Head, Environmental Sciences, General Motors Company
Dr. Wm. G. Rosser, Visiting Associate Professor, Department of Physics and Astronomy, State University of Iowa
Dr. C. E. Sagan, Department of Astronomy, University of California at Berkeley
Prof. Harold Saunders, Department of Sociology, State University of Iowa
Prof. Martin Schwarzschild, Princeton University Observatory
Prof. E. G. Segré, Department of Physics, University of California at Berkeley
Dr. Frederick Seitz, President, National Academy of Sciences
Mr. J. W. Senders, Bolt, Beranek & Newman, Inc.
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Appendix III

Agenda and Chronology for the Space Science Summer Study

June 17 - August 10, 1962

June 17 4:00 p.m.

Welcome and informal reception

June 18-19 10:00 a.m. - 12:00; 1:30 - 4:30 p.m.

Introductory statement by Study chairman
Open discussion of major scientific problems in nation's space program

June 20-22 Briefings by NASA and other government agencies

June 20 8:30 a.m. - 12:00

Briefings on current and future programs in space sciences—NASA:

Space Science Program	H. E. Newell
Manned Space Flight Program	J. F. Shea
Space Applications	M. J. Stoller
Advanced Research and Technology Development	A. Gessow

1:30 - 5:00 p.m.

Briefings on current and future programs in space sciences—other government agencies:

Department of Defense Program	L. Allen
Army Program	R. M. Hurst
Navy Program	W. Berg W. E. Wright
Air Force Program	C. G. Stergis

June 21	8:30 a.m. - 12:00	
	Atomic Energy Commission Program:	
	Space Power Supplies	G. M. Anderson
	Nuclear Rocket Propulsion	H. R. Schmidt
	Biomedical Research Program	N. Barr J. Liverman
	Research Program in Physical Sciences	A. R. Van Dyken
	National Science Foundation Program	W. J. Koltun L. Karel
	1:30 - 5:00 p.m.	
	NASA space science mission briefings:	
	Grants and Research Contracts	T. L. K. Smull
	Program Review and Resources Management	J. D. Nicolaides
	Launch Vehicle Program	V. L. Johnson
June 22	8:30 a.m. - 12:00	
	Geophysics and Astronomy Program	J. E. Naugle
	Bioscience Program	O. E. Reynolds
	Lunar and Planetary Program	O. W. Nicks
	Tracking and Data Acquisition	C. R. Morrison
	Closing Remarks	H. E. Newell
	1:30 - 3:00 p.m.	
	National Bureau of Standards	K. Kessler R. J. Slutz
	Closing Remarks	L. V. Berkner
	3:00 - 5:00 p.m.	
	Organization of working groups by scientific areas	
June 25 - July 6	9:00 a.m. - 12:00; 1:30 - 5:00 p.m.	
	Consideration of scientific programs by Working Groups on Astronomy, Fields and Particles, Atmospheres of the Solar System, Lunar and Planetary Research, Biology (week of June 25; joint meeting with Fields and Particles on July 6), Meteorology (June 29 only). Detailed program briefings by representatives of NASA and other government agencies as appropriate; discussion and formulation of group plans; development of findings, positions and recommendations.	

July 3, 6	Seminars of general interest	
July 3	Main Phase of Magnetic Storms and the Ring Current	S.-I. Akasofu
July 6	Neutral-Point Theory of the Aurora	S.-I. Akasofu
July 9	9:00 a.m. - 12:30 p.m.; 2:00 - 5:00 p.m.	
	Plenary Session	
	1. Progress reports by Chairmen of working groups in scientific areas:	
	(a) Astronomy	J. W. Firor (for L. Goldberg)
	(b) Fields and Particles	J. A. Van Allen
	(c) Atmospheres of the Solar System	H. G. Booker
	(d) Meteorological Research	W. W. Kellogg (for H. Wexler)
	(e) Lunar and Planetary Research	H. H. Hess
	(f) Biology	A. H. Brown
	2. Identification of special topics requiring joint coordination by working groups in scientific areas.	
	3. Outline of plans for future working groups:	
	(a) July 10-12: International Relations	R. W. Porter
	(b) July 23-29: Man as a Scientist in Space Exploration	N. Nelson
	(c) July 24-27: Social Implications of the National Space Program	J. M. Goldsen
	(d) July 26-27: Sterilization of Space Probes	A. H. Brown
	4. Consideration of agenda for July 10-13 discussions with NASA officials on administrative, organizational, and policy topics.	
July 10-12	9:00 a.m. - 12:00; 1:30 - 5:00 p.m.	
	Working Group on International Relations	
July 10-13	Working group discussions with senior officials of NASA on administrative, organizational, and policy topics.	

July 10 2:00 - 5:00 p.m.

Data Problems: Handling, reduction and distrubution of observational data; questions of on-board data processing; exclusive rights of prime experimenters; computational capabilities and future requirements; publication and dissemination of results; data center needs.

July 11 2:00 - 5:00 p.m.

NASA/university relationships and scientific manpower and training problems.

July 12 9:00 - 12:30

Scientific uses of spacecraft launched by other federal agencies.

July 13 9:00 - 12:30

Procedures for selection of experimenters and experiments; evaluation of role and function of NASA Space Sciences Steering Committee and Subcommittees.

July 13 2:00 - 5:00 p.m.

Balance between NASA program of research in space with NASA support of ground-based and theoretical research; university facilities, and other types of necessary ancillary programs.

July 16 9:00 a.m. - 12:30

Working Group on NASA Scope and
Organization (continuation of
July 13 discussion)

2:00 - 5:00 p.m.

Working Group on Bistatic Radar Astronomy

July 17 9:00 a.m. - 12:30

Working Group on Atmospheres of the Solar System (consideration of Working Group report)

July 17 2:00 - 5:00 p.m.

Working Group on Bistatic Radar Astronomy
Working Group on Lunar and Planetary Research (chemical and TV photography; review of Working Group report)

July 18 9:00 a.m. - 12:30 p.m.

Working Group on NASA/University Relationships and Scientific Manpower and Training Problems

2:00 - 5:00 p.m.

Working Group on Bistatic Radar Astronomy

3:45 - 5:00 p.m.

Working Group on Fields and Particles (consideration of Working Group report)

July 19-22 Visits and Briefings: Ling-Temco-Vought (Dallas) and Cape Canaveral

July 23-29 9:00 a.m. - 12:30 p.m.; 2:00 - 5:00 p.m.

Working Group on Man as a Scientist in Space Exploration

July 23 Working Group on NASA/University Relationships

July 24-27 Working Group on Social Implications of the National Space Program

July 26 Working Group on NASA/University Relationships

July 26-27 Working Group on Space Probe Sterilization

July 30-31 9:00 a.m. - 12:30 p.m.; 2:00 - 5:00 p.m. (July 30 only)

Plenary Session

1. Reports of Working Groups

(a) NASA/University Relationships; Manpower and Training	H. G. Booker
(b) Scope and Organization of the NASA Scientific Program	L. Griffis
(c) Social Implications of the National Space Program	J. M. Goldsen

July 30-31	(d) Scientific Opportunities on Spacecraft Launched by Other Federal Agencies	L. C. Van Atta
	(e) Small Satellites	G. F. Pieper
	(f) International Programs	H. Odishaw
	(g) Man as a Scientist in Space Exploration	N. Nelson
	(h) Space Probe Sterilization	A. H. Brown
	(i) Celestial Mechanics	L. Steg
	(j) Bistatic Radar Astronomy	H. G. Booker
	2. Development of plans for final report	
	3. Discussion of plans for final briefings on August 8-9.	
July 31	p.m.: Tour of Collins Radio Company (Cedar Rapids)	
August 1	Working Group on NASA/University Relationships	
August 1-7	Consolidation of findings and preparation of reports	
August 5-6	Working Group on Organization of the Life Sciences in NASA	
August 8-9	Final briefings	
August 8	9:30 a.m. - 12:30 p.m.	
	1. Resume of Summer Study Program	J. A. Van Allen
	2. General Management and Policy Problems	
	(a) NASA/University Relationships	H. G. Booker
	(b) Organization and Scope of NASA Scientific Program	L. Griffis
	(c) Block Allocation of Payload Space	G. F. Pieper
	(d) Scientific Opportunities on Spacecraft Launched by Other Federal Agencies	L. C. Van Atta
	2:00 - 5:00 p.m.	
	(e) International Programs	H. Odishaw
	(f) Social Implications of the National Space Program	J. M. Goldsen
	(g) NASA Bio-Management	N. Nelson
	(h) Man as a Scientist in Space Exploration	N. Nelson

August 9	9:00 a.m. - 12:30 p.m.	
	3. NASA Opportunities and Policies in Scientific Areas	
	(a) Biological Research in Space	A. H. Brown
	(b) Lunar and Planetary Research	H. H. Hess
	(c) Meteorological Research	W. W. Kellogg
	(d) Particles and Fields in Space	A. J. Dessler
	2:00 - 5:00 p.m.	
	(e) Stellar, Galactic, and Radio Astronomy; Solar Physics	J. W. Firor
	(f) Celestial Mechanics	L. Steg
	(g) Atmospheres of the Solar System	H. G. Booker
	(h) Bistatic Radar Astronomy	V. R. Eshleman
	(i) The Context of the Summer Study	W. E. Porter
	4. Concluding Remarks	L. V. Berkner
August 10	Administrative matters.	

Appendix IV

List of the Working Groups and Subgroups
Space Science Summer Study

The name of the chairman of each working group or subgroup is indicated in parentheses after the group title. Where two names appear, the second is that of the co-chairman or vice-chairman. Some of the subgroups were not formally constituted, so that the names given are actually those of persons who in consultation with others prepared studies on special subjects for the use of the main working group.

1. Working Group on Astronomy (Goldberg, Deutsch)
   - Subgroup on Radio Astronomy (Findlay)
   - Subgroup on Ground-Based Solar Studies (Firor)

2. Working Group on Particles and Fields (Van Allen, Dessler)
   - Subgroup on Interplanetary Plasma (Snyder)
   - Subgroup on Solar Cosmic Rays (Van Allen, Ney)
   - Subgroup on Geomagnetically Trapped Radiation (Van Allen, Dessler)
   - Subgroup on the Geomagnetic Cavity (Cahill, Dessler)
   - Subgroup on the Aurora and Airglow (Ney)
   - Subgroup on Galactic Cosmic Rays (Dessler)
   - Subgroup on Geomagnetic Fluctuations (Cahill)
   - Subgroup on the Zodiacal Light (Ney)

3. Working Group on the Atmospheres of the Solar System (Booker, Kellogg)

4. Working Group on Lunar and Planetary Research (Hess, Hedberg)
   - Subgroup on Ground-Based Observations and Experiments, Moon (Van Atta, Shoemaker)
   - Subgroup on Ground-Based Observations and Experiments, Venus and Mars (Abelson, Wise)
   - Subgroup on Television and Direct Photographs (Sternberg)
   - Subgroup on Meteorites (Anders, Whipple)
   - Subgroup on Libration Points (Steg)
   - Subgroup on Return and Analysis of Lunar Samples (Anders)
   - Subgroup on Spacecraft vs. Experiments (Van Atta)
   - Subgroup on Problems Related to and Scientific Tasks for the Apollo Program (Nelson)

5. Working Group on Biology (A. H. Brown, Pittendrigh)
   - Subgroup on Radiation (Atwood)
   - Subgroup on Morphogenesis (Gordon)
   - Subgroup on Biological Rhythms (Prosser)

    - Subgroup on Microbiology (Horowitz)
    - Subgroup on Biophysics (Fernandez-Moran)
    - Subgroup on Plant Physiology (Erickson)

6. Working Group on Meteorological Rockets and Satellites (Wexler, Kellogg)

7. Working Group on International Relations (R. W. Porter, H. S. Brown)

8. Working Group on Man as a Scientist in Space Exploration (Nelson, Galambos)
    - Subgroup on Scientific Undertakings for Man in Space (Kuiper, Shoemaker)
    - Subgroup on Man's Place in Sensing and Control Loops (Eshleman)
    - Subgroup on Selection, Training, and Career Development (Lindsley, Galambos)

9. Working Group on Space Probe Sterilization (A. H. Brown) [originally a subgroup of Biology]

10. Working Group on the Social and Economic Implications of the National Space Program (Goldsen)

11. Working Group on Bistatic Radar (Booker, Eshleman) [originally a subgroup of Atmospheres]

12. Working Group on the Scientific Uses of Spacecraft Launched by Other Federal Agencies (Van Atta)

13. Working Group on Data Problems (Van Allen)

14. Working Group on NASA/University Relationships (Booker)

15. Working Group on Celestial Mechanics (Steg, Szebehely) [originally a subgroup of Astronomy]

16. Working Group on Block Allocation of Payload Space (Pieper)

17. Working Group on the Scope and Organization of NASA's Space Science Program (Griffis)
    - Subgroup on NASA/Industry Relations (Van Atta)

18. Working Group on the Organization of the Life Sciences in NASA (Nelson)

Chapter Two

ASTRONOMY*

I. Introduction

Since 1947, when the first ultraviolet spectrograms of the Sun were obtained from above the atmosphere with sounding rockets, astronomy has been on the threshold of a long-awaited era. With the launching of NRL's Solar Radiation Satellite I and, now, NASA's stabilized Orbiting Solar Observatory I, the narrow bounds which the terrestrial atmosphere has always imposed on the exploration of the full astronomical spectrum are breached still further. The spectrum opens out, not for just the few precious seconds when a sounding rocket or the X-15 climbs the apex of its quick trajectory, but for weeks and even months at a time.

The Orbiting Solar Observatory (OSO), with its complement of various detectors, marks the first in a carefully planned series of orbiting observatories, some to observe the Sun and some the stars. Successive members of the series will grow in power and versatility; the detailed designs are well advanced for the next three or four. Launchings of all spacecraft planned at present will occur within the next four years, if the existing schedule can be maintained. Some tentative plans are already being made for the orbiting observatories that will first go into service in 1967 and later years.

The successful flight of the first OSO is therefore highly portentous for all of astronomy. While it is still in orbit, and its successors are still abuilding, the time is ripe for an independent reassessment of the whole astronomy program. This has been one objective of the Space Science Summer Study. We have tried to steer a middle course between a study that is so broad that its conclusions find no applications in the present conduct of the NASA program, and one that is so detailed that it attempts to judge the scientific utility and/or engineering feasibility of every specific experimental proposal.

> FINDING: In broad outline we endorse the present NASA astronomy program.

II. Suborbital Space Research and Small Satellites

The advent of the orbiting observatories does not herald the obsolescence of the more modest vehicles that have promoted space astronomy in the past. Some of these vehicles, like the Stratoscope balloons and AeroBee-Hi rockets, offer the great advantage that the instruments can be recovered with little damage and used

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*See Appendix III for list of participants in the Working Group on Astronomy.

again. The results of each flight may therefore be taken into account during the preparation of the next one. An obvious advantage accrues to an observing program with this kind of quick feedback. Defects of instrumentation may be corrected at relatively slight cost, and later experiments may be modified for the elucidation of unexpected results or ambiguities turned up by earlier ones. We shall note some other arguments that attest to the continuing necessity for experiments with vehicles which are less sophisticated than the orbiting observatories.

A. Balloons

Most of the important ultraviolet spectrum still remains inaccessible even at the highest altitudes that can be attained by balloons. In addition, it now appears doubtful whether balloon-borne telescopes can achieve an angular resolution better than 1/3 second of arc for solar observations. Schwarzschild thinks that it may be practically impossible to avoid air turbulence inside the telescope (caused by solar heating) to the degree necessary to reach a definition of, say, 1/10 second of arc on the Sun. A great deal of important balloon work remains to be done, however, within these limits of resolution and spectral range. Examples that have been mentioned include: $H\alpha$ observation of the Sun with the resolution of Stratoscope I (about 1/2 second of arc); the study of micrometeoritic dust in the upper atmosphere by means of coronagraph observations of the air above the balloon; infrared observations of the planets; and the high-definition photography of astronomical objects at night when the troubles caused by solar heating are absent.

B. X-15

The X-15 is another vehicle that may carry astronomical instruments above most of the atmosphere. Code has developed a gyro-stabilized platform with a stability of several minutes of arc. He plans to use this device to facilitate photography of the Orion region in the spectral range 2000-3000 Å. He also has built a telescope fitted with an ultraviolet photometer, for use in the X-15. The aircraft can carry a 1000-pound payload to a height of 50 miles; it remains at high altitude for 5 to 10 minutes at a time. However, most flights occur during daylight hours, and astronomical observations always take second place after aeronautical measurements of an engineering nature, which have priority. Nevertheless, it is clear that important exploratory observations can sometimes be made from this vehicle. These observations may serve as a guide to the kinds of measurements that should be attempted from the orbiting observatories; they may also provide a clue to the kinds of astronomical observations that will be suitable for assignment to astronauts on future space missions.

RECOMMENDATION: We urge that these astronomical capabilities of the X-15 be utilized.

C. Sounding Rockets

The value of the sounding rocket in space research is evident. The present-day scientific program of research from satellites and deep space probes owes its origin almost entirely to the scientific results obtained from sounding rockets in the last fifteen years. Many important results have been obtained with their use in the fields of meteorology, aeronomy, ionospheric physics, and energetic particles; in astrophysics their value has been proved by the extension of the solar spectrum through the extreme ultraviolet and into the X-ray region. The first and only extreme ultraviolet stellar spectra have been obtained from a sounding rocket.

The principal question is: to what extent should the sounding rocket program be continued, or is it nearly outmoded now that orbiting vehicles are becoming available? The answer to this question is very clear.

FINDING: The sounding rocket will continue to be essential in the space program.

There are several reasons for this conclusion: (i) Exploratory measurements. The sounding rocket makes possible the investigation of many astrophysical phenomena at a moderate cost prior to setting up an orbiting observatory program; for example, the profiles of spectrum lines of the Sun in the extreme ultraviolet other than Lyman-α should first be investigated from rockets, not only to determine their general nature before constructing a satellite experiment, but also to discover whether they are of sufficient interest to warrant a thoroughgoing study of a monitoring nature. (ii) Brief observations. Sounding rockets are better adapted and less expensive than orbiting vehicles for observations that can be accomplished in a few minutes' time and do not require monitoring. (iii) Recovery. Recovery of data and instrumentation is now fairly reliable from the Aerobee-Hi rocket flown at White Sands. This makes it possible to examine the results, 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. Recovery also permits using photographic and other methods that are very difficult from orbiting spacecraft. (iv) Training. The sounding rocket program provides an opportunity for young and inexperienced scientists to develop their skills under conditions which promote creativity and a sense of accomplishment, with more room for individual experimentation and exploratory measurements.

The next step in sounding rocket development is the inertially guided Aerobee-Hi rocket. This development, sponsored by NASA, is expected to produce within the next year a rocket which can be pointed within one minute of arc in yaw and pitch at a succession of any five moderately bright stars. A solar-pointed Aerobee will also be developed, a modification in which an optical sensor rather than an inertial system produces the fine pointing at the Sun. These developments are of equal importance; the one permits making extensive studies of the extreme ultraviolet spectra of stars prior to the orbiting of an OAO, the other makes it possible to conduct many more sophisticated experiments on the radiations emitted from the Sun than are possible with the biaxial pointing control. Among these solar experiments are spectroheliograms at various extreme ultraviolet and X-ray wavelengths. The three-axis control will prevent the Sun's disk from rotating relative to the instrument, and thus makes possible attaining an adequate exposure during the time of flight of a rocket. Many of these solar experiments cannot be conducted from the present OSO, and should be carried out from rockets prior to design of experiments for the advanced OSO.

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.

D. Small Satellites

When considering scientific satellites, questions frequently arise as to the relative roles of the large "observatory" types of spacecraft, carrying several or many different experiments, and the small single-purpose satellite. In the field of astronomy, certain kinds of observations of the Sun and the night sky can be equally well performed with a small satellite, such as, for example, the measurement of the radiation from the entire Sun in certain wavelengths and its variation with time, or scans of the night sky in selected spectral regions. Preliminary data on ultraviolet radiation from stars, planets, and diffuse objects in the night sky, at the present time limited to a very small number of rocket observations, could also be acquired by measurements from small satellites.

The Naval Research Laboratory group, in particular, has had considerable experience with small satellites (Solar Radiation I (GREB) and III, and LOFTI I) and proposes to continue this type of investigation. Although not working in the field of astronomy, Van Allen has also had very good results with small satellites and is a strong proponent of their use, where appropriate. Many of these satellites were launched piggy-back on a larger space vehicle, a method which greatly reduces the cost of launch per satellite. The failure of some of them to separate properly presents a straightforward engineering problem that can be cured without great difficulty. A comparable technique would be to launch a number of small satellites in a cluster, where a common orbit is suitable for all.

There is little question that a small satellite with its own simple telemetry system offers advantages to the experimenter, particularly in providing more nearly than a multi-purpose satellite the conditions under which the typical independent-minded investigator would wish to work: he retains control over his own investigation as far as is possible under the physical and economic limitations of satellite research; he may even read out his own data on an immediate basis; and he usually reduces them himself. In some respects a small satellite affords greater flexibility; for instance, if a given experiment requires a particular orbit or a scanning rate and pattern that can be provided by spin stabilization, conflicts with other on-board experiments are not a concern.

Data storage and command readout systems have been used successfully on many small satellites, and indeed are essential if 100% recovery of the data is required. On the other hand, continuous transmission of data (which to date has been associated only with small satellites) is to be recommended not only on the basis of simplicity, but also because it gives anyone in the world who can tune a simple receiver to the satellite transmission frequency the opportunity to read out and reduce the data, and thus participate in space research in a very real sense. The result would be to broaden the present base of space research by enlisting the interest of many scientists at home and abroad who now feel that space research is too complex and risky to commit themselves to. In the present state of affairs, it is true that one is taking a chance of getting only about 2% of the potentially usable data per observer; but in some programs this makes relatively little difference, especially if the satellite is monitoring a slowly varying physical parameter, or if arrangements can be made for the adequate geographic distribution of (essentially volunteer) observers. (In this connection it is noteworthy to observe that, before Solar Radiation Satellite II was launched, Friedman wrote to a number of persons who he thought might be interested in recording its transmissions, and received a gratifying response.)

An experimenter proposing an investigation involving small satellites should be asked, does the use of such a satellite better serve a scientifically useful end than other available approaches? The words, "scientifically useful," should be interpreted in the broad sense, of course, with due weight being given to some of the less tangible factors discussed above.

> FINDING: In the light of the foregoing discussion, the existence of large space observatories in NASA's program, such as the OGO, OSO, and OAO, must not be allowed to prejudice the use of small satellites, essentially limited to a single experiment under the control of a single investigator (or group of investigators at one laboratory).

It must be realized that active investigators are not likely to propose small-satellite experiments unless there is some possibility that the proposal will be approved; if the scientific community is to take advantage of small satellites, the fact that such satellites are available must be made more widely known. Suitable launch vehicles must be obtainable on reasonable time schedules, and individual experimenters must have access to the body of engineering experience which has been gathered from the small satellites launched to date.

III. Orbiting Solar Observatories

A. General Considerations

There is little disagreement among solar astronomers on the broad and important questions now awaiting solution. These questions can be listed briefly.

(i) Evolution: What is the origin and course of evolution of the Sun and similar stars?
(ii) Internal Structure: What are the details of the processes by which energy progresses outward from the center of the Sun? What is the composition and physical state of the Sun at all levels? What is the origin of the solar magnetic field and the solar cycle?

Answers to the foregoing questions lean heavily on theoretical studies and laboratory work on properties of atoms. The following questions are more directly related to present solar observations:

(iii) Photosphere: What are the physical conditions in the photosphere? What is the spectrum of the turbulence observed there?
(iv) Chromosphere: What is the structure of the chromosphere? In particular, how can one account for the increase of temperature with height which continues into the corona?

(v) Solar Activity: What is the origin and energy supply of the many sporadic phenomena observed on the Sun — sunspots, flares, radio bursts, etc. ?

(vi) Corona: What is the form of the outer corona? How does it connect with the interplanetary medium?

Those scientists studying the Sun at the present time feel the need for improvements in observational capabilities in at least two respects that are uniquely attainable by space facilities. These are angular resolution and wavelength range.

1. Angular Resolution. A number of problems of interpreting phenomena on the Sun depend at present on improved knowledge of the fine structure of the solar atmosphere. This is true for problems of photospheric turbulence, chromospheric models, the physics of active solar regions, and the heating of the chromosphere and corona. Most of the world's solar astronomers are now engaged in studying one or more of these problems. Solutions to these problems depend strongly on better angular resolution.

There are some opportunities for improvement in the resolution in observations made from the ground. With full exploitation of such techniques as the utilization of good observing sites, the suppression of turbulent air currents in and around the telescope dome structure (sometimes done by discarding the dome altogether), shutter control by photoelectric seeing monitors, very short exposures, and possibly other techniques, the number of photographs of solar features with resolution of 1 second of arc or better will be greatly increased.

In the special case of the height gradients in the solar chromosphere, a ground-based technique is available which allows even better effective resolution. During a typical solar eclipse, observations of the chromosphere closely spaced in time during totality record the emission from the narrow region of the chromosphere covered or uncovered by the Moon's motion during the short time interval between observations. The height resolution so obtained is equivalent to that which could be derived from direct observations with a resolution of 1/10 second of arc.

Experience to date with the balloon-borne telescope for photographing white-light features of the solar disk demonstrates that a resolution of 1/2 second of arc can be dependably obtained at 80,000 feet altitude. The limit arises in part from solar heating of the mirror and of the small amount of air still in and around the telescope.

Each improvement of angular resolution in the ground-based and balloon observations contributes to the understanding of solar phenomena. It is important to realize, however, that none of the improvements and techniques suggested in the preceding paragraph, with the special exception of the eclipse observations, can obtain a resolution smaller than the scale height of the phenomena observed. In the higher solar atmosphere the temperature and scale heights become very large and the resolution obtained with ground-based measurements may be smaller than the scale height; however, current disagreements in the temperature of this region as determined by different methods can be interpreted as being due to an as-yet-unobserved fine structure in the corona.

The resolution needed to make major progress in an area is not always known. In the case of the spectrum of photospheric turbulence a resolution of 1/10 second of arc should be sufficient. Pictures of active features of the chromosphere and low corona made at the limb of the Sun with a resolution of 1 second of arc or slightly better show features at the limit of resolution of the picture. Similarly the quiet chromosphere has features which visual observers claim are finer than anything yet photographed.

> FINDINGS: In summary, it seems clear that the capability of photographing or otherwise recording solar features with an angular resolution of 1/10 second of arc is needed now and would produce great advances in solar physics. This requirement cannot be met from the ground, nor from any solar spacecraft now being planned. There seems to be no reason why the pointing and stabilization techniques being developed for the OAO cannot be adapted to a solar observatory. The development of such a spacecraft represents the next important task for solar astronomers to take up with the active support of NASA.

2. Wavelength Range. Almost all of the visible light from the Sun originates in a layer of the Sun's atmosphere, the photosphere, which is a few hundred kilometers thick. Higher layers are therefore difficult to observe using visible light, not only because of the obscuring effect of the bright photospheric light, but also because the higher layers, having conditions of temperature and density quite different from the photosphere, radiate mostly in wavelengths outside the visible band. Similarly, the active features of the Sun, such as active prominences and flares, represent a wide range of physical conditions, and a complete description of these phenomena requires observations made over broad wavelength regions outside the visible.

For two reasons the extension of satellite observations to the ultraviolet and X-ray region seems more important at present than an extension to the infrared. First, the infrared radiation comes principally from the upper photosphere, and its observation may not tell us much that is not already deducible from observations in the visible. Infrared observations would actually favor regions of lower temperature than the photosphere, and most of the solar atmosphere above the photosphere is at a much higher temperature than the photosphere. Second, much of the infrared emission from the Sun can be observed from the ground and, even better, from balloons through a series of atmospheric windows; this field is far from being fully exploited.

Much effort has already gone into ultraviolet and X-ray observations of the Sun from rockets and satellites. Rocket-borne instruments have sampled the ultraviolet and X-ray emissions, and have photographed and scanned the spectrum of the Sun from the visible down to about 100 Å. The first OSO has given repeated scans of the far-ultraviolet spectrum over several weeks and has thereby demonstrated that the line emission in this wavelength range is variable, some lines increasing in intensity at times of solar flares.

The needs for improved angular resolution and for extension of measurements to shorter wavelengths are not, of course, independent. We confidently predict that the need for angular resolution in the ultraviolet and X-ray wavelengths will be similar to those needs already developed in the visible wavelengths.

B. Present OSO Program

The OSO (S-16) has opened up the ultraviolet and soft X-ray wavelengths for essentially continuous observation. The scheduled S-17 will add the capability of scanning the disk of the Sun at particular ultraviolet wavelengths with a scanning aperture of about 1-1/2 minutes of arc, and will provide routine monitoring at Lyman-α, He Iλ584, and He IIλ304 with 1-minute resolution, and in two X-ray bands. The S-17 will also explore the possibilities for detecting the visible light from the outer corona, taking advantage of the lower intensity of interfering scattered light outside the atmosphere.

> FINDING: We foresee a continuing need for explorations of the solar spectrum and recommend that S-16/S-17 type flights be continued at the rate planned by NASA, about two launchings per year for several years.

The need for continued use of the OSO will not come exclusively from the requirements of solar physics. For example, the study of the ionosphere and the higher regions of the Earth's atmosphere is dependent on a quantitative description of ultraviolet radiation incident on the atmosphere. One sensitive technique for measuring the height gradients of terrestrial atmospheric constituents is the measurement of the solar ultraviolet spectrum as a function of height of the spectrometer or photometer carried in a sounding rocket. Success of this technique depends in part on a satellite monitor of the solar ultraviolet spectrum so that the height curves may be corrected for solar variability during the sounding rocket flight.

The OSO is especially needed in the next few years for observations to be used in studies of flare prediction. (See the section on ground-based solar observations for a discussion of this point.)

C. Advanced OSO Program

Possible design specifications for an advanced OSO, as explained by NASA representatives, assume a Thor-Agena B launch and a polar, slightly retrograde, orbit. They provide for payload space which would allow optics 10 feet long and up to 22 inches in diameter, pointing accuracy of 5 seconds of arc, pointing stability of 1 second of arc for 5 minutes, and various raster scans of selected regions of the Sun by the whole spacecraft. Data storage could be 40 x 10^6 bits/orbital revolution.

These specifications describe a spacecraft which is a major improvement over the S-17 in two respects: (i) in the pointing accuracy and stability and (ii) in the size of optics that can be carried. The great need for stable pointing stems from the requirement for improved angular resolution discussed above. The need for longer and heavier optics will certainly arise, both because of the long focal lengths required to make use of the high-resolution capabilities, and because of the need for high spectral resolution to analyze in detail the ultraviolet and possibly visible spectrum. The proposed spacecraft is therefore the next logical step needed to advance solar physics.

RECOMMENDATION: We recommend that NASA develop, for solar observations, a spacecraft more advanced than the present OSO, to be ready for use in late 1965 or early 1966.

Certain auxiliary instrumental development must progress rapidly if full advantage is to be taken of the advanced OSO. Foremost among these is the consideration of the thermal problems of high-resolution optical systems pointed at the Sun. If the problem of thermal stability of the optics can be solved, the projected pointing stability of 1 second of arc for 5 minutes and the payload diameter of 22 inches hold the possibility of approaching, with short exposures, the desired 1/10-second resolution. Full utilization of such high resolution requires also the existence of electrical read-out image tubes of several-thousand-line resolution (see Section VII. A of this Chapter) and the commitment, at least occasionally, of the entire memory to this image device.

IV. Orbiting Astronomical Observatories

A. Present Program (OAO) and Future Program (10 Year)

In this report we use the term ''Orbiting Astronomical Observatory'' (OAO) in the narrow sense to refer to that particular model of spacecraft now being developed by Grumman Aircraft Engineering Corporation under contract with NASA for launch in 1964 and succeeding years. It is basically a spacecraft which can carry a 36- to 38-inch reflecting telescope, together with adequate power and telemetry, and is capable of being pointed and stabilized to maintain orientation with a precision of 4 to 5 seconds of arc in the first of the series, with expectation of eventual improvement to perhaps 0. 1''. A wide range of auxiliary equipment can be attached to the telescope, such as photometers, spectrometers and spectrographs, image tubes (in the broadest sense), and so on. Cells in the sides of the telescope tube, together with spare weight and telemetry capacity, will allow a few additional experiments to be placed on board, pointing parallel or at some fixed angle relative to the main instrument. These factors will make the basic OAO an extremely versatile instrument with which to gather data in the ultraviolet spectral range (including soft X rays) and in the infrared, for a wide variety of celestial objects (in fact, practically everything excepting the Sun).

An attempt was made to analyze the costs of the OAO program. Although the initial price is high (nearly $200 million for the first three OAO's, including development costs, the actual cost of three spacecraft, the experiments, launching, tracking, telemetry, data reduction and distribution, etc.), the cost of additional vehicles is estimated at perhaps $30-$40 million, provided the contractor's engineering team is maintained intact. This price will also buy a great deal of reliability in the form of a highly redundant system, designed in such a way as to minimize the probability that the system will fail all at once, and early. The several hundred spacecraft status measurements and the computer routine for troubleshooting and selection of alternate modes of operation should allow the OAO to continue functioning for some time, with only slowly diminishing effectiveness.

The initial three OAO's carry experiments to do survey experiments in the ultraviolet on stars, nebulae, the interstellar medium, and galaxies, starting with broad-band detectors and progressing toward greater spectrophotometric resolution. This program was noted with approval. There are many more advanced measurements that will obviously become desirable in later OAO's. These include measurements of equivalent widths and line profiles of stellar lines in the ultraviolet for the purposes of analysis of stellar atmospheres and determination of abundances, monitoring stellar activity in the ultraviolet, determining the wavelength dependence of interstellar polarization, and making high-resolution studies of star clusters and other objects. However, it is very difficult to make specific plans for more advanced projects until preliminary scientific data about ultraviolet astronomy become available from the first few OAO's. (For a comprehensive listing of problems for future research, see Chapter VII of Science in Space, Report by the National Academy of Sciences, 1960-61, and NASA's "Thinking Document.")

The analogy with conventional terrestrial astronomical facilities is clear. Just as terrestrial telescopes of, say, 24- to 36-inch aperture have not become obsolete with the construction of the Mt. Palomar 200-inch telescope, neither will the OAO be immediately outmoded by the next stage of development of larger orbiting astronomical facilities (see Section IV. B of this Chapter). We predict a long and useful life for the current model of the OAO, at least for the 10 years or so that seem required for its successor to be developed and launched, and probably a good deal longer. It is not conceivable that all the observational programs which such a versatile telescope is capable of carrying out will be finished in such a short time. The OAO also seems to provide the simplest mechanism by which an astronomer who wishes to do space research and is qualified to do so can hope to carry out an experiment or observing program, and he will probably prefer it to any other method.

> RECOMMENDATION: We recommend that, as far as possible, NASA schedule launchings of the OAO, so that, on the average, at least one OAO is operational throughout the next ten years. Decisions on what specific experiments to schedule for particular future OAO's must be kept as flexible as possible within the limitations of the long lead times imposed by both budgeting and engineering development.

This flexibility has two obvious advantages. First, it allows a maximum amount of feedback from earlier OAO's, not only for the improvement of techniques and design, but also — more important from the scientific standpoint — the following up of interesting leads uncovered in earlier exploratory surveys. Very few data are yet available, and we are still breaking very new ground. In particular, it would probably be a mistake at this early date to commit OAO IV (late 1966?) to the infrared program recommended by NASA's Ames Research Center for the study of planets, cool stars, a search for dark objects, etc.; the decision might well await the results of more intensive use of infrared techniques from the ground or balloons, which are just now getting under way (see Section VII. B, below). This is not to say that infrared studies should be rejected indefinitely, for they comprise an important area of research; the question concerns only the best method of pursuing them.

A second important reason for keeping decision-making as flexible as possible is that it gives an experiment that fails to get into orbit and function on the first attempt another chance in a fairly reasonable time. From the administrative standpoint, this minimizes the necessity for duplicate spacecraft and standby vehicles set aside for the purpose of launching them. This factor is also important from the standpoint of universities with graduate students who are counting on a successful flight to get material for graduate study. A graduate student must have some assurance that, during the several years he can afford to stay in school, the experiment on which he may be depending for his data will actually fly.

* * *

The proposed NASA schedule of launching at approximately 9-month intervals was examined. Several factors are working at cross purposes here. Factors in favor of a longer interval are: the desire of astronomers not to be forced into a too hectic a race with time; the need to allow enough time between successive launchings to permit the technical and scientific feedback to have effect; and the need, in the competition for funds and manpower, to stretch out expenses over somewhat longer intervals. Factors militating against a longer interval are: the advantages in economy and efficiency of keeping an engineering team together to build a series of similar spacecraft; and the necessity of keeping the time between a scientific idea and its fruition as short as possible, which, as noted, is especially important in graduate schools with space-science programs.

The possibility of developing rendezvous, docking, and retrieval techniques and their application to satellites for maintenance and modification (discussed in Section VI. B) may have a bearing on the design of the latest members of the proposed OAO series (say, 1968-1972), and also on the prospective pace of successive launchings. It appears possible to modify the present mechanics of integrating the instruments into the spacecraft system to simplify the problems of introducing substitute experiments while in orbit, if desirable. Assuming that it will be possible to reach the satellite to repair or modify it, and assuming that reliability will continue to improve steadily, the number of launchings required to keep one functioning OAO in orbit may well be reduced to considerably less than one per year.

B. Future Large-Aperture Space Telescope

The planning of a large-aperture space telescope is the next logical step during the next 5 to 10 years in developing a successor to the telescopes in the present OAO program. Since the latter is now estimated to cost $200 million or more, depending on the number of these satellites launched during the remainder of this decade (1964-1969), and the maximum aperture is only 36 to 38 inches, it is obvious that a much larger instrument, say 100 inches or more, would represent a truly enormous investment for astronomy. For this reason, it is vital that its scientific justification receive the most careful and comprehensive consideration by the astronomical and related scientific communities. Insofar as presently possible, investigators whose research interests require the largest telescopes having highest resolution should take time to identify those problems whose solution depends upon, or may be significantly advanced by, a large gain in telescopic power.

The magnitude of such a project is not only great in cost, but also in technological scope and in man-hours for its realization. Thus, the thinking necessary for its conception and initiation cannot start too soon. Although the time in the Summer Study devoted to its discussion was hardly sufficient for a thorough examination of such a program, some of the most obviously important topics associated with a large-aperture space telescope were reviewed. Among these topics are: (1) aperture size; (2) satellite vs. lunar location for an observatory; (3) the possibility of manned maintenance; (4) development of new techniques; (5) the need for a study group of national scope.

1. Aperture Size. It was generally agreed that the next step beyond the OAO telescopes should provide for improvement by a really significant factor in both light-gathering power and in resolution. Although it still remains to be seen whether a 36-inch mirror can be optically figured and observationally operated to its theoretical limit of 0.1 second of arc, either during a high-altitude balloon flight (Stratoscope II) or in an Earth satellite (OAO II), there seems to be good reason to regard this possibility as within the reach of present techniques. In any case, commitments have been made, and actual construction has begun, on this assumption. Considering capabilities 10 years hence, augmented by the benefit of intervening experience, it is reasonable to count on the use of high-quality primary mirrors several times as large as those now being constructed. If this expectation is well founded, a gain in light-gathering power by a factor of 10, and in resolution by a factor of 3, does not seem unrealistic. Therefore, an aperture of 100 inches or more could be set as a goal, but this should not be made a specification for fear of freezing on an unrealistic number. On the other hand, to plan anything much bigger than 100 inches may result in taking too great a leap at once.

2. Satellite vs. Lunar Location for an Observatory. The group consensus on this lively issue favors a location on an Earth satellite, mainly because of the advantages of zero gravity, lower cost, and the possible easier accessibility by orbital rendezvous or from an accompanying space station (see Section VI. B). Lunar gravity would require a much heavier structure to control flexure, probably with the use of numerous bearings which give trouble in a vacuum; this difficulty would be much reduced on an Earth satellite. It is also likely that the effects of the great changes in temperature on the lunar surface would be more difficult to overcome than those in a satellite. A satellite, however, might have to be launched into a higher orbit than the OAO, because of the relatively rapid and large variations in solar radiation to which a close satellite is exposed. Although the Moon may have advantages, such as a firm base for telescope stabilization, and large amounts of matter at hand for protection against high-energy radiation, the price to be paid and the time required for these purposes is likely to be far too high to justify building a telescope on the Moon. If the amount of dust in motion near the surface of the Moon is large, this fact alone would practically eliminate the Moon as a desirable location.

3. The Possibility of Manned Maintenance. Any very large telescope is so complex that it seems certain to require regular maintenance. Thus, the very first thinking about the large-aperture space telescope must include the concept of manned access not only to permit initial adjustments, if necessary, and later maintenance and repair operations, but also to permit the even more important activities of installation of new and different auxiliary equipment, and the recovery of data or material that cannot be easily telemetered. The existence of projects such as the DynaSoar, Space Plane, and Satellite Interceptor indicates that the possibility of manned access to an orbiting observatory can be realistically considered (see Section VI. B).

4. Study Group for Large Space Telescope. Preliminary studies for a large-aperture space telescope involve so many ramifications that it must become a national enterprise; it is important for the success of any large project that able and interested persons be involved in the earliest planning while there is still a wide range of choices to be made. Some choices need to be made now for a large space telescope.

> RECOMMENDATION: Consideration should be given to the desirability of a large space telescope, and the technical problems associated with it, as the next step beyond the present OAO program. It is suggested that a small study group be organized for the summer of 1963 to explore this subject, and to prepare a report delineating the relevant technical problems, together with the scientific objectives insofar as they can be foreseen. Such a study group should explore problems associated with a large, highly versatile space instrument, intended primarily for stellar and nebular studies through the entire spectral range from soft X rays to infrared, to which a single telescope can be adapted; but it should also consider problems associated with solar telescopes.*

V. Radio Astronomy from Space Vehicles**

A. Introduction

Radio astronomy is a young science which started in 1932 but which began extensive growth only after World War II. It is properly regarded as a branch of astronomy, with as wide a scope, embracing the study of all celestial objects by the radio waves they emit. So far in radio astronomy much attention has been directed to specific tasks, but it is well to realize that, as is true for astronomy, future progress in the science must be based on the availability of a large quantity of good astronomical observations. To produce this observational material is one of the main tasks of radio astronomers.

Measurements of the positions, intensities, and polarization of the discrete radio sources must be made. Both the intensity and state of polarization of the radiation from the sky background must be measured. Special studies have to be made of celestial objects such as the Sun, the Moon, the planets, and many of the most interesting extragalactic radio sources. All these observations are needed throughout a wide range of radio frequencies. It is well known that radio astronomical observations can be made from the ground over a spectral range of about

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*A small minority of the Working Group on Astronomy of the Space Science Summer Study abstained from this recommendation for the following reason: At a time when not a single image of a celestial body has been obtained in a satellite, it is premature to convene a group to study a space telescope larger than the 38-inch telescope of the OAO.

**See also Appendix I to this Chapter. Section V was prepared by a Subgroup on Radio Astronomy, whose participants are given in Appendixes I and III to this Chapter.

ten octaves, from near 10 Mc/s to about 10,000 Mc/s. In comparison with this spectral range, one may note that the visible spectrum covers only about one octave. For these reasons it is true that, for many years to come, the main emphasis in radio astronomy is certain to be on observations made from the ground.

The advent of space technology, however, opens the radio-astronomical window wider. By making observations from above the ionosphere, radio telescopes can be used at frequencies much lower than 10 Mc/s. These observations are likely to be crucially important in elucidating the nature of nonthermal sources. The monochromatic intensity of these objects is still rising steeply at the lowest frequencies observable from the ground. This means that an estimate of the total radio flux from such an object depends critically upon a very insecure extrapolation of the spectrum to lower frequencies.

It should be realized that there will be a frequency limit at the lower end of the spectrum even for satellite observations. This limit, which depends on the electron density prevailing in the interplanetary space, is probably as low as 100 kc/s (a wavelength of 3 kilometers) but is not known for certain. The ionosphere of the Earth will act as a shield to protect a space observatory from most of the man-made radiation coming from the Earth at frequencies between this lower limit and 10 Mc/s.

Observations from well above the Earth's atmosphere will also extend the radio-astronomical window at the short-wave end. Atmospheric absorption and radiation affects ground-based radio astronomy at wavelengths shorter than 3 centimeters. In order to explore the gap between this wavelength and the infrared regions, radio telescopes will have to be placed above essentially all of the Earth's atmospheric constituents.

B. The First Space Radio Astronomy Experiments

The first experimental work from space vehicles in radio astronomy are concentrating on the long-wavelength end of the spectrum. Preliminary results from these relatively simple radiometers and antennas will be of great value. Experiments are planned and being made to measure the total integrated radio flux from the sky at frequencies in the range from hundreds of kilocycles to 4 Mc/s. For these experiments, antennas without great directivity will be sufficient. Although the experiments are mainly exploratory in nature, their results will be very valuable both immediately and as a basis for future planning. They will extend the measurements which have been made from the ground by Reber and Ellis, who have attempted to observe at low frequencies through holes in the ionosphere. These same low-frequency experiments may give further information on the spectra of bursts of radio noise coming from the Sun and from Jupiter. At longer wavelengths, such experiments may tell how the electron content of the upper part of the ionosphere blends into the electrons in the solar corona or in interplanetary space.

> FINDING: Space radio measurements, of course, widen the radio window both to the longer and shorter wavelengths. The ground-based observations at the long-wavelength edge of the window are limited exclusively by the inability of the radiation to penetrate the atmosphere, while at the short-wavelength and instrumental limitations are as

important as the window restrictions. These facts suggest that it is proper for the early exploration to emphasize the long wavelengths.

C. The Next Steps in Space Radio Astronomy

1. Technological Requirements. Several technological steps on which work is already proceeding have to be achieved before advances in space radio astronomy can be made after the first sketchy explorations. At the short-wave end of the spectrum, sensitive, stable, and well-calibrated radiometers working in the millimeter and submillimeter wave range are needed. Antennas — probably parabolic dishes or similar designs — must be developed, and these must be capable of being carried into space and there must maintain the accuracy of their surface figures. The antennas must be capable of being directed quite precisely, and it must be possible both to check the direction of pointing of the antenna and to calibrate its gain.

At the long-wave end of the radio spectrum, the chief need is for technological advances in the field of antennas. To achieve directly even modest directivity requires antenna systems of large size. Even a 5° beam antenna at 100 kc/s requires an antenna 30 to 35 kilometers in extent. The technique used on the ground to simulate large antenna arrays (by aperture synthesis, for example) may reduce some of the size requirements on the space radio-astronomy antenna, but cannot circumvent the need for at least large linear spacing. If, however, directive antennas can be built or simulated in space, and if they can be calibrated both in their direction of pointing and in their gain, there is an interesting future open for long-wave space radio-astronomy experiments. (See Section VI. A., on possible lunar site.)

RECOMMENDATION: We recommend that NASA support development of antennas, radiometers, and other equipment required for space radio astronomy.

2. Possible Future Space Radio Astronomy. If it be assumed that the technology described above is available, many problems lie open for study. There will almost certainly be need for more exploratory experimentation before some of the subjects are attacked. A list of possible experiments, both at the long- and the short-wave ends of the radio window are given in Tables I and II.

The antennas represent the chief unknown in this discussion. Means of extending large long-wave antennas are not yet known in detail, nor is it known whether millimeter-wave antennas could be light (inflatable) structures or whether they would have to be comparatively massive structures capable of withstanding launch accelerations.

D. Conclusion

Space radio astronomy is limited at present for two reasons: The first is the need for the new technology which has been discussed above. The second is the limitation imposed by the fact that relatively few scientists are working in the field and by the fact that many challenging problems which face the scientist can be solved by ground-based radio-astronomy measurements.

FINDING: Space radio astronomy will grow in the next ten years only if ground-based radio astronomy also grows vigorously. We believe that this fact justifies NASA participation in the support of ground-based radio astronomy. (See Section VII.)

TABLE I

Experimental Objectives of Space Radio Telescopes Operating Below Frequencies of 10 Mc/s.

Objective	Experimental Technique
Galactic and extragalactic radio emission mechanisms	Multichannel radiometric observations of the flux distribution of cosmic noise between 10 Mc/s and 100 kc/s. (Such experiments made on the non-thermal radio sources will determine the spectra of the sources at the low-frequency end where the shape of the spectrum gives a sensitive test of the emission mechanism.)
Dynamic solar radio spectra	Multichannel and sweep-frequency radiometric observations of the dynamic spectra of solar radio outbursts.
Ionized component of the interstellar medium (measurement of low "emission measures")	Precision determinations of the brightness temperature of interstellar space in the frequency interval between 100 kc/s and 1 Mc/s.
Investigations of the interplanetary medium	Multichannel radiometric observations of the cutoff of low-frequency cosmic radio noise at times of solar disturbances. Time average determinations of the lowest frequency of propagation possible in the interplanetary medium.
Planetary radio noise	Close-proximity observations of planetary radio noise sources due to sporadic disturbances, non-thermal spectra, and radio noise sources in regions of the Van Allen type.
Planetary ionospheric experiments	Modulation of multichannel reception of cosmic noise in the frequency range between 100 kc/s and 10 Mc/s, produced by a changing electron density environment. Requires a satellite in an eccentric orbit about a planetary ionosphere.
Lunar ionosphere	Detailed intercomparison of the cosmic radio noise received by a multichannel radiometer on the surface of the Moon, using the noise received by a similar radiometer orbiting the Moon as a standard of reference.

TABLE II

Experimental Objectives of Centimeter-Wave and Millimeter-Wave Space Radio Telescopes

Objective	Experimental Technique
Extension of solar observation	Multichannel radiometric observations of the solar radio noise between 5 mm and 1 mm. This wavelength interval pertains to the deepest layers in the solar radio source envelope.
Lunar thermal studies	Multichannel millimeter-wave radiometric observations of the lunar surface as a function of lunar phase and surface characteristics. (Small millimeter-wave telescopes operating in a satellite system orbiting the Moon obtain in each revolution information requiring one month's observation from the Earth. Extension of the spectral interval under observation will provide information pertinent to the physical characteristics of the lunar surface material, and radio resolution of small regions on the surface of the Moon impossible to observe individually from terrestrial locations.)
Planetary thermal observations	Observations of the millimeter-wave spectra of planetary surfaces and atmospheres from close by. (The variability of the thermal planetary spectrum from limb to limb, as could be observed by a fly-by probe or a satellite orbiting about the planet, provides information pertinent to the surface temperature of the planet, the composition and physics of its atmosphere. A thermal map of its surface provides information related to the planetary rotation rate and orientation of the rotation axis (e.g., Venus) and permits the resolution of surface features impossible to resolve from the Earth.)
Galactic continuum and radio star observations	(i) Spectral distribution of the thermal component of cosmic radio noise in the galactic plane. (ii) Selected discrete sources, both thermal and non-thermal, may ultimately be observed between 1 and 10 mm with sufficient resolution and radiometric sensitivity.

TABLE II (Continued)

Experimental Objectives of Centimeter-Wave and Millimeter-Wave Space Radio Telescopes

Objective	Experimental Technique
Microwave spectroscopy of planetary atmospheres	(i) Millimeter-wave frequency-scanning radiometers observing the spectral self-emission of resonant transitions of molecules in planetary atmospheres. (ii) Frequency-scanning observations of the microwave "Fraunhofer" spectrum, by observing the planetary molecular transitions in absorption against the solar millimeter-wave spectrum.

VI. Astronomical Tasks for Man in Space

A. Apollo

Man in space will serve astronomy in two main ways: first, by the *in situ* examination of satellites and planets; and second, by the maintenance and improvement of observatories in space. The first major step towards these possibilities is the Apollo program, by which NASA expects to carry out a manned landing on the Moon during the interval 1967-70.

Recommendations to NASA, for a program of scientifically useful exploration, investigation, and experimentation that the Apollo astronauts can reasonably be expected to conduct, so far fall into four classes: (i) observation and exploration of the large-scale features of the lunar surface itself, including mapping; (ii) the selection and return of mineral samples for geochemical, physical, and biological analysis; (iii) studies in small-scale surface physics, e.g., the effects of erosive and destructive processes, such as bombardment by short-wave radiation, cosmic rays, and micrometeorites; and (iv) the emplacement of equipment, either to measure properties of the body of the Moon (e.g., seismographs and magnetometers, and transit-type telescopes to measure lunar librations) or to gather astronomical data on extralunar objects (e.g., dipole antennas and receivers for long-wave radio astronomy, radio interferometers, optical stellar interferometers to resolve close double stars and measure diameters of stellar disks, and other pieces of equipment similar to those recommended for the OAO program).

The first three classes are of central importance; in fact, to state the obvious, such investigations are the prime scientific reason for going to the Moon at all.

In class (iv) the distinction should first be made between equipment that measures properties of the Moon itself and that which makes use of the Moon as a stable space platform from which to secure astronomical data. Equipment that measures properties of the Moon itself is clearly to be classed with the other *in* situ investigations already mentioned, and is equally important. Whether or not such instruments are carried to the Moon and emplaced there depends more on logistics than on science — that is, on whether travel of more than a few meters on the surface of the Moon will be possible, whether a vehicle for surface travel will be provided, whether such large loads can be carried on the first few missions, whether a separate cargo carrier may be possible or necessary, and so on.

We shall not attempt here to exhaust the arguments concerning the astronomical experiments. The recommended experiments are indeed very good ones from the scientific standpoint, but there is certainly a question whether they could not be performed equally well from a space vehicle. In fact, some of the experiments may already have been performed by the time of the Apollo landings. The design and operation of a large space observatory for optical studies (similar in concept to the OAO and developing from it) was compared with an observatory on the Moon, and in the light of available facts, we doubt whether the Moon is the most suitable location (see Section IV. B. 2, Satellite vs. Lunar Observatory).

The case against emplacing radio-astronomy equipment on the Moon is less clear: some radio astronomers believe that the radio quiet on the far side of the Moon and the availability of the Moon as a platform for large arrays confer advantages that may outweigh the disadvantages of difficult access and the presence of gravity.

In any case, it is clear that the first few exploratory landings of the Apollo program will be difficult for the astronauts and that they will be preoccupied by the demands of returning to Earth alive. Whatever time they have for scientific tasks should certainly first be devoted to those investigations and explorations of the Moon itself which cannot be performed elsewhere. Only after these investigations are carried out in a preliminary way, and if there is still an opportunity to carry out further tasks and payload capacity to take the equipment to the Moon, should consideration be given to carrying out the purely astronomical investigations.

> RECOMMENDATION: In situ experiments on, or investigations and explorations of the Moon itself, which by their nature require man's presence on the Moon, should have priority before those making use of the Moon as a space platform for astronomical observations of extralunar objects.

B. Manned Maintenance and Modification of Space Telescopes

The prospect that man will be able to travel into space, and to the Moon within the next decade, opens up many possibilities for advances in astronomy which are not attainable in any other way. Certainly the present requirement for absolute reliance on remote control, operation, and maintenance of satellites can be relaxed somewhat. The presence of a human being on board an orbiting space observatory while observations are being made is emphatically not desirable, because his motions would upset the high degree of pointing stability demanded. A man, however, would be extremely useful for the following operations, which may be practically impossible to carry out by automatic or remote-controlled devices:

Repair of ordinary malfunctions. It seems likely that the useful lifetime of any space system would be greatly prolonged by the replacement of defective components and other simple repairs. It has been said that most of the satellites now in orbit but inoperative could be made to function again if a man with a screwdriver could get at them.

Resetting the instrumental range of operation in sensitivity, spectral response, etc. The ability to do this would confer much greater flexibility on instrumental design and the resulting range of operation of each instrument.

Retrieval of data in physical form. It is unnecessary to elaborate the obvious: an enormous amount of information can be packed into a single photograph, but the telemetry of an equivalent of information, especially over very great distances, presents a staggering technical problem.

Change of equipment and modernization. As in the case of terrestrial telescopes, the capability of attaching new, better designed, auxiliary equipment, or equipment designed to obtain data of a different sort, is greatly to be desired. It seems likely that if an orbiting observatory were built around a sufficiently large and versatile telescope, the ability to change attachments would postpone absolescence for many decades.

In passing, it should be mentioned that an observatory on the Moon, if it should prove desirable to build one, would benefit equally from service and maintenance operations of this kind; and in addition a man might participate in actual observations. Unknown factors are at present too great to warrant dwelling on this point.

C. Steps Toward Practical Systems

Present planning and development work on manned and unmanned rendezvous techniques hold forth the promise that a man can actually be put aboard an orbiting observatory to perform the tasks enumerated above. We refer to: (i) NASA studies of rendezvous techniques, leading to a practice rendezvous for Project Gemini, possibly in early 1964, and rendezvous for the Apollo Moon mission several years later; (ii) tentative NASA plans to provide Gemini crews with space suits that will allow them to leave the capsule; (iii) NASA plans to land later Gemini spacecraft with wings; (iv) current U.S. work on Projects DynaSoar and Space Plane, both essentially winged vehicles with the ability both to maneuver in the atmosphere and to get into and out of orbit; (v) current U.S. work on a satellite system with the ability to rendezvous with objects in space.

We understand that some thought is being given to the possibility of retrieving satellites from orbit with such vehicles, and that the outlook for such operations is good. Time scales of the order of 5 to 10 years have been quoted for the perfection of the foregoing techniques.

The economics of these operations will naturally need to be considered. Until costs of recovery are reduced by the perfection of controlled landing techniques which permit repeated use of the same satellite, it is probably better to think in terms of rendezvous and docking for maintenance and modification, and possibly of ejected capsules for recovery of data in physical form. In any case, for very large facilities rendezvous may be preferable to recovery. When recovery techniques are perfected, on the other hand, it may even be economically sound to consider recovering some of the earlier satellites, including those already launched, for their repair and modification, and replacement in orbit.

> FINDING: In short, there are obviously many ways in which a man can greatly enhance the effectiveness of operations with an orbiting scientific facility. We believe that the planning and design of astronomical space facilities for the future should very seriously consider the potential benefits of human assistance and intervention suggested by the foregoing possibilities.

Some very large questions still remain: How well will man be able to function on longer missions into space? How will he fare, out in the open in a space suit? How much careful, detailed work will he be able to do? In the light of experience in manned space flight so far, the answers look promising. The authorities on such matters are obviously those astronauts who have been in space. The viewpoint of these astronauts will undoubtedly strongly influence the design of future space systems. It is therefore important for scientists to know what the astronauts think and have to say on these questions, and important for the astronauts to understand what astronomers would like to do. The best way to accomplish this is by personal interchanges, especially during those periods well after a successful flight and when no other flight is imminent, when other pressures are not severe.

> RECOMMENDATION: We believe that the manned space flight program may become a decisive factor for astronomical research in space, to some degree by direct manned astronomical observations -- particularly, of course, in situ on the Moon and planets -- but mainly by making it possible to maintain and modify major astronomical facilities in space. With this in view, we urge NASA to make possible, in the near future, personal contacts between interested astronomers and some of the astronauts who have been in space. The purpose of these contacts should not be briefings or debriefings of imminent or recent flights, but should aim at the exchange of ideas for later activities and at the establishment of enthusiastic working relations for the future.

VII. Ground-Based Activities

New technical capability of launching high-altitude rockets, artificial satellites, and deep space probes has opened dramatic new possibilities for astronomical research. These possibilities raise the question: What is the scope of accompanying ground-based research that would be appropriate for NASA to support?

To begin with, there are two large areas whose connection with the space program is so direct and obvious that their support by NASA should be a foregone conclusion. First, it is evident that the successful accomplishment of space astronomy missions requires a broad base of technological research and development. The principal problems here are the early identification of need and continued refinement of specifications which will promote the development of adequate techniques and instrumentation on a suitable schedule. Second, it is also evident that the successful accomplishment of NASA missions requires relatively thorough descriptions of conditions in various regions of space, especially where manned expeditions are contemplated. Although a great fund of knowledge exists from past astronomical, physical, and geophysical observations, and from recent space observations, there are many fields in which further ground-based observations are required. For instance, better knowledge of the lunar-surface characteristics, the planetary atmospheres, and the nature of solar activity would clearly contribute directly to the success of NASA missions and to the successful design of space experiments.

We come next to several classes of ground-based work whose connection with the space science program is not quite so direct. One of these consists of ground-based observations which complement or assist in the interpretation of space observations. For example, ground-based solar observations in the optical and radio atmospheric windows would obviously assist in the interpretation of spaceborne solar observations. Similarly we cite the field of laboratory astrophysics, in which are obtained the physical data on wavelengths, transition probabilities, collision cross-sections for various processes, properties of plasmas with and without magnetic fields, and so on, all of which are vital for the successful interpretation of space-experimental results obtained in unfamiliar parts of the spectrum or corresponding to extremes of pressure or temperature. Another set of examples could be drawn from a great range of theoretical studies.

> FINDING: We believe that ground-based activities in astronomy, complementary to the space program, should receive NASA support almost as a matter of course, although detailed decisions on priorities for funding would naturally rest on the degree of urgency of a given investigation for the more immediate space program.

Successive categories of ground-based activities can be conceived to form a progression starting with activities which are essential to NASA (and must therefore be performed by NASA if not being performed by other institutions) and extending, at the other extreme, to conventional observations which furnish no obvious support for NASA missions. Since NASA missions directly or indirectly involve most branches of astronomy, it is difficult to find many examples which precisely illustrate the latter extreme. It is therefore conceivable that an extreme point of view eventually could be developed by which space studies would lay claim to all astronomy -- both spaceborne and ground-based -- as the logical scope of operations for NASA, but this is clearly impracticable. Nevertheless, it is essential that NASA be able to make sound scientific choices. In attacking any particular astronomical problem full latitude in choosing the method of approach should be allowed. Ground observatories, balloons, aircraft, sounding rockets, satellites, and deep space probes should be considered impartially. The cost, nature and quality of data, future utility of the instruments which are constructed, probability of success, and time schedule are among the factors which should be considered in selecting a method of approach. From a broad viewpoint, the interests of science and the country are both served best by including the option of ground operations. If an important astronomical question is of interest to NASA, can best be performed from the ground, and is not being sponsored by another agency, NASA should be free to consider including it in the NASA program.

> RECOMMENDATION: Support by NASA of space astronomy has not yet been accompanied by commensurate support of the equally important area of ground-based astronomical research. To help redress

the resulting imbalance should be largely the responsibility of the National Science Foundation and other federal agencies. It is clear that NASA should not assume this burden. We endorse the present NASA policy of funding ground-based research in those areas which are sufficiently closely related to problems of space astronomy. Such support is obviously appropriate where information is vitally needed for the consummation of NASA missions or for the interpretation of observations obtained with space vehicles. Nevertheless, it would be incorrect to assume that a sharp distinction can be drawn between astronomical research which does back up the space program and that which does not, and we therefore urge that ground-based astronomy be funded by NASA on the basis of a relatively liberal interpretation of what constitutes supporting research.

This statement pertains only to the research grants and contracts aspect of NASA support of astronomy, and not to the extensive and admirable university and training program already instituted by NASA. Several areas of ground-based research in astronomy where NASA support may be needed to fulfill its space research missions are discussed in more detail in the next Sections.

A. Development of Techniques and Instrumentation

Many technological problems will have to be solved to make effective use of future sophisticated spacecraft. An example of a research program designed to overcome such problems is the Kitt Peak work on metal mirrors, space optical systems, remote-controlled telescopes, and stabilization devices.

We need to identify other technical developments that will be needed to prepare future space instrumentation. The following techniques are among those requiring work: (i) sensitive image tubes with thousand-line resolution; (ii) ultraviolet reflection optics and gratings with good reflectivities; (iii) X-ray optics; (iv) closed-cycle cryostats; (v) large radio antennas which can be assembled, unfolded, or otherwise erected in space; (vi) millimeter-wave antennas suitable for space.

RECOMMENDATION: Proposals for development work in these areas should be supported.

B. Ground-Based Astronomy in Direct Support of NASA Missions

Areas of ground-based astronomy and laboratory work in direct support of NASA missions include solar patrols; observations and studies of the Moon; the optical, infrared, and radio study of planetary atmospheres and surfaces; the refinement and greater use of infrared techniques to observe extraterrestrial objects through atmospheric windows before recommending their indiscriminate use in space; expansion of precise star catalogues; meteor and meteorite studies; advanced methods in celestial mechanics; data handling and analysis; infrared, ultraviolet, and X-ray spectroscopy.

RECOMMENDATION: We note with strong approval the existing support of such work by NASA, and recommend that this policy be continued and expanded.

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Observations in the Spectral Region 1 μ to 1 cm. Observations in the infrared, submillimeter- and millimeter-wave regions offer the means to improve greatly our understanding of certain astronomical problems, and to study, perhaps, objects which are essentially unobservable in the presently used wavelength bands. Observations relevant to the former category of problems are those of infrared emission lines from abundant atoms and from molecular constituents such as CH, NH, and OH in the interstellar gas; molecular spectral lines in planetary atmospheres; infrared spectral lines of H_2O, CH_4, etc., in stars which are cool enough to allow such molecules to exist; and observation of stars of types M, S, R, and N, whose intrinsic luminosity can be accurately determined only from infrared data. In the latter category are observations of cool objects, such as very cool stars and protostars, whose temperatures probably lie in the range 100-1000°K, and perhaps cooled white dwarfs. Radiation from interstellar dust, whose temperature is of the order of 100°K, may also be observable, which would allow a great advance in knowledge of this important component of the galaxy. Much of the mass of the galaxy inferred from gravitational effects remains unaccounted for in direct observations (estimates run as high as 50%), and some of it may be found by infrared observations. It should also be possible to observe important regions whose visible light is obscured by dust, such as the nuclear regions and other distant parts of the Milky Way.

Until now, nevertheless, very few astronomical infrared observations have been made, principally because no detectors of sufficient sensitivity have been available. Those few past observations have been obtained usually with rather makeshift, relatively undeveloped instruments of low sensitivity compared to presently available instruments. Observational programs have been of short duration, and have been made from sites that probably do not offer the best available observing conditions. Several of the known atmospheric infrared windows have not been explored at all. Thus there is no broad background of experience on which to base conclusions as to the improvements to be gained from space observations.

Recently, much effort has been directed toward the development of detectors, and has resulted in impressive improvements in detector sensitivity. These include, for the far infrared, the doped germanium photoconductor such as the one being used by Murray at the California Institute of Technology and, particularly, the cooled doped germanium bolometer developed by Low at Texas Instruments. This latter device is very effective throughout the infrared and millimeter-wave regions, exceeding by a factor of about 10^3 the sensitivity of previous detectors (such as the Golay cell) for these wavelengths. In the millimeter wavelengths, superheterodyne radiometers using travelling-wave tubes as intermediate-frequency amplifiers appear promising. Many groups are now attempting to apply these various devices to astronomical use, including, at infrared wavelengths, the Lunar and Planetary Laboratory of the University of Arizona, the California Institute of Technology, and the University of California; and, at millimeter and submillimeter wavelengths, the Massachusetts Institute of Technology, the University of Texas, Georgia Institute of Technology, the Feather Ridge Observatory of Collins Radio Co., and the National Radio Astronomy Observatory. Some of these projects, including those at the California Institute of

Technology, the National Radio Astronomy Observatory, and the University of Arizona, will study observing conditions at high, dry sites. The University of California project will attempt infrared observations from a balloon at an altitude of about 80,000 feet. Thus it appears very likely that we will soon lessen our present ignorance regarding atmospheric impediments, detector sensitivity, and the types and value of astronomical data to be obtained at infrared, submillimeter, and millimeter wavelengths.

> FINDING AND RECOMMENDATION: We will soon have the data necessary to evaluate the place of infrared and short-wave radio observations in the space program. Accordingly, we recommend that until this information is available, infrared observations from satellites of celestial objects should be given a low priority.

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An area of particular interest is the detection of cool bodies in space, a potentially important subject of which we have very little knowledge at present. The invisible companion of the star Epsilon Aurigae is one of the few cases where we suspect the presence of one of these bodies. A survey-search for such bodies is at present a very time-consuming task, since no acceptable multi-picture-element or "image tube" device exists for the far infrared. Thus, a search for such bodies with present detectors will probably produce only a few results per unit observing time. This situation suggests that inexpensive methods should be applied on the ground. Furthermore, the cool bodies should radiate over a wide spectral region, allowing as effective a search in the atmospheric infrared windows as at the terrestrially inaccessible wavelengths.

> RECOMMENDATION: We recommend that observations aimed at the detection of cool bodies should be made initially from the ground.

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A field in which ground-based observations can never be entirely satisfactory is the study of infrared molecular spectra of planetary atmospheres. In such studies, molecules to which the greatest interest is attached, such as H_2O and CO_2, occur abundantly in the terrestrial atmosphere, and impress strong telluric lines on the planetary spectra. This often makes it difficult or impossible to measure the abundance in other atmospheres of molecules occurring in the terrestrial atmosphere.

A Stratoscope II balloon flight early in 1963 will carry instruments to obtain infrared spectra of Mars from an altitude of 80,000 feet. This experiment should show conclusively whether balloon altitudes are sufficient to overcome the interference from the telluric lines. If not, the important problem of planetary spectra will have to be attacked from observatories in space.

> FINDING: Plans for infrared planetary observations from space should await the results of the forthcoming balloon-borne experiments, except in those cases where planetary probes offer obvious advantages by their proximity to a planet.

Lunar and Planetary Observatories. The general question of the expansion of ground-based lunar and planetary observations, either by the enlargement of observatory schedules at existing observatories or by establishing more facilities, was not discussed during the Summer Study, since most specialists in this field were not able to participate.

> RECOMMENDATION: It is suggested that NASA convene a panel as soon as convenient to study the whole issue of planetary and lunar observatories. We suggest that Dr. John Hall be asked to serve as chairman of such a group and that five or six others be invited. Possible members are: J. Chamberlain, F.D. Drake, F.K. Edmondson, W.W. Kellogg, G.P. Kuiper, R. Leighton, N.U. Mayall, C. Mayer, G. Munch, G. Newkirk, W. Sinton, R. Wildt, and a representative of the National Science Foundation.

The Summer Study was asked by NASA and Air Force representatives to comment on a proposal that the Air Force build and NASA operate a planetary observatory (with potentialities for solar work) in Otero County, New Mexico. Sacramento Peak Observatory is also located in Otero County, and most planetary observing sites in the U.S. are now located in nearby areas. It is advantageous in observing planets to have observations spread in longitude to provide more continuous observation of the planetary atmospheric phenomena which develop on a time scale of 10 to 30 hours. In the particular case of Mars, which rotates at a rate nearly equal to that of the Earth, such a spread in longitude is necessary to give observations, within a few days, of all sides of the planet. Also, in ventures of this kind, the first order of business must be to choose a competent and interested scientist to help in selecting a site and to plan and direct the observational program. These considerations were embodied in the following recommendations.

> RECOMMENDATION: A second solar observatory in Otero County, New Mexico, seems ill-advised. A planetary or lunar observatory such as that contemplated by the Air Force can be very important, but finding an eminently qualified man to plan and direct it is essential. There should be great flexibility in the choice of the site, with the aim of achieving a better distribution in latitude and longitude with respect to existing observatories.

C. Ground-Based Solar Studies*

It has been recommended that NASA should support those areas of ground-based research in astronomy which are in direct support of NASA space activities. The following paragraphs discuss ground-based solar work which stands in such a relation to the space activities.

1. Observations Needed for Decisions. The S-17 Orbiting Solar Observatory is equipped with two methods of observation -- a raster scan of the whole Sun and a steady pointing at the center of the solar disk but allowing a coronagraph to scan,

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*This Section was prepared by a Subgroup on Ground-Based Solar Studies. See Appendix III for list of participants.

spirally, the outer corona. Two operating modes will be used: scanning for four orbits and pointing for one, or vice versa. The operating mode may be selected by radio command from the ground. The person making this selection needs to know the state of activity of both the solar disk and the corona in order to decide which mode will yield the most information.

The trend in the OSO and Advanced OSO series is towards more versatility and therefore towards more ground-based decisions. The need for good monitors of relevant solar parameters will thus increase during the next few years. Monitors in the wheel section of the S-17 type craft will probably not fill this need since the necessary angular resolution will be hard to obtain.

The only reasonably good sources of continuous information on solar activity at present are the ground-based H-α flare patrols and the solar radio-frequency patrols. The H-α patrols are largely photographic and so do not supply information on a short time schedule. The same is true for the radio patrols* in the U.S.: only the low-frequency spectrograph in Boulder gives immediate indications throughout the daylight hours. There are a number of single-frequency radio patrols around the Earth, but the results from these are difficult to interpret except for outstanding events.

For making decisions on the basis of the H-α features and active radio regions, where a time lag of 24 hours or more is allowable, the present patrols may be adequate somewhat more than half the time. For decisions which require faster response the optical patrols must be supplemented with visual observations or frequent and rapid film processing and inspection, and the radio spectrographs must have facsimile outputs and frequent inspection.

2. Flare Prediction. Major solar flares may be associated with the production of energetic particles in quantities sufficient to endanger astronauts in flight. At the moment the extent of this hazard is variously estimated to be non-existent, small, or very important. The following discussion assumes the hazard to be real and important.

If the hazard is real, ideally one must predict the occurrence of a particle-producing flare as far ahead in time as the longest mission planned for manned spacecraft. This requirement may be drastically reduced if shielding can be provided in the spacecraft behind which man can hide. Then a prediction or even a flare observation which can be relayed to the astronauts before the particles arrive would be sufficient. Since the early manned lunar flights will coincide with a period of high solar activity, it will not be possible to select periods with low probability of flare occurrence by selecting periods with no active regions on the Sun. Good flare predictions must therefore be based on a better understanding of the flare process than we now possess.

Since the time until manned lunar missions is short compared to the usual time scale for the development of understanding complicated phenomena like solar flares, more specific approaches to the flare-prediction problem are needed in

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*Includes swept-frequency receivers working (with some gaps) from 8 Mc/s to 4000 Mc/s and a solar-mapping antenna operating at 9.7-cm wavelength.

parallel with basic solar physics studies. So far, synoptic approaches have not produced usable predictions, and it therefore seems necessary to observe a wider range of flare characteristics to give the synoptic approach a greater chance for success. Such characteristics are magnetic fields in active regions, fine structure of Hα features, and ultraviolet and X-ray emission from active regions. The last of these, of course, requires satellite observations.

Ground-based flare patrols, either visual or photographic, are in operation about 95% of the time, so that few flares escape detection entirely. The high-quality photographic coverage of the sequential development of flares is, however, only about 50%. Yet it is such photographs that must be used in a synoptic study of the growth of an active region and the subsequent occurrence of a flare.

3. Observations and Studies Needed to Complement Observations in Space. Space equipment for observing the Sun is necessarily specialized and limited in its scope and cannot provide a broad observational context for interpreting a particular measurement. For example, the observation in a spacecraft of an increase in ultraviolet line and X-ray emission requires ground-based Hα heliograms, and measurements of radio emission and ionospheric effects before one can even begin to construct a model of the solar event that produced the increase. Ultimate success in understanding such events probably will require a knowledge of the magnetic fields in the active solar region and of the visible spectrum. Since the X rays originate in coronal regions, limb observations of enhanced coronal continua are also important. Moreover, since the energy supply for the flare must in some manner pass through the photosphere, observations of that region will probably be necessary.

More fundamentally, both ground-based and space measurements of solar phenomena can be interpreted only when we understand basic solar physics. Any program of solar studies must therefore expect much of its effort to go into basic research in the spectroscopic analysis of the solar atmosphere, including laboratory measurements of appropriate atomic parameters, basic work on the hydromagnetics of plasmas, and research on the production of energetic particles in the Sun. Work in this area is in progress at many institutions and with various types of support.

It is, of course, not possible to draw a sharp line separating those activities which support the space work from the rest of solar physics. Any area which lags in its development for lack of support or lack of interest and which, because of the lag, makes the planning or interpretation of the very expensive satellite and probe measurements difficult or impossible will seem to be an area which "directly supports the NASA activities."

4. Specific Recommendations

Hα Flare Patrols. There is much discussion of the Hα Flare Patrols flare process at present, with frequent emphasis on the possibility that the optical flare plays a role secondary to that of magnetic fields, energetic particles, or radio waves. However, the Hα photographs are still, and will likely remain, the basic observational information around which other kinds of measurements are arranged. The Hα pictures are capable of showing development of the flare simultaneously as a function of time and position -- an advantage which no other type of observation now possesses.

Many flares are not photographed at present. The coverage depends on time of day and season, the least coverage occurring during the hours 0000 to 0400 UT, and during the northern winter. The percentage coverage could be significantly increased by the encouragement of the following: (i) reactivation of the excellent Australian patrol; (ii) activation or reactivation of patrols in Peru, Hawaii, and Manila (all already under development); (iii) establishment of data exchange with the Purple Mountain Observatory in China.

As important as increasing the photographic coverage is the improvement of quality of flare photographs. This difficult field requires experiments by experienced observers with methods to improve seeing, explore off-line techniques, improve limb-disk comparisons, and so on.

> RECOMMENDATION: We recommend that NASA encourage the improvement of the international photographic flare patrol by supporting efforts to reduce the gaps in the present coverage and supporting efforts to improve the quality of the existing patrol stations.

Flares. The study of solar flares is of intrinsic interest for solar physics, but is also important for space activity because of the role of flares in changing the conditions of the interplanetary medium. This is true whether or not flare particles prove to be harmful.

> RECOMMENDATION: We recommend that NASA support proposals contributing to a broad study of the mechanism of solar flares.

Experimental work. One field that is clearly lagging at present is the laboratory effort to measure atomic parameters of astrophysical interest. Laboratory spectroscopic data, atomic cross-sections, and transition probabilities are all badly needed, especially those pertaining to the ultraviolet wavelengths.

> RECOMMENDATION: We recommend that NASA support proposals which include laboratory studies of astrophysical interest.

VIII. Administrative Matters

A. Data Distribution

Traditionally the independent investigator studying a scientific problem gathers his own data, reduces them to whatever form he requires, and draws his own conclusions -- or at least he has a large measure of control over this process. Ideally, he would be responsible only to himself, a condition closely approached in the university environment. This condition is modified to some extent where the investigator is under contract to study a particular problem. The circumstances of satellite research modify the conditions still further, especially for the investigator whose experiment is one of many incorporated into a combined payload. In the latter case, obviously the only practicable arrangement is the existing one, in which NASA undertakes to provide the following services for all participants: (i) tracking the satellite to furnish information on its position as a function of time; (ii) recording the telemetered data from the instruments on board, and sorting and decoding the taped data; (iii) supplying any or all of the foregoing information to the participant in the form most useful to him. Physical and economic limitations effectively rule out any other procedure. We are pleased to learn that NASA will furnish the data even in raw form just as they are telemetered, if the investigator wishes, and that NASA also finds it possible to display data in such a way as to afford "quick looks" to the investigator who so desires.

A question has often been raised concerning the release of experimental data from satellites to individuals other than the experimenter. It has been argued by some that, since NASA is a government agency and therefore a public institution supported by public funds, the satellite data are in effect public property and should be released as soon as possible to anyone who wants them, regardless of the interests of the investigator who conceived and designed the experiment, and who in many instances built it or at least supervised its final execution. This view is certainly much too extreme. This argument can be applied with equal logic to work done in any laboratory of any branch of the government, indeed even to that done at publicly supported universities or under grants from the National Science Foundation. The fact that satellite experiments are an order of magnitude more costly than most others and that they involve the efforts of large numbers of people makes them different only in degree, not in kind. We therefore reject this view.

At the other extreme, those hewing to the scientific tradition believe that the investigator should not be bound by any formal obligation, that he should be let entirely alone to use any or all of the data at his own chosen pace, and have the right to withhold publication of the data until he has no further use for them, if he sees fit. We feel that the proper view on the publication of data or its release to others than the experimenter lies somewhere between these two extremes, but much closer to the one just described.

> FINDING: We heartily approve the spirit of the NASA policy on the release and publication of satellite data. (See Appendix II). It protects the experimenter in his right to use his data ahead of all others for a reasonable length of time, but provides for eventual release in the event of unusual delays, with these provisions always being made in consultation with the experimenter. It may be that later experience will require modification of this policy in some minor respects, but it does not seem possible to improve on it now.

B. Sharing of Observing Time

Closely related to the question of data distribution is that of sharing data, or the sharing of observing time. NASA's plan for the use of observing time with the Goddard stellar ultraviolet spectrograph on the second OAO, in which it is proposed that astronomers at large be invited to submit requests for spectra of those particular stars that interest them, is excellent. Prime experimenters on other OAO's have offered to do the same thing to the extent that they can do so without interfering with their own programs. Some sort of policy is needed here to guide the prime experimenters and NASA in managing requests from the invited "guest observers" in an equitable way.

> FINDING: In arranging the observing program of an OAO whose time is to be shared in this way, we agree that the prime investigator should have the first priority; prime investigators on earlier or succeeding OAO's, second priority; and guest observers, lowest priority.

The highest priority for the prime investigator hardly requires further comment: in view of the exigencies of satellite astronomy, he must be given ample opportunity to carry out the program that he himself has in mind, obtaining and keeping however many data he believes that he can manage. If after the first few weeks or months of observation he has all or most of the data he wants, he might then share observing time with others.

Second priority should go to prime observers on other OAO's, if they want it, in order to allow them to secure the information necessary to improve their own experiments, or to make some observations if their own experiment failed on an earlier OAO. To a small degree, this makes up for lack of immediately available back-up equipment to take care of failures.

As to guest observers at large, it seems premature to set up a detailed plan now, or even to issue invitations. The detailed time-sharing plan will, in any case, differ somewhat from satellite to satellite, and must be worked out in conference among NASA, the prime experimenter, and the other investigators with experiments on the satellite. A certain precedent already exists for such arrangements in the guest observer policy at some of the conventional observatories; these examples may be useful as a guide.

> FINDING: It may be safely assumed, we believe, that those in control of the programming will avoid any tendency on the part of the group of active observers to become a closed shop against applicants new to the field, that each request will be judged on its merits, and that foreign astronomers will receive the same consideration that is given to American astronomers.

C. Space Projects Potentially Harmful to Astronomy

We are aware of the history of the West Ford project, and of the efforts of the Space Science Board to evaluate the possible effects of this project on astronomy. The Multiple Echo Balloon proposal was also known to many of the Summer Study participants. The small group of astronomers who have been following the progress of West Ford for the Space Science Board for the past two years has recently been charged by the Board with the wider responsibilities of identifying and evaluating space activities which might be damaging to other branches of scientific research.

One of the main problems in making the work of such a group effective is the difficulty of learning about and identifying potentially dangerous projects in time to influence the outcome. The group considered that it would be reasonable to ask NASA to arrange to monitor all its own space projects from a position within the NASA organization, so that potentially harmful operations might be identified early. Once identified, the Space Science Board Committee would, it is hoped, be competent and able to assess the potential damage which astronomy might suffer.

> RECOMMENDATIONS: We realize that there are many space activities outside the control of NASA, but we ask NASA to take the lead by watching its own projects for the protection of the astronomical future. We further urge that parallel actions be taken in the Department of Defense and the Atomic Energy Commission, and that over-all

responsibility for identifying harmful operations be placed in the Office of the National Aeronautics and Space Council. We recommend that NASA designate a person who would monitor NASA operations to identify those space projects which might have a detrimental effect on astronomical research. The designated person should maintain good liaison with the Space Science Board.

Appendix I

Report of The Radio Astronomy Subgroup

A. Meeting of June 28, 1962

The following persons attended the meeting of the Subgroup. The present report represents the results of this meeting, together with later additions and revisions emerging from discussions of the main Working Group on Astronomy.

J.W. Findlay, Chairman
H.G. Booker
B.F. Burke
D.D. Cudaback
A.J. Deutsch
F.D. Drake
F.K. Edmondson
J.W. Firor
G.H. Herbig
G.R. Marner
N.U. Mayall
L.C. Van Atta
E.R. Dyer, SSB Secretariat
K.W. Linnes - NASA Contributor
N.G. Roman - NASA Contributor
H. Spinrad - NASA Contributor

B. Discussion of Current Work in Space Vehicles

The current radio astronomy space program was discussed, with particular reference to the following work:

University of Michigan - F.T. Haddock: Sweep-frequency receiver in EGO.
Harvard College Observatory - A.E. Lilley: Multifrequency receiver (Air Force Vehicle).
British work - Cambridge University - F.G. Smith: Fixed frequency.
Canadian work - Sweep-frequency topside sounder.

The experiments being carried out are all good. The relatively small number of experiments reflects the fact that few proposals have been made to NASA.

C. Future Objectives in Radio Astronomy Space Research

The radiometer experiments to be carried on the Mariner approach to Venus were considered. Although these radiometer experiments appear well designed, it seems in retrospect that a ground-based experiment might have achieved an equivalent result. However, the group realized that there was no such suitable ground-based experiment proposed to NASA, and that a suitable vehicle-borne experiment was offered. The ground-based experiment would be a centimeter-wave variable-spacing interferometer, using dishes of about 85 feet diameter and the equipment would cost at least $3 million.

The future of millimeter- and submillimeter-wave radio astronomy was considered at some length, and the supporting research program of NASA in this field was reviewed. The NASA program, and in particular the contract work arranged on the development of millimeter-wave components, and test and calibration equipment was welcomed. All work in this area is still ground-based; but if the high promise of the helium-cooled germanium bolometer as a detector can be realized in a practical stable radiometer, the future may well be bright. Such a radiometer can be tested on the gound but eventually could be used on a space vehicle. For this purpose a light compact source of liquid He temperatures would be needed, and NASA was asked to watch for such developments, with the note that some military development has already taken place. It seems possible that such a radiometer, using a 30-foot dish (which seems technically feasible), may detect all the planets, secure data for the study of dark nebulae, and map perhaps 10-20 of the stronger nonthermal radio sources with an angular resolution of 30 seconds of arc. Equipment for space vehicles should only follow a careful ground-based program.

The experimental objectives of the follow-on and long-range programs of the "Thinking Document" and the techniques described in the paper by Lilley (WG-1/2 of June 26) were considered. The objectives, as stated in the "Thinking Document," embrace much of the future field of research for radio astronomy on the ground as well as in space. The three areas which can easily be distinguished are as follows.

1. The Long-Wavelength End of the Radio Spectrum (30 Meters to 3 kms Wavelength). As soon as instrumental techniques are available in space vehicles, many problems are open to attack. All will need space vehicles capable of carrying radiometers and long-wave directional antennas. Such antennas, as a first step, should have beam widths of about 5°. They should be capable of pointing to about 1/2° in direction, and methods of calibrating the gain and the pointing of the antenna are essential. If possible, the antennas should be able to make polarization measurements.

2. The Short-Wave End of the Spectrum (1 Millimeter to 10 Millimeters Wavelength). The space radio-astronomy experiments, particularly those described by Lilley, are all good, and all require radiometer and antenna developments. Again, good resolution and accurate calibration are requisites.

3. Ground-Based Radio Astronomy. Many of the objectives of the "Thinking Document" should be met by a continuation and extension of present ground-based radio astronomy. In deciding what research to support in this field, the Subgroup expressed the hope that NASA would be guided by the principles suggested by the whole Working Group in its recommendation on the support by NASA of ground-based astronomy.

There is still work to be done on the ground in developing techniques of radiometers and antennas, and many observations still are of discrete radio sources possible from the ground, despite the atmospheric limitations. This work should continue towards the shorter wavelengths until radio-astronomy observation becomes definitely limited by the atmosphere.

D. Radio Astronomy and Manned Space Flight

The Subgroup discussed possible radio-astronomy experiments that could be carried out on the surface of the Moon in the early stages (perhaps within the first ten missions) of the Apollo program. The general conclusion was that most space radio astronomy experiments would be better performed from orbiting space vehicles and that the early phases of Apollo did not seem to be particularly suitable for radio-astronomy observations. This view was adopted with some reluctance, since later phases of the development of the lunar mission might alter the prospects, but it was felt that the field of experimentation was, in the early phases, perhaps more valuable to geological and biological research than to radio astronomy.

E. Supporting Ground-based Research

The Subgroup agreed in hoping that more good experimenters would come forward to lead work in the design and engineering of good antenna systems, both for long and millimeter wavelengths, for use in space vehicles. The antennas must be capable of accurate calibration, both in pointing accuracy and in effective collecting area.

In addition to the items of ground-based research already treated, the need for ground-based radio observations of planets for the scientific mission of NASA was considered. Among the radio observations of the planets that are highly desirable are: microwave radio spectra of all planets, brightness distributions or radio diameters wherever possible, measurements of the polarization and intensity as a function of time of those planets possessing nonthermal radio emission, and millimeter and submillimeter spectral measurements designed to detect and measure molecular constituents of the atmosphere. All of the foregoing observations are very time-consuming, and require the full-time services of more than one large radio telescope. At the present time, planetary radio observations have been impeded by the lack of radio telescopes that can be fully devoted to such observations. In the likely event that a group or committee were to be formed to consider a ground-based planetary astronomical observatory, the Subgroup recommended that such a committee should consider carefully the need for the observatory to conduct radio-astronomical observations, and agreed to offer the name of F. D. Drake as a possible member of such a committee.

A brief review of the need for extending existing facilities for ground-based continuous monitoring of the Sun at several radio wavelengths was made. This led to the conclusion that the Subgroup under J. W. Firor should consider this question, extending its membership if necessary to do so.

F. Interference to Radio Astronomy by Other Space Activities

No further discussions took place, since it was agreed that the action already proposed by the full Working Group was satisfactory.

Appendix II

NASA Policy on Data

(Excerpts from Remarks by Dr. Homer E. Newell, Director of the Office of Space Sciences, NASA HQ. Briefings, June 20, 1962.)

Those scientists both inside and outside NASA who participate in the space science program are always interested in the policies concerning the use of the data obtained. Traditionally, a scientist conceives, prepares for, and carries out his experiment, analyzes the data, and publishes his results, assuming responsibility for what he has done. The question always arises as to whether the NASA policy on the use of data will support this traditional responsibility on the part of the scientists. I will read pertinent sections of the management instruction which NASA has issued on the use of data obtained from NASA-supported experiments.

The first section describes the purpose. This instruction defines the procedures to be followed in relations with individual scientists or groups participating in the NASA space sciences program with regard to release and publication of experimental data obtained in NASA sounding rocket satellites, space probes, or other spacecraft operations.

Another section describes the scope. First, scientists as a group feel a responsibility for making the results of their efforts known to the worldwide scientific community. Scientists have been the most vigorous proponents of free dissemination of the knowledge gained from their investigations and in general can be expected to release experimental scientific information rapidly. Secondly, in the usual case, an agreement that will assign the responsibility for the analysis and publication of the data to the scientist will exist between NASA and the scientist. In the period before the publication of the results of the experiments it may happen that another scientist may wish to have access to the experimental data in its preliminary form. This is generally arranged informally by a request made directly to the experimenter. Next, occasions may arise when it will be necessary for NASA to release the results of experimental investigations made under its sponsorship without waiting for publication by the cognizant scientist. It is expected that this will happen only when the responsible scientist is unable to meet his commitments within a reasonable time. Next, in many instances the data acquired from space vehicles, such as the observations made in astronomy, will have a cumulative value. Whenever this is the case, it is desirable that such data be made available for the general use of the scientific community. Next, the original data received from a satellite or space probe is meaningless until the various channels are demultiplexed, the calibration of the instrumentation is introduced, and the data is arranged in chronological order. Provisions for these operations must be included in the arrangements made for supporting the applicable experiments.

The final section states the policy. First, the original data recordings received from a satellite or probe shall be stored at the cognizant NASA field installation. Second, secondary records containing reduced data shall be prepared under the cognizance of the responsible NASA field installation. On occasion this operation may take place at the contractor's or experimenter's establishment. In the secondary records, all extraneous material and all confusion of data will be removed to the fullest extent possible. The records will be prepared by the NASA field installation or contractor with the cooperation of the experimenter so as to satisfy the needs of the experimeter and NASA for reduced data. Third, the experimenter shall be given whatever original or reduced data from his experiment his agreement with NASA calls for as soon as possible after they are received. The cognizant NASA field installation will retain copies of the reduced data furnished the experimenter. The experimenter will have sole use of the reduced data for study for a period previously decided upon between him and the Office of Space Sciences, NASA Headquarters. Fourth, no general fixed times shall be set for the period of the exclusive use of this data by experimenters. Each case shall be considered individually and no general rules shall be established. Next, if during a period in which the experimenter has not recorded the results of his experiment, information on the progress of the experiment or portions of the reduced data are requested by another scientist for designing a space science experiment or for the preparation of a technical paper, informal arrangements for the release of the needed data will be made by the experimenter concerned. Both the cognizant NASA field installation and the Office of Space Sciences, NASA Headquarters, will be informed of these arrangements. Next, at the end of the agreed-upon period during which the experimenter will have the opportunity to analyze and report on the reduced data, the reduced data may be released for general use if such a course seems best for developing space science. An extension of the period of sole use requested by the experimenter will require the approval of the Office of Space Sciences, NASA Headquarters.

Appendix III

Principal participants in Working Group on Astronomy included the following:

L. Goldberg, Chairman
A. J. Deutsch, Vice Chairman
B. F. Burke
F. D. Drake
F. K. Edmondson
V. R. Eshleman
J. W. Evans
J. W. Findlay
J. W. Firor
G. H. Herbig
A. E. Lilley
G. R. Marner
N. U. Mayall
M. Schwarzschild
L. Spitzer
R. Tousey
F. L. Whipple

E. R. Dyer - SSB Secretariat

D. Cudaback)
G. Kessler) Rapporteurs

NASA Contributors

A. Cameron
R. Jones
J. Kupperian
J. Lindsay
L. Meredith
J. Naugle
N. Roman
C. Sonett
H. Spinrad (JPL)

USAF Contributor

G. Colchagoff

Participants - Subgroup on Radio Astronomy

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H. G. Booker
B. F. Burke
A. J. Deutsch
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J. W. Firor
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A. E. Lilley
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R. T. Hansen
O. C. Mohler
A. K. Pierce
Helen Dodson Prince

Chapter Three

CELESTIAL MECHANICS*

I. Introduction

NASA's space science program, aimed at obtaining a better description of space and better values of the physical constants involved in this description, must necessarily tap the resources of celestial mechanics. Celestial mechanics finds an essential applied role in the determination of orbits and trajectories and through that role, exerts critical influence on space missions in determining the feasibility of a given mission objective and in setting the primary systems requirements, for example in guidance and propulsion. Very appropriately, therefore, celestial mechanics, particularly its applied aspects, has received important and growing emphasis in NASA's programs. This report reviews primary uncertainties and areas of major scientific interest and makes appropriate recommendations aimed at improving scientific knowledge in the field through space experiments and ground-based research. A more detailed discussion of lunar satellites and solar parallax appears in the appendixes of this report.

II. Summary Reviews and Recommendations

A. Experiments

Space experiments which are pertinent to celestial mechanics and which are to be performed by satellites and probes are listed below.

The two major problems to be investigated using artificial earth satellites are related to the drag and to the higher-order gravitational harmonics. It has been established by post-flight analysis that a correlation exists between atmospheric drag and solar radiation. The solar radiation must be monitored at several frequencies during periods when satellites subjected to appreciable atmospheric drag are under sufficiently extensive observation to allow the correlations to be analyzed and resolved.

Much new information concerning the harmonics of higher order and degree (the "J coefficients," both zonal and tesseral) has already been obtained from the analysis of observations since the first artificial satellite was launched; but a desire to achieve greater accuracy will continue to exist. Satellites of high density (i.e., with low area/mass ratio), especially of the flashing-light type, are particularly important for this purpose. For such satellites the effects of both air drag

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*See Appendix VII for list of participants in Working Group on Celestial Mechanics.

and radiation pressure are reduced, so that radiation monitoring is less important. Flashing-light satellites give the great advantage of permitting precise optical observations throughout the night, a considerable advantage over twilight observations possible with solar-illuminated satellites. An alternative approach appears to be that of launching satellites in special orbits so that certain resonance properties of the motions can be enhanced and utilized. An example of this type is a satellite with a 24-hour orbit in the equatorial plane.

> RECOMMENDATION: Consideration should be given in the Earth Satellite program to experiments and satellites designed to improve our knowledge of the Earth's gravitational field.
>
> This represents an area in which the interests of celestial mechanics and geodesy overlap. Representatives of both disciplines should participate in the planning of experiments and in the interpretation of observational data.
>
> Problems connected with time should be investigated with reference to experiments outlined in the ''NASA Long Range Sciences Thinking Document,'' dated May, 1962, pp. 1-25.
>
> RECOMMENDATION: Artificial lunar satellites should be established with the principal purpose of determining the gravitational field of the Moon. It is recommended that such experiments, despite the engineering difficulties, be given much higher priority than is received, for instance, by those experiments which are aimed at clarifying the Moon's magnetic field. Consideration should also be given to the landing of a three-axis telescope system on the surface of the Moon in order to determine its physical librations.

While it is recognized that a NASA working group on selenodesy is considering this question (see Appendix I), the importance of this project is to be emphasized. Precise orbit calculations cannot be performed for vehicles in the vicinity of the Moon without reliable and detailed information on the Moon's gravitational field, with obvious serious implications for the presently contemplated Apollo mission profile.

> RECOMMENDATION: Artificial satellite(s) around Venus should be established to obtain information on the gravitational field of that planet.

Neither a fly-by trajectory nor a capsule which enters the atmosphere of Venus and impacts on the surface will give the gravitational information needed. Satellites around Venus will furnish information concerning the gravitational field of the planet and the orientation of the equator if the rates of motion of the pericenters and of the poles of the orbit planes are observed. This should be a guide line for designing the equipment.

Note: The rates of motion of the pericenter and the orbital poles will reveal also the order of magnitude of the planet's period of rotation. A transponder on the planet's surface will not only furnish a more direct evaluation of the period of rotation, but also aid in determining the astronomical unit.

RECOMMENDATION: Artificial planets with one-year periods should be established. (These can be space probes which also perform fly-by experiments near Venus.)

The transponder in the artificial planet with one-year period will assure communication and range information for several complete orbits and will lead to a better determination of the astronomical unit. A detailed discussion of the solar parallax appears in Appendix VI.

RECOMMENDATION: Further development of the Azusa-type system should be considered, as well as the establishment of additional sites, to improve the tracking of deep space probes.

RECOMMENDATION: Libration point satellites (placed at the triangular libration points in the Earth-Moon system) should be given consideration.

The observed motion of such satellites, when compared to analytical results, will not only be an excellent test of the theory, but will increase our knowledge of the mass of the Moon. The degree of stability of such satellites, which can be studied effectively only by analytical methods, may be related to cosmogonical questions. Exploration of these points in space may also result in the acquisition of dustlike material of lunar origin without landing on the Moon. (See Chapter Four, Appendix I, Section C.2.)

B. Celestial Mechanics: Basic Problems

The previously listed experimental undertakings in the field of celestial mechanics require a broad analytical study for the interpretation of the results. Experiments and space flights conducted for purposes other than celestial mechanics likewise require precise orbit determination, optimum trajectories (optimized with respect to the specific experiments to be performed), etc., all of which necessitates the broadening of the present activities of NASA in the field of celestial mechanics.

It is to be recognized that high-speed digital computers do perform orbit computations if the initial conditions are given and if the method of computation has been chosen. Both of these require analytical developments. Furthermore, digital computers cannot establish the set of available trajectories (the totality of motions) with the generality of an analytical representation of a family of trajectories. Without this, optimizations pertinent to the feasibility of the whole mission cannot be performed on a sound basis. Therefore, the possibility of performing a particular space experiment frequently depends on analytical studies in celestial mechanics.

Programs have recently been developed for high-speed electronic computers to perform algebraic manipulations and expansions of trigonometric series. These new techniques present possibilities for attacking problems which at present are not manageable. Moreover, the researcher will be able to concentrate on the essentials of his problem, being freed from the necessity of devoting much of his time to lengthy algebraic developments by hand. The new approach will therefore lead to the opening of a new era of research in celestial mechanics.

The following examples constitute a partial list of problems in present day celestial mechanics: convergence of solutions and boundedness of orbits, artificial satellite motion with drag, simultaneous occurrence of two small divisors, capture, stability, re-entry window, and the two-body problem in general relativity.

Some of these problems are obviously relevant to the NASA program, while for other problems, no such immediate connection is obvious. It would be shortsighted, however, to exclude from NASA support any aspect of research in celestial mechanics merely because no application to present or contemplated space efforts is evident.

> RECOMMENDATION: In view of the general scientific interest and in view of its great influence on NASA engineering decisions, analytical research in celestial mechanics should be given appropriate emphasis and support.

> FINDING: The accuracy with which many of the constants of the solar system can be determined from observations is ultimately limited by the accuracy of the star catalogue positions that serve as reference points in the sky. At the present time a clear-cut need is indicated for an effort toward improving stellar positions, both for individual stars and with respect to the system of reference. This applies especially to the southern celestial hemisphere.

Appendix I

Preliminary Recommendations of the Working Group on Selenodesy*

Introduction

The determination of the distribution of mass within the Moon is central to any discussion of the origin of the Earth-Moon system. The astronomically observed deviations of the figure of the Moon from that of a hydrostatic body suggest that the Moon is at present a cold, strong body capable of supporting stress differences as large as those supported within the Earth. If the Moon is indeed cold, and if the Moon is of uniform or nearly uniform density, then the composition of the Moon must differ from the mean composition of the Earth. Alternatively, the Moon may have undergone chemical differentiation similar in character to that experienced by the Earth. In this case, the mass would be concentrated toward the center of the Moon and the total chemical composition of the Moon could approximate that of the outer portions of the Earth. The distribution of mass within the Moon and possible irregularities in this distribution are closely linked with questions of the seismic activity of the Moon, the radioactivity of the Moon, and the character of the lunar surface; all of which questions are integral parts of the lunar program.

The ratio of the mass of the Earth to that of the Moon is uncertain by about one part in 800. This ratio enters into the general system of astronomical constants; a closer determination is required for an improvement of the fundamental constants.

A lunar orbiter would be particularly useful in fundamental experiments on gravitation. Two classes of questions are of particular interest: (1) The detection of gravitational waves; (2) The determination of a possible secular change in the strength of gravitation.

In summary, a lunar orbiter can yield fundamental information on:

1. Planetary properties of the Moon
2. Fundamental astronomical constants
3. Nature of gravity.

---

*The Working Group on Selenodesy named in the title was a special group convened by the Lunar Program Office of the NASA Office of Space Science in the spring of 1962, and not one associated with the Summer Study. Its chairman was Gordon F.J. MacDonald. This interim report is subject to further modification before submission to NASA; it is appended here by several members of the Celestial Mechanics Working Group of the Summer Study who are also members of the NASA Working Group of Selenodesy because of summaries with their findings on the Moon's interior and figure.

Recommendations

It is clear that the positional data for a lunar satellite are essential to the interpretation of any experiment carried aboard. Because of this, the fundamental importance of accurate orbital requirements for the scientific problems noted above may be overlooked.

We recommend that the selenodetic experiment be considered an integral part of any orbiter. While other requirements may dictate the detailed orbit, fundamental information regarding the Moon's gravitational field can still be extracted provided it is clearly recognized that an important mission of any lunar orbiter is the determination of the gravitational field.

If the stress differences in the Moon are of the same order as they are in the Earth, then something like 25 terms for the spherical harmonic expansion of the gravitational field should cause perturbations of a few hundred meters amplitude. Kaula has carried out calculations on close lunar orbiters (perigee height, 100 km) with varying eccentricities and inclinations. He finds the effect of the terms in the harmonic expansion of the lunar gravitational field do not increase appreciably with the increase in eccentricity from 0.01 to 0.15. The effects do increase appreciably with increase in inclination from 11.5° to 69°. The perturbation of the perigee height by the Sun and Earth was less than 47 km in all cases considered. The synchronous rotation and revolution of the Moon do not give rise to any resonances for lunar orbiters of less than 2 or 3 lunar radii in semi-major axis -- i.e., far enough out that the Sun and Earth effects are dominant.

We recommend an orbit of moderate eccentricity of 0.1 to 0.15 an appreciable inclination, about 60°, and a perigee of 100 km or higher. Additional orbits at 0°, 30°, 90°, inclination are needed to sort out different effects.

We have not received definitive information from JPL regarding current tracking capability. The precision of radar range data and Doppler data is very good and has a potential of yielding very precise orbital data. Preliminary estimates by Lorell indicate that at the present time range rate data can be obtained with a precision of about 0.03 m/s (at one sample per minute — equivalent uncorrelated). It is anticipated that this number may be reduced to 0.01 m/s. Radar range could be available to 100 meter precision and eventually to 10 meters. Lorell concludes that it is reasonable to discuss orbit position and velocity data to within 3000 meters and 1 m/s respectively, based on a short tracking arc of the order of one hour long. It can be seen that on a longer arc results may be somewhat better. An improvement of the coefficients of the zonal spherical harmonic potential will result if the selenocentric instantaneous orbital elements are determined with an accuracy of 10^{-3} during three months. The coefficients of the tesseral harmonics can also be obtained with this accuracy.

The relativistic experiments require both a longer period of observation (at least a year), and a higher precision. The test of the secular change in the gravitational constants require knowing the period to an accuracy of a few parts in 10^{-10}.

We recommend that JPL prepare a summary study of the tracking accuracy expected in the early lunar orbiters and an estimate of the foreseeable improvement over the next few years.

This would be of particular value to the Working Group if it could be in the hands of the committee members prior to the next meeting which will be early in the fall of 1962.

Because of the critical need of accuracy both for experiments on the interior of the Moon and the relativistic experiments,

> We recommend a thorough study of possible advanced tracking techniques. In particular, the use of light masers and a satellite with a corner-cube optical reflector holds great possibilities.

The experiments discussed above demand a relatively long lifetime for the lunar orbiter, some months or greater.

> We recommend that serious consideration be given to the possibility of extending the lifetime of the orbiter beyond the lifetime required for its initial mission. Provision should be made for as long a lifetime as possible of the unoriented vehicle.

While valuable information in the area of selenodesy can be obtained from a vehicle whose primary purpose is photography, a vehicle whose sole purpose is examination of the gravitational field in the vicinity of the Moon could yield data of inestimable value to the scientific community.

> We, therefore, recommend that serious consideration should be given to a separate gravitational experiment in the lunar orbiter series.

Appendix II

The Mass of the Moon

Prepared by D. Brouwer and G. M. Clemence

The only available observational method for obtaining the mass of the Moon is that of deriving the lunar inequality L from displacements of a body in the solar system caused by the motion of the center of the Earth about the center of mass of the Earth-Moon system. Modern determinations have used close approaches of Eros to the Earth. The expression of the lunar inequality is

$$L = \frac{\mu}{1+\mu} \frac{\pi_\odot}{\sin \pi_{☾}}$$

in which

μ = the mass ratio Moon/Earth
$\pi_\odot$ = the constant of the solar parallax
$\pi_{☾}$ = the constant of the lunar parallax

The Eros opposition 1930/31 yielded for L by two different methods of treatment,

L = 6″.4378 (Spencer Jones)
L = 6″.4429 (Delano)

From a weaker solution Rabe obtained L = 6″.4356
From the 1900/01 opposition Hinks obtained L = 6″.4305

At the present time two different values of the solar parallax merit particular attention

$\pi_\odot$ = 8″.7984 (Rabe - dynamic method)
$\pi_\odot$ = 8″.7941 (JPL and Lincoln Lab. - radar)

These give for μ^{-1}

$\pi_\odot$ \ L:	6″.4378	6″.4429
8″.7984	81.367	81.302
8″.7941	81.326	81.261

The range of 0.01 among these values is probably a realistic indication of the present uncertainty in the Moon's mass. The choice of

$$\mu^{-1} = 81.30 \pm 0.10 \text{ (standard error)}$$

is probably as good as any that can be made at the present time.

Appendix III

Moments of Inertia of the Moon

Prepared by D. Brouwer and G.M. Clemence

The Moon has three unequal principal moments of inertia;

A - approximately about the line joining the centers of the Earth and the Moon,

B - about the axis in the Moon's equatorial plane perpendicular to the A axis,

C - about the Moon's axis of rotation.

The fact that the Moon's physical libration is small leads to the inequalities

$$A<B<C$$

The ratios α, β, γ are defined as

$$\alpha = \frac{C - B}{C},$$

$$\beta = \frac{C - A}{C},$$

$$\gamma = \frac{B - A}{C}.$$

(This is the definition used by Jeffreys. Other authors use $\alpha = (C-B)/A$ $\beta = (C-A/B$, $\gamma = (B-A)/C$, but the distinction is insignificant.)
These quantities are obviously related by

$$\alpha - \beta + \gamma = 0.$$

The value of β is reasonable well determined. Jeffreys' latest value is

$$\beta = 0.000\ 6294 \pm 15 \text{ (standard error)}.$$

The ratio

$$f = \frac{\alpha}{\beta}$$

can be obtained from a study of the physical libration of the Moon. The theory of the forced libration in longitude shows that for $f = 0.662$ resonance arises, and that it is difficult to distinguish between values of $\underline{f}$ a short distance on either side of the critical value. This was established especially by S. T. Habibullin (Bull. Inst. Theor.

Astr. Leningrad 6, 255 (1955)) and by G. Schrutka-Rechtenstamm (Sitz.-Ber. Oesterr. Akad. d. Wiss., 164, 323 (1955)). Both authors favor a value for f near 0.61. The corresponding value on the other side of the critical value would be $f = 0.71$, which was generally favored before this recent work.

The expression of the gravitational potential of the Moon requires the values of

$$(\alpha, \beta, \gamma) . \frac{C}{M R_{☾}^2}$$

in which $R_{☾}$ is the Moon's radius. For a homogeneous body the ratio C/MR^2 would be 0.40. Jeffreys puts

$$\frac{C}{M R^2} = 0.3970 \pm 0.0007$$

(The Earth, Third Edition, p. 149). This value is conventional assuming a homogeneous Moon. Comparison of observed and theoretical values of mean motion of node and perigee yields 0.56.

With this value the ratios become:

	$\frac{C-B}{MR_{☾}^2}$	$\frac{C-A}{MR_{☾}^2}$	$\frac{B-A}{MR_{☾}^2}$
$f = 0.60$	0.000 1497	0.000 2495	0.000 0998
$f = 0.71$	0.000 1772	0.000 2495	0.000 0723

The Moon's radius is usually taken to be

$R_{☾} = 0.2725$ x the Earth's equatorial radius or 1738 km

Note: A useful collection of references on the lunar libration problem is contained in Physics and Astronomy of the Moon, edited by Z. Kopal, Chapter 2, by K. Koziel, Libration of the Moon (Academic Press, 1962).

Appendix IV

Effect of Control Jets on Lunar Orbiter

Prepared by J. Lorell

The control system contains six jets in three opposing pairs. Each jet produces a thrust of the order of .0004 lbs. For normal operation, these jets are used intermittently, and in essentially alternately opposing directions so that the resultant force over a cycle should be zero. However, misalignments may occur, the magnitude of which we can pessimistically estimate to be about 0.1% of the total thrust. Thus, we can look at the following two situations.

1. A malfunction causing three jets to continuously thrust in one direction, with net thrust 1.2×10^{-3} lbs. With a spacecraft weight of 1000 lbs., this yields an acceleration of 3.6×10^{-5} ft/sec^2.

2. Normal operation with three jets malaligned to give uniform side thrust of 10^{-6} lbs. The corresponding acceleration is 3×10^{-8} ft/sec^2.

Case 1 would probably lead to an uncontrolled spin, causing a mission failure, and therefore should not be considered for our purposes. We will, therefore, use Case 2 to compare with the magnitude of other forces acting on the spacecraft.

The following table compares the magnitude of forces due to: (i) radiation pressure (photon flux at 1 A. U. from Sun); (ii) the Moon's gravity (at the lunar surface); (iii) the second harmonic J in the Moon's gravity potential; and (iv) the Earth, E, at Moon's distance.

Force	Magnitude of Acceleration (ft/sec^2)
Jet Misalignment	3×10^{-8}
Radiation Pressure	1×10^{-7}
Moon's Surface Gravity	6
Second Harmonic, Moon	1.8×10^{-3}
Earth	2×10^{-4}

Thus, it appears that the control jet force is comparable to that due to radiation pressure, and is at least 4 orders of magnitude smaller than the force due to the asphericity of the Moon's gravity field.

Appendix V

Preliminary Report on the Use of Lunar Satellites for Fundamental Experiments on Gravitation

Prepared by R. H. Dicke

Nature of the problem: There are two types of fundamental questions which could be answered by lunar orbiters of sufficient precision. One class concerns gravitational waves. If there were intense gravitational waves, essentially primordial waves, remnants of an original cataclysmic high-density state of the universe, such waves could disturb planetary and satellite orbits, causing random, apparently unexplained, fluctuations. In this same class of effects falls the long-range scalar wave associated with the scalar field which may be of significance to cosmology. While variations of the order of 10^{-10}/year is all that would be expected for a smooth energy distribution, the distribution may not be smooth and effects of the order of 10^{-8}/year may be considered possible.

The second class of experiment concerns methods of looking for the secular decrease in the strength of gravitation, a decrease predicted by several cosmologies (Dirac (1937), Jordan (1955), Brans and Dicke (1961)). A slow decrease in the gravitational constant would result in a decrease in the period of a lunar satellite. The fractional rate of decrease of G expected from the Brans-Dicke theory falls in the range $1 - 3 \times 10^{-11}$ per year.

There are several advantages in using a lunar satellite rather than an Earth satellite for purposes of these fundamental experiments:

a) Low gas damping because of the lack of a lunar atmosphere.

b) Low ionic drag because of the presumed absence of a Van Allen belt about the Moon.

c) Although the orbit of a lunar satellite would be strongly perturbed by the Earth, and by the Moon's figure, these perturbations should have only slight effect on the satellite period. Here the low rotation rate of the Moon is an advantage.

d) Precise angle and/or distance measurements on such a satellite would be capable of giving an improved orbit for the Moon which is less affected by drag than the satellite.

e) It should be emphasized in this connection that the burden placed on data from a lunar satellite for purposes of selenodesy as well as for these fundamental experiments would be much reduced if one or more optical corner reflectors were placed on the Moon's surface for use with improved pulsed optical masers, or alternately if radar transponders were landed on the Moon.

Among disadvantages of a lunar satellite should be mentioned the longer period because of the lower density of the Moon and the much lower velocity which makes precision Doppler and distance measurements more difficult and makes the satellite more easily perturbed by extraneous influences.

It would appear that one of the basic radar tools available for making precision orbit measurements is the Doppler radar. If it be presumed that Doppler velocities can be determined to ± 10 cm/sec, this would enable the phase of the orbit to be determined with an accuracy of 0.1 second of time, not accurate enough to be of any real interest for the problem of the secular variation, as a year's observations would yield a fractional accuracy of the period of 1×10^{-10} to 3×10^{-9} depending upon the extent to which the errors can be treated as random. However, if errors are random, an accuracy of 10^{-10} would be of considerable interest for the gravitational-wave problem.

Radar ranging with a sophisticated coherence radar should be capable of giving an accuracy of ± 1 meter in range or better. (See later the statement about the significance of such an accuracy.) This would make it possible to determine the anomaly (phase) of a satellite (orbit radius 2000 km) with an accuracy of 5×10^{-7} radian. One year's observations could then give a period to an accuracy of 2×10^{-10} to 10^{-11}, depending upon randomness in error. These values would be of considerable interest.

Because of the desirability of being able to make orbit measurements over extended periods of time, consideration should be given to the design of a dense, passive satellite in the form of a high-density corner-cube optical reflector. Such a device should ride piggy-back on another vehicle with radar transponder equipment and should be pushed gently off, only after an accurate orbit is obtained from radar measurements. This should make possible the use of light-maser pulses thereafter, in fact even before release of the reflector.

Light masers for this type of work are believed to have an important future, and plans must be made now if the reflectors are to be in the sky when they are needed. While masers of the type needed are not currently available, they will almost certainly be ready in one or two years. With suitable modulation technique applied to the maser, distance measurements with an accuracy better than 1 meter should be possible. It should be emphasized in quoting these accuracies that one does not mean an absolute distance to this accuracy. The velocity of light is not that well known. What is meant is that a change in distance by this amount would be measurable. It is interesting that a relatively small optical reflector would be adequate for use with an intense maser. A reflector 20 - 30 cms on a side is large enough if a large telescope is available for tracking.

One of the questions involved in the accuracy of the radar range is the effect of plasma density on radar range. The increase in wave velocity and decrease in group velocity associated with plasma vary inversely as the square of the frequency. Hence, a large retardation at 100 Mc/s is rather small at 1000 Mc/s. Nonetheless, a rough estimate places the radar delay for this higher frequency at 3 - 10 meters.

It is clearly big enough to warrant close attention. It is fortunate that this uncertainty can be eliminated by the simultaneous use of two different frequencies. Also, even higher frequencies than 1000 Mc/s would be desirable.

Appendix VI

The Value of the Solar Parallax

Prepared by G. M. Clemence and D. Brouwer

The unit of distance, conventionally used by astronomers wherever interplanetary distances are involved, is the astronomical unit, which is so defined that the mean angular motion of a planet of negligible mass, moving in the gravitational field of the Sun alone, would be 0.017 202 09895 radian per day precisely. As a consequence of the definition of the astronomical unit, the mean distance of the Earth from the Sun is 1.0000 00236 astronomical units. The theories of the motions of the planets give the distance between any two planets at any time, expressed in astronomical units, with a precision of six or seven significant figures if the planets have been adequately observed, which is the case for the eight principal planets, and a number of minor planets.

Planetary distances may be translated from astronomical units into kilometers if any single distance at a single instant of time is known in kilometers.

A fundamental constant of astronomy is the solar parallax, defined as the angle subtended by the Earth's equatorial radius at a distance of one astronomical unit. It is known to three significant figures (1962). From the solar parallax, together with the value of the equatorial radius in kilometers (known to at least five significant figures) the number of kilometers in the astronomical unit is easily calculated.

In recent years it has become possible to measure interplanetary distances directly by means of radar echoes. From such measurements the number of kilometers in the astronomical unit is first inferred, and then the solar parallax may be calculated.

Thus, from the dynamical point of view, the measurement of interplanetary distances by means of radar is equivalent to the determination of the solar parallax; either may be obtained from the other (to at least five significant figures). Of course, with the radar technique it is also necessary to know the velocity of the signals (which is supposed known to at least six significant figures) and the radius of the planet from which the echoes come (which in the experiments so far made seems to be adequately known.)

The value of the solar parallax has been intensively investigated by astronomers since 1672. A variety of techniques has been employed. In Table I is a summary of results compiled recently by G. de Vaucouleurs. One astonishing thing is the close agreement among the five classes of determinations. The agreement is fortuitous, at least in part. For example, the trigonometric result is the mean of six determinations, obtained by combining with weights inversely proportional to the probable errors. If the weights had been taken inversely as the square of the errors, the mean would have been 8".7920 instead of 8".7986. However this may be, the weight of the dynamical determination B is so great that the general mean is bound to be close to 8".798.

A summary of the radar results, taken mostly from a recent report from the Lincoln Laboratory in the Astronomical Journal, Vol. 67, page 181 (1962), is given in Table II. Two immediate impressions from these independent determinations are their consistency with each other, and the contradiction with the optical results. Details given by the authors furnish much additional information that is not shown in the Tables.

1. Actually the radar determinations are based not only on measured distances but also (at least at the Jet Propulsion Laboratory) on measured relative velocity.

2. Results obtained with the use of widely different wavelengths are in good agreement.

3. One of us (Clemence) has examined possible relativity effects, not taken into account by the authors, and has found them to be negligible.

4. It appeared that with the use of the ephemerides of the Earth and Venus published in the American Ephemeris (that is, Newcomb's orbits), the results derived showed a systematic trend during the two-month period. By introducing the corrections obtained by R. L. Duncombe from optical observations 1750-1949 considerable improvement was obtained, as is illustrated in Table III, which shows results recently published in the Astronomical Journal from the Jet Propulsion Laboratory. It should be possible to utilize the radar observations in combination with the optical observations to improve the values of the orbital constants used in constructing the ephemerides, and in that way to arrive at a more precise result. This undertaking, which is of considerable magnitude, is in progress at the Jet Propulsion Laboratory. Perhaps it can succeed only after further conjunctions of Venus have been observed, as is stated in the JPL article. The present position may be summarized as follows:

The optical result B has been the subject of frequent discussions among several of us (especially Brouwer and Clemence). No significant flaw in it has been discovered. Moreover, it is confirmed by earlier determinations by the same method. If the radar measurements had never been made, it would deserve complete confidence.

The radar results have been extensively examined by thoroughly competent persons (including ourselves), and no significant flaw has been discovered. If the optical result B had not existed, they would deserve complete confidence. The contradiction between the optical result B and the radar results is almost ten times as large as its probable error, which shows that at least one of the determinations is affected by a systematic error, of a type that cannot be discovered by statistical treatment of the observations. Therefore, it is very important that the solar parallax should be determined with modern precision by independent methods.

One adequate method consists of radar observations of a planet through several complete revolutions. In our opinion the easiest way to accomplish it is with an artificial planet in an orbit resembling the Earth's orbit, equipped with a transponder for the purpose of amplifying the echoes.

In stating our conclusion we are not disparaging the importance of a method proposed by A. E. Lilley of the Harvard College Observatory, consisting of observations of the Doppler shift in the 21-cm line produced in the spectra of radio stars by intervening hydrogen gas. In our opinion this should also be carried out.

TABLE I

Summary of Optical Determinations

	$\pi_{\circ}$	p. e.	A(km)		wt
A. Trigonometric methods............	8″.7986	±0″.0015	149,523,000	±25000	13.3
B. Mass of Earth + Moon (Eros)	8.7983$_2$	3$_6$	149,528,000	± 6100	55.4
C. Parallactic Inequality.	8.7967	18	149,555,000	±31000	11.1
F. Constant of Aberration (geometric).....	8.7979	18	149,535,000	±31000	11.1
G. Constant of Aberration (spectroscopic) .	8.7970	22	149,500,000	±37000	9.1
Weighted Mean.......	8″.7980	±0″.0002$_7$	149,533,000	± 4600	100.0 (total weight)

R = Earth's equatorial radius = 6378.165 km.

TABLE II

1961 Venus Radar Determinations

	Frequency Mc/s	A				wt
Millstone (MIT)....	440	149,597,850	± 440	8″.794184	±0.000024	30.6
Goldstone (JPL)....	2388	149,598,850	250	8.794125	.000015	49.1
Jodrell Bank	408	149,601,000	5000	8.792999	.00029	2.5
Moorestown (RCA) .	---	149,596,000	(5000)	8.794293	.00029	2.5
U.S.S.R...........	700	---	800	8.794081	.000047	15.3
Mean		149,598,630	± 180	8″.794138	0.000011	100.0

TABLE III

Summary of JPL Results

	Length of the astronomical unit, minus 149,000,000 (km)	
	Reduced with Newcomb's orbits	Reduced with Duncombe's orbits
I. Doppler frequency near eastern elongation.........	597,550 ± 200	598,950 ± 200
II. Doppler frequency near western elongation.........	599,650 ± 500	598,250 ± 500
III. Open-loop range at conjunction........	598,970 ± 100	598,930 ± 100
IV. Closed-loop range at conjunction	599,150 ± 100	598,850 ± 100
V. Long-count Doppler near western elongation.........	599,750 ± 500	598,750 ± 500

Appendix VII

Principal participants in the Working Group on Celestial Mechanics included the following:

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Gerald M. Clemence
Paul Herget
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Victor G. Szebehely
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Chapter Four

LUNAR AND PLANETARY RESEARCH*

I. Introduction

The over-all lunar and planetary program of NASA as presented in the "Thinking Document" is well reasoned and excellent in concept. The existing NASA program in practice involves many necessary compromises among scientific values, engineering feasibilities, and longer-range research needs. Slippages in schedules resulting from vehicle development problems further complicate evaluation of the course which can be taken by the program in the next few years.

Nevertheless, there are certain areas of basic research and development, types of facilities, and programs which should be particularly emphasized or considered in proper perspective under changing conditions. Thus far, little has been returned from space missions in the way of scientific data bearing upon the problems of the lunar and planetary research. One might say that the only missions successful in adding to our knowledge were the Soviet return of pictures of the far side of the Moon and the indication from Lunik III of the absence of a magnetic field. Thus we urge the early return of lunar and planetary scientific data by extending programs now becoming reliable, by setting limited goals for each mission, and by carrying out in the near future those experiments with the least engineering complexity while we are developing those of greater complexity.

This report deals with the immediate future up to and in part including Apollo. Inasmuch as landing a man on the Moon is an announced national goal, much emphasis is placed on those scientific experiments and observations which are vitally necessary to accomplish this mission or which might contribute to its success. Our specific recommendations are strongly influenced by this consideration.

Subjects which relate primarily to the fields covered in other chapters -- astronomy, planetary atmospheres, fields and particles, and the life sciences -- are in general omitted except in a few cases where overlap seemed justified. Because we are concerned primarily with the time interval up to manned landings on the Moon, comparatively little emphasis has been placed on important but more distant goals. Thus, not much has been said about exploration of the planets, asteroids and comets. Whatever has been said herein about the Moon can probably be applied to Mars at a later date. Pictures of asteroids, a comet probe, and measurement of the figure of Venus to determine whether it is rotating and whether the planet might be largely liquid would all be of great interest.

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*See Appendix II for the participants in the Working Group on Lunar and Planetary Research.

Section II below discusses the findings on lunar and planetary research. Section III lists the recommendations. Appendix I consists of supporting documents subdivided into three categories: A. Unmanned spacecraft experiments; B. Earth-based observations and experiments, and C. Special topics of scientific interest. The items set forth in this Appendix were prepared by individuals or small groups of individuals whose names are attached to each such contribution. Many more such topics might have been discussed. In general, we chose those which seemed to have particular pertinence to this report and those for which we had individuals present with necessary technical competence.

II. Discussion of Findings

A. Most of NASA's Program Is Endorsed

FINDING: As the expanded report of the Lunar and Planetary Working Group will show, we endorse most of NASA's program, but our main concern here is to improve this program. The following findings and recommendations, therefore, deal almost entirely with proposed improvements.

The over-all lunar and planetary program of NASA as presented in the "Thinking Document" is well reasoned and excellent in concept. In particular, the Ranger, Surveyor, and Mariner programs are designed to take advantage of reasonable advances in the general art and in the technology of boosters. With regard to these programs, we are primarily concerned with insuring that the planned experiments are actually performed successfully, unambiguously, and on a reasonable schedule in spite of experiment failures, launch failures, or delays in booster development. In recommending that information bearing on engineering design for the Apollo mission be given overriding priority for the next few years, we are not finding fault in general with the particular experiments of basic scientific interest already selected by NASA.

B. Accelerated Lunar Research Is Needed to Support the Apollo Mission

FINDING: The acquisition of information about the lunar environment and surface must be accelerated if responsible engineering decisions are to be made in time for a successful manned lunar landing and return by 1968 or by 1970. To this end it is necessary that NASA step up both the number and the information-gathering scope of unmanned flights planned as prerequisites to the manned landing, and that it also give increased support to Earth-based experiments and observations designed to provide information bearing on the probable conditions to be met and coped with during a manned landing and return. The philosophy of the Apollo program should be to utilize to a maximum the contributions feasible from Earth-based experiments and unmanned spacecraft.

We refer here to "Requirements for Data in Support of Project Apollo," issued by the Office of Manned Space Flight on 15 June 1962. This document indicated that technological data as design inputs must be received before the end of 1964, as design modifications before the end of 1965 and as hardware modifications before the end of 1966. This schedule emphasizes the need for early return of minimum data and for giving priority to engineering design data.

The reference document indicates that current Apollo design and development effort is relying on Earth-based and space-probe observations and measurements, and defines in detail requirements for environmental data, surface physical characteristics, and reconnaissance and topographic data. These requirements appear in general to be defensible; but we would particularly emphasize those relating to hazards on the lunar surface.

We are convinced that the current Earth-based and unmanned spacecraft programs will fall far short of providing information essential to the Apollo mission on the required schedule. If Apollo needs are to be met, greater emphasis will have to be placed on these programs, and information useful for engineering design will have to be given priority over information that is purely of scientific interest. Furthermore, we believe that closer collaboration between NASA's manned and unmanned programs is essential if the Apollo design information is to be obtained in time and if it is to be promptly and properly utilized.

C. Engineering Design Information Must Be Given Priority over Basic Scientific Information

> FINDING: If the Apollo time schedule is to be met, data acquisition necessary to support engineering decisions for this mission must take precedence over the acquisition of other data of possibly greater basic scientific interest. Overriding priority therefore, must be assigned to the early and reliable return of information of practical importance. Absolutely essential to designing for the Apollo mission is information concerning the hazard due to extreme surface roughness, the hazard due to deep or electrostatically-charged dust or to extreme surface crushability, and the hazard due to shrapnel-like secondary fragments from meteorite impacts. Of great importance therefore are high-resolution photographs of the lunar surface, lunar topographic and photogeologic maps, determination of the lunar gravitational field (especially in view of the recent decision to rendezvous in a lunar orbit), and design information for a lunar surface vehicle. All of this information should be obtained before making key engineering decisions with regard to the Apollo spacecraft.

Great ignorance about the Moon, together with the limited capacity of early spacecraft, demand that experiments be selected with great care. In this situation the requirements of the Apollo mission dictate that information essential for engineering design be given priority over basic scientific information that is not essential for the next few years. In addition to general information requirements detailed in the Apollo reference document, we would particularly emphasize the need for information relating to hazards on or near the lunar surface.

The hazard resulting from extreme surface roughness could be evaluated with the aid of high-resolution TV photographs, high-resolution chemical film photography, and high-resolution radar and laser observations from the Earth.

The hazard resulting from deep or electrostatically charged dust, or from extreme surface crushability, could be further investigated by high-resolution, Earth-based infrared and radio observations, by Earth-based experiments in which observed lunar surface characteristics are duplicated in a simulated lunar environment and by some of the soft-landed experiments programmed for Surveyor.

The hazard resulting from shrapnel-like secondary fragments from meteorite impacts could be evaluated on the basis of direct recording of these fragments and their effects in observations of considerable duration at several points on or near the Moon's surface.

Lunar topographic and photogeologic maps of low and high resolution are essential to the Apollo mission and would represent the culmination of a very extensive analytical and mapping operation. Knowledge acquired regarding hazards of various kinds as a function of position on the lunar surface could be presented on lunar maps.

If various information about the Moon leads to a strong preference as to Apollo landing sites in which considerable accuracy of landing is involved, a precise knowledge of the lunar gravitational field will be required, especially in view of the recent decision to rendezvous in a lunar orbit (see Chapter Three, Celestial Mechanics).

Information about lunar surface roughness and the characteristics of the surface materials may provide strong arguments for the use of a lunar surface vehicle and, at the same time, data essential for its engineering design. Such a surface vehicle might be designed not only to cope with the problem of passability of the lunar surface, but also to provide some protection from proton radiation and meteorite secondaries.

D. Much Information for the Apollo Mission Can Be Obtained with Unmanned Spacecraft

FINDING: Much of this information can be obtained by means of unmanned lunar impactors, soft-landers, and orbiters. Specifically, greater emphasis should be placed on the Ranger and Surveyor programs and especially on their extensions. Ranger can provide at the earliest possible date detailed TV photographs of limited areas on the Moon, can hard-land rugged experiments for early return of data, and could be modified to orbit light TV equipment and scientific experiments. Surveyor can soft-land TV cameras for a very close look at the surface and more elaborate and fragile instruments for mechanical, chemical, seismic, and gravity measurements, and can monitor hits by secondary fragments from meteorite impacts. Surveyor can also orbit TV cameras for general lunar photography, scientific instruments and radio and laser receivers for bistatic measurements with a transmitter on Earth, and can be modified to return chemical film from lunar orbit. Satisfactory TV photography as described above requires that the image orthicon be converted to qualified flight hardware and that more powerful transmitters (500 watts) be developed. Finally, if necessary, a soft-landed surface vehicle could be remotely controlled and tested for performance in the face of roughness, crushability, and electrostatic characteristics of lunar surface materials.

The most pressing need of lunar and planetary programs — in particular the lunar program in support of the Apollo mission — is the early return of scientific data. This requires greater stress on unmanned programs and especially on their extensions as they establish their reliability and effectiveness. In the case of the Moon, budgeted overlaps in the Ranger, Surveyor, and Apollo programs are essential to maintain a reliable and steady flow of lunar data. Continuation of a developed series minimizes the cost of obtaining scientific information since cost per launch decreases and reliability increases. The scientific programs associated with the Ranger and Surveyor spacecraft should therefore be tailored to the needs of the Apollo mission and assigned corresponding priority.

All lunar and planetary programs, present and future, depend strongly on TV photography. Improvements in the state of the art are possible, and it is vital that research and development efforts be expanded. Specifically, effort should be directed toward increasing sensitivity by converting the image orthicon camera to qualified flight hardware and developing more powerful transmitters, on the order of 500 watts, to increase bandwidth and permit higher resolution, hard impact and orbiting systems. Reliable fast-write, slow-read video storage systems require improvement for use in planetary probes. Higher-resolution TV on lunar orbiting systems appears to be the most effective method for surveying the Moon pending the time when the recovery of chemical film becomes possible.

In view of the unusual surface characteristics of the Moon as indicated by the photometric function, by thermal and electrical characteristics, and by the performance of surface models created under simulated lunar conditions, it is recommended that serious study be given to the design of transport vehicles to cope with lunar surface conditions. When information allows a specific choice of vehicle, it should be tested under simulated lunar conditions on the Earth, and then, if necessary, be soft-landed for remotely controlled operation on the Moon as soon as scheduling permits. Such experience could be used to design a remotely and locally controlled vehicle for transport of astronauts on the Moon if the difficulty of travel by foot in a space suit on the lunar surface is as great as suspected. This vehicle, properly developed, could be of extreme value in extending our exploration and scientific study over the lunar surface from a few landing points, both before and after manned landings.

E. Much Additional Information Can Be Obtained by Earth-Based Observations and Experiments

FINDING: Much additional information can be obtained relatively soon and economically from Earth-based experiments and observations. Essential contributions to practical knowledge can be obtained from observations over a large part of the electromagnetic spectrum, and specifically using optical, infrared, radio, radar, and laser techniques. Essential mapping and interpretive study of the topography, geology, and other features of the lunar surface can only be obtained by placing increased emphasis on completion of the Air Force LAC series of lunar maps and on photogeologic analysis by the U.S. Geological Survey and others. Laboratory experiments can be performed on the behavior of materials under simulated lunar conditions and on duplicating under these conditions measured surface characteristics of the Moon. Gold finds that dry dust particles at high vacuum settle into extremely sticky agglutinated masses, potentially capable of fouling up the mechanical parts, TV camera lenses, or solar cells of a lunar landing craft, or even turning an Apollo spaceman into a walking dust ball.

To insure early and economical return of data essential to its missions, NASA should increase its emphasis on Earth-based experiments and observations relating to the lunar and planetary bodies. On a short-range basis this emphasis would involve utilization of existing optical, infrared, radio, and radar facilities in the U.S. and elsewhere, with particular emphasis on lunar studies supporting to the Apollo mission. On a long-range basis, it would involve encouraging the establishment of one or more lunar and planetary observatories for more general and basic research. Such observatories would integrate optical telescopes with rapidly evolving infrared and laser techniques, would include high-resolution radio and radar facilities, and would assure NASA of essential scientific support at comparatively modest cost and under conditions favorable for international cooperative effort. (This last point is expanded in Chapter Fifteen on International Cooperative Programs.)

Mapping and interpretive study of the topography, geology, and other features of the Moon's surface should be accelerated, since this can be carried out readily and simply to an advanced stage prior to manned landings and since it provides early, low-cost basic information prerequisite to such landings and to many other investigations. In particular, NASA should encourage the early completion of the Air Force LAC Series of lunar maps and on photogeological analysis by the U.S. Geological Survey and by others. Urgent objectives are general topographic and geologic maps of the entire visible surface, detailed maps of certain areas of critical significance, and comparative studies of lunar and terrestrial topographic and geologic features. Higher-resolution TV return from lunar landers and orbiters and increased Earth-based telescope facilities for photogeologic study are essential elements of this program. A later objective would be to extend this mapping effort to include the entire far side of the Moon.

More Earth-based laboratory experiments should be performed on the behavior of materials under simulated lunar conditions. Many other useful data might be obtained on the effect of presumed lunar conditions, such as extremely low atmospheric pressure, high proton bombardment, low gravitational field, drastic temperature changes, lack of free water, etc., on materials and operations on the lunar surface. It would be of interest to determine the possibility of the survival of adsorbed films of water or gas molecules on finely divided rock particles under lunar conditions. Synthetic lunar surfaces could be tested, not only for the photometric function, but also for the duplication of observed thermal and electrical characteristics.

F. Practical Information about the Planets Should Be Accumulated

FINDING: The needs of future manned flights must be anticipated by obtaining practical information about the surfaces and environs of the planets well in advance of scheduled manned landings or visits. Specifically, the Mariner program should be emphasized and extended to provide a continuing flow of scientific information about the planets – especially about Mars, since that is probably the next manned landing site.

An early beginning and a steady accumulation of data and analyses regarding Mars and the other planets would allow us to make timely and responsible decisions in connection with manned landings or visits. With proper emphasis and extension the Mariner program could provide much of the early data. Additional early information could be obtained even on a relatively low priority basis for Earth-based equipment designed initially for lunar observation. Eventually unmanned planetary orbiters and landers would be required.

G. The Scientific Importance of Other Experiments Is Recognized

FINDING: Other experiments were recognized to be of great scientific importance, though necessarily of lower priority than the practical information in support of the Apollo mission. A most significant achievement would be the return of a sample, or samples, of lunar surface material. A review of the importance of the unmanned programs and perhaps of the schedule for the Apollo manned lunar landing may well justify a decision to return a lunar sample from an unmanned landing. The two stable points of equilibrium in the Earth-Moon gravity field would be attractive sites for locating long-term satellites for gravitational measurements and for observations of solar activity. Study of the nature, distribution and origin of debris in the vicinity of these points could be accomplished by Earth-based observations already started, by routing a scheduled interplanetary probe with appropriate detectors through the vicinity of one of the stable points, and perhaps by recovering a sampling probe from the region. By tracking a capsule in orbit around the Moon, we could obtain reliable moments of inertia, and thus an idea of the mass distribution within the Moon. This is particularly important in view of the low density of the Moon (near $3.3 g/cm^3$) and G. F. J. Macdonald's interesting suggestion (see below) that the outer shell has higher density than the interior. The systematic study of meteorites already present on Earth and currently arriving could be encouraged and supported at relatively small expense, and would provide a much better basis from which to assess information regarding the Moon's composition.

The return of a lunar sample would be a most significant achievement in advancing our understanding of the Moon. Many types of highly informative measurements (e.g., isotopic studies) cannot be carried out by remote control, but must be done in the laboratory. The total amount of new information gained from a laboratory study of even a randomly selected one-pound sample would be very great and likely to rank as a major scientific achievement. Therefore, if it should be feasible to schedule an unmanned landing and return from the lunar surface, then the return of a lunar sample should be one of its prime objectives.

Recent analytical investigation of points of equilibrium in Earth-Moon space has clarified the analytical characteristics of these points and, coupled with unverified Polish observational reports, raises interesting questions relative to the nature, distribution, and origin of "debris" in the vicinity of the two stable points. Operational use of these points, e.g., for long-term solar-flare observations, for study of charged particles outside of the Earth's magnetosphere, and for gravitational measurements, appears attractive. Ground-based observations of the stable points has been initiated by the U.S. Geological Survey. Therefore, consideration should be given to routing a previously planned interplanetary probe with appropriate detectors through the vicinity of one of the stable points and studies should be initiated to determine the value and feasibility of recovering a sampling probe after passage through one of those regions.

The density and moments of inertia of the Moon are of considerable interest. The density of the Moon, near 3.3 g/cm^3, is considerably lower than the decompressed density of the Earth and also lower than chrondritic meteorites. G. J. F. Macdonald has examined the moments of inertia derived from the Moon's physical libration and has suggested the possibility of an outer shell with higher density than the interior, an inference which is a literal interpretation of his results but which would be hard to accept without further evidence. It is recommended that these puzzling characteristics of our nearest neighbor in space be made the subject of a determined experimental effort. First among these would be to place a capsule in orbit around the Moon, track it, obtain reliable spherical harmonics of the Moon's gravitational field and moments of inertia, and thus an idea of mass distribution within the Moon. Later, when the capability exists, these observations should be supplemented by seismic and gravity experiments on the Moon to determine the internal structural characteristics.

Meteorites are the oldest and least altered samples of planetary matter accessible to us, and thus offer unique clues to the chemical, physical, and nuclear processes that occurred during the formation of the solar system. Because of their primitive character, they provide a frame of reference against which the evolution of the Moon and planets can be judged. Past experience has shown that any new advances in the understanding of meteorites also benefit the lunar and planetary programs, either by providing direct answers to some questions, or by making it possible to ask other, more sophisticated questions. In view of this interaction between meteorite and planetary research, it seems desirable to maintain a strong emphasis on meteorite work.

In the selection of alleged meteorites received by museums most are being rejected, either because they resemble terrestrial materials, or because they do not resemble well-authenticated meteorites. Although most of the objects thus discarded are undoubtedly worthless, the foregoing criteria will also lead to the rejection of genuine extraterrestrial matter of uncommon appearance. Samples of extremely great scientific interest, possibly including lunar and planetary material, may thus be lost. Such materials can be identified with certainty by their content of cosmic-ray-produced nucleides, either stable (He^3) or radioactive (Na^{22}, Al^{26}). Under favorable conditions, one mass spectrometer or one gamma-gamma coincidence spectrometer, set up specifically for this purpose, can screen several hundred samples annually. To cover the field adequately, only a few such instrumental systems, costing no more than about $50,000 each, would be required.

An understanding of impact craters is essential to an understanding of the surface features of the Moon. Much basic information can be gained from a study of terrestrial meteorite craters, and it would seem that present efforts, both experimental and theoretical, should be increased if all essential background information is to be available before lunar exploration begins in earnest. Specifically, more must be learned about shock effects; pressure and temperature contours during impact; changes in the chemical and physical properties of materials in and near the crater; throwout patterns; and the possibility of dating the impact by K-Ar or other methods.

There is a distinct shortage of basic physical and chemical data on some important planetary constituents, e.g., olivine, pyroxene, serpentine, nickel-iron, etc. Where such data exist, they generally cover only a small part of the temperature, pressure, and composition ranges of interest. Phase diagrams are often known for only two components at 1 atmosphere, when three or four components at higher pressures would be of interest. The equations of state of some common gases are not known at the temperatures and pressures prevailing in the Jovian planets. These and many other data are of basic importance in planetary science. Many laboratories exist that are equipped to determine data of this kind (e.g.,National Bureau of Standards, the Atomic Energy Commission laboratories, industrial and academic laboratories), but there seems to be no effective channel of communication between the users of these data and the potential suppliers. Therefore, a committee should be established to survey the existing needs in this field, and to secure the cooperation of qualified laboratories. Much if not all of this organizational work could be done by the appropriate committees of the National Academy of Sciences -- National Research Council.

Further investigations should be made of the potentialities of a gun sampler for recovering lunar rock in the Surveyor soft landing, as a possibly more reliable substitute or backup for the complicated rock drilling apparatus presently proposed. Gun samplers in common use by the petroleum industry use an electrically detonated powder charge to drive into the rock a hollow bullet with a sharp cutting edge in front and retracting cables behind, thus allowing the recovery of cores of either soft or hard rock formations about an inch in diameter and two inches in length. A variety of bullet types and charges could be provided to meet various contingencies with respect to the kind of lunar rock which might be encountered. The gun sampler is a comparatively simple and foolproof device and its adaptation to the Surveyor package should not be difficult. Although it does not have the advantage of the drill in providing a long sequential series of samples and a hole in which various probe measurements can be made, it might provide a desirable backup in case of malfunctioning of the drill and might perhaps replace the drill in some Surveyor packages.

Astronomers have called to our attention the possibility that on occasion, perhaps once in 10^6 to 10^7 years, solar storms of great intensity might occur. It may be these which have caused the Earth's magnetic field to reverse in polarity. The possibility was also suggested that such severe solar storms might so upset the Earth's atmosphere and ionosphere that they might have been the initiators of glaciation. If layered sequences of sediment were to be found on the Moon, one might find in them a level or levels indicating an episode of intense proton bombardment at some past time. Thus there is a chance that such layers may be useful as time planes in lunar stratigraphy. A study of abyssal sediments in the oceans on Earth might also reveal by some character or constituent the occurrence of an extremely intense solar storm. Possibilities might exist for Earth-Moon time correlations for the more recent of such events. If such events were recorded in layers of the oceanic sediments, it would be extremely interesting to examine the remanent magnetic directions in the material above and below the layers to see whether they correspond with a reversal of the Earth's field.

III. General Recommendations

RECOMMENDATION: We recommend that NASA achieve greater coordination, information exchange, and sharing of program responsibilities between its manned and unmanned programs.

RECOMMENDATION: We recommend that NASA orient its Apollo program philosophy toward greater emphasis on early scientific and engineering design information, and toward greater dependence on existing unmanned spacecraft programs and Earth-based observations and experiments as the principal sources of this information.

RECOMMENDATION: We recommend that NASA orient its Ranger and Surveyor program philosophy toward timely acquisition of information essential to the Apollo mission, and in particular that NASA schedule generous overlaps in the Ranger, Surveyor, and Apollo programs because, with continuation of a developed series, cost per launch decreases and reliability increases.

RECOMMENDATION: We recommend that NASA increase its emphasis on further Earth-based observations and experiments, especially in relation to the Moon, to provide early and economical data essential to the Apollo mission.

Appendix I

Supporting Documents

These documents were prepared by individual members or small groups of members of the Working Group on Lunar and Planetary Research. Many of these papers are technical contributions. Some present arguments for and against the views discussed, and their appearance here does not necessarily imply endorsement by the full group.

Group A: Unmanned Spacecraft Experiments

A. 1 Utilization of Unmanned Lunar Flights in Support of the Apollo Mission to Insure "Best Preparation"

L. C. Van Atta

Unprecedented technological problems complicated by scientific unknowns make the Apollo mission the most difficult undertaking in the history of man. International competition and resulting political commitments have forced upon this complex mission a somewhat unrealistic timetable. Mission failures would cost lives, international prestige, and tremendous dollar investments. Under these conditions, NASA philosophy for the whole Apollo enterprise must be that of "Best Preparation."

"Best preparation" for manned landing on the Moon means solving all technological problems and eliminating all scientific unknowns humanly possible by ground observations and unmanned space flights before and during the period when the information will be vital to the Apollo mission. Even as the cost per successful unmanned flight goes down, booster slippages and the inevitable failures will dictate a stretch-out of the Apollo schedule. There is more time for a pre-Apollo program than is indicated by the schedules or the support for present unmanned programs.

Lunar landing and takeoff is certainly the most difficult operation of the whole Apollo enterprise. As a result of the recent decision to accomplish this landing by means of a lunar orbit rendezvous, serious rendezvous problems are created, but the weight of Apollo which must be injected into a parabolic lunar transit has been reduced from three times to twice the weight landed on the Moon. This decision has reduced the size of booster required, but has further complicated the lunar flight itself.

In view of this situation it becomes all the more essential that a series of preparatory actions be taken in keeping with the philosophy of "Best Preparation." An important justification for these supporting actions is the psychological effect on the general public and on the astronauts themselves of knowing that as far as can be foreseen all necessary instruments, equipment, and supplies have been prelanded and checked as ready for use of the manned landing. A second important justification is that Apollo weight can be reduced to a minimum and yet much more equipment can be provided for the success of the mission.

The first essential is a thorough study of the lunar environment and surface characteristics. It is vital to know what radiations and meteorites will bombard man and his equipment on the Moon; whether the lunar surface dust will swallow man and machine or gather a foot thick on every metal and glass surface because of electrostatic forces; whether the temperature falls rapidly below the surface with the possible presence of ice, or rises and eliminates this possibility at low latitudes.

A second essential is to develop and operate unmanned roving vehicles to extend the exploration of the surface and to establish a method for transporting man when he arrives. The success of such a remotely controlled vehicle may be vital to man's travel on the Moon and would reduce the accuracy with which landings would have to be effected. A series of unmanned landings, however, would permit development of the art of precision landings.

A third essential is to locate on the Moon prior to a manned landing all equipment and supplies which can be shown to add to the safety and effectiveness of the mission. Transmitters on the lunar surface are needed for navigation of the orbiting Apollo, guidance of the touchdown vehicle, orientation of the landing astronauts and guidance for the return launch. The total weight of such equipment might be 1,000 pounds. The highpoint of man's arrival on the Moon will be his communication back announcing his safe landing, with periodic reports of his findings thereafter. Effective reporting by words and pictures would involve communication equipment with a total weight of about 600 pounds.

Certain scientific experiments or engineering tests of the lunar surface, being difficult to operate automatically, might be prelanded for operation by the astronauts, and could be expected to weigh several hundred pounds. Every countdown done on Earth for a space launching is an exciting experience involving a variety of complex and heavy equipment. Such checkout equipment cannot be carried on the Apollo because of weight and so must be prelanded. Finally, supplies such as oxygen, food, water, medication, test equipment, and spare parts to a total weight of some 500 pounds should be prelanded.

A.2 Development and Test of a Lunar Surface-Transport Vehicle

L.C. Van Atta

It has become clear that the lunar surface is very different indeed from that of the Earth. Perhaps this should not have been too surprising in view of the very different environment on the Moon. Unimpeded meteoritic bombardment, absence of atmosphere and especially of water vapor, lower gravitational attraction, and intense proton bombardment combine to create unusual conditions.

To the extent that these conditions have been simulated in a vacuum chamber they have resulted in a surface of weird appearance and unusual characteristics. The tendril-like growth of dust formations, however, does reproduce the photometric function reasonably well after proton bombardment. In addition it promises to approximate the observed thermal and electrical characteristics: a very low thermal conductivity and radar reflectivity.

Of great concern, however, is the performance of this dust when disturbed. The dust then travels along electrostatic lines of force, unimpeded by atmosphere and practically not even by the Earth's gravitational field, and clings in great depth to exposed surfaces. The relief of strains by stirring may add to electrostatic forces or even cause heating. The concern is the metal and glass surfaces exposed to such disturbed dust on the Moon might be coated to such great depth as to obstruct vision, prevent travel by foot and even impede travel by a surface-transport vehicle.

In view of this situation, it has been recommended that serious study by given to passability of the lunar surface for traffic, well in advance of projected manned landings on the Moon. Advance information on performance in situ can be obtained only with a soft-landed, remotely controlled, surface-transport vehicle. Therefore it has been recommended that such a vehicle be designed, tested under simulated lunar conditions on Earth, and finally soft-landed on the Moon for tests under actual operating conditions. Furthermore, it has been recommended that the probable need to provide man on the Moon with vehicular surface transport be recognized by using experience gained with the unmanned surface vehicle to design a remotely and locally controlled vehicle for transport of astronauts on the Moon.

Faced with the need to minimize the weight of the Apollo and yet adhere to the philosophy of "Best Preparation" for the manned lunar landing, it appears reasonable to develop such a surface vehicle and to soft-land it on the Moon in preparation for the manned landing. If the vehicle were equipped with a transponder beacon it would eliminate the need for a pinpoint landing, since the vehicle could be commanded to come to the site of the manned landing. If the vehicle were equipped with good communications equipment it could maintain reliable contact with the landing site during extended forays into the surrounding area.

A.3 Television Photography

K. Linnes, E. Shoemaker, and S. Sternberg

Our purpose was to investigate available information on the photometric conditions of the Moon and to match these conditions with television sensor capability.

The illuminance of the Moon's surface by the Sun is:

(Eq. 1)
$$E = \frac{I}{r^2} f(\theta) = E_s \cdot f(\theta)$$

where: E = illuminance (lumens/ft^2), E_s (solar constant in ft-lamberts) = 13×10^3 ft-lamberts, and $f(\theta)$ = photometric function (Figure 1), with $\theta = 0^\circ$ along the Moon-Sun line.

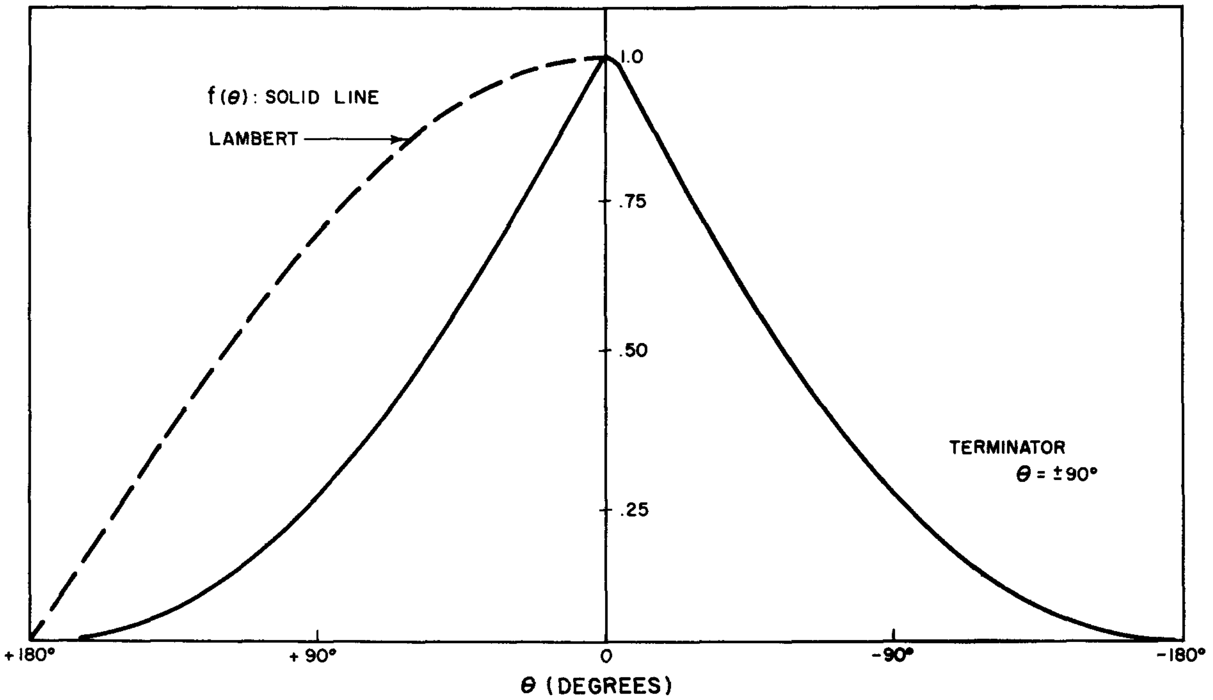


FIGURE 1.

Photometric function of the lunar surface, showing illuminance as a function of angle of incidence of sunlight (phase angle). At $\theta = 0^o$, the Sun is overhead (subsolar point); at $\theta = 90^o$, the Sun is on the horizon (terminator).

The luminance B (in ft-lamberts) that a television sensor would observe normal to the Moon's surface at any angle θ is given by:

(Eq. 2) $B = \alpha f(\theta) E_s$

where: α = lunar reflectance or normal albedo.

With a Moon surface model having slopes of ± 15°, a series of approximations yields the result that there are 2 shades of grey at 30° from the terminator ($\theta = \pm 60^o$) and 4 levels between 20° and 25° from the terminator. The ratio of two levels is defined as $\sqrt{2}$. Educated assumptions yield values of $\alpha = 0.2$ for highlight reflectors on the Moon and $\alpha = .05$ for average reflectors on the Moon. Working through Equation (2) for highlight reflectors at 30° from the terminator, one obtains B = 600 ft-lamberts (see Figure 2.)

Taking B through the camera optics and shutter system, we can obtain the following expression for the average light energy falling on the photosensitive target of the television sensor tube:

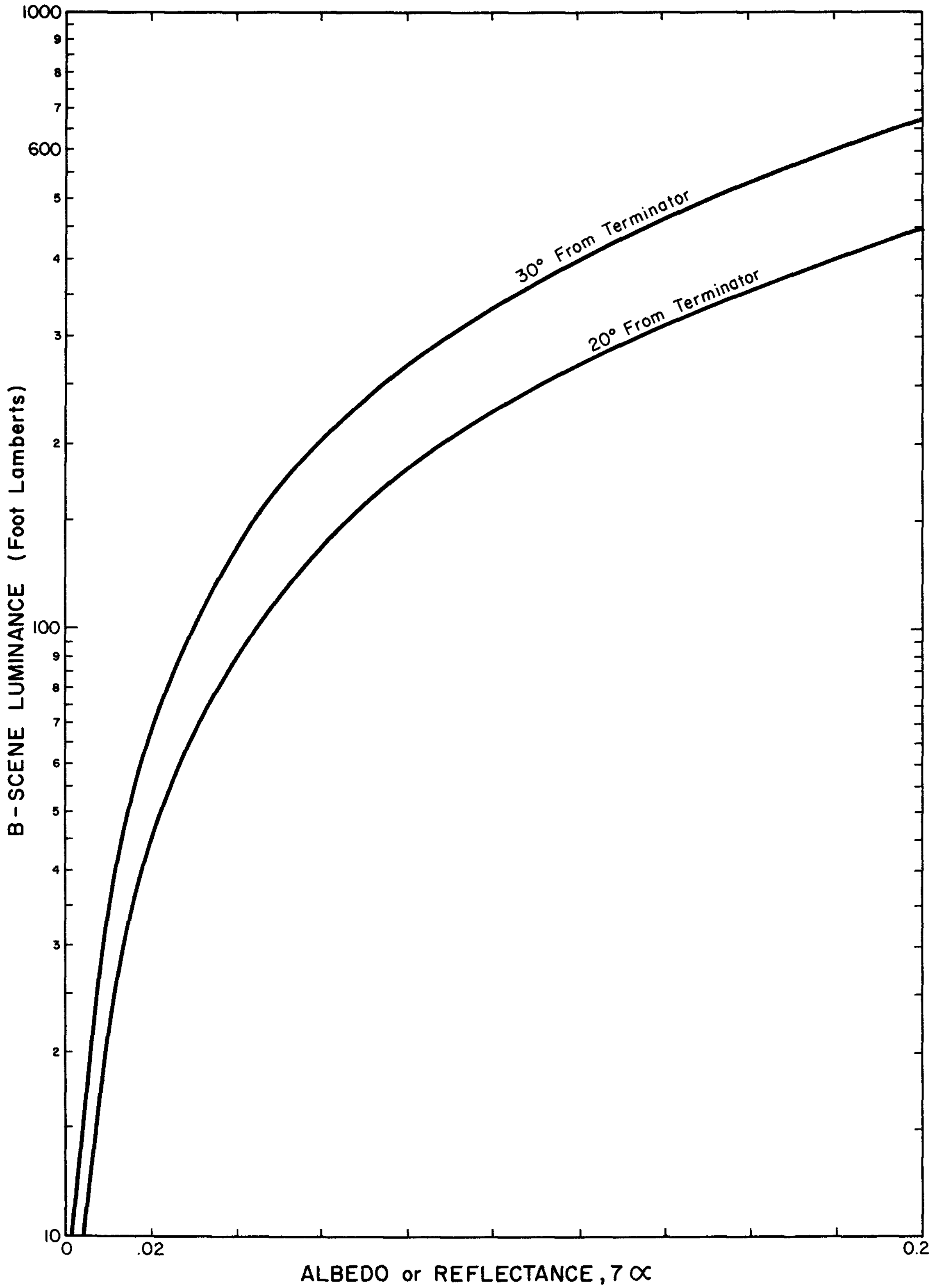


FIGURE 2.

Luminance of lunar surface as a function of albedo α and phase angle θ°.

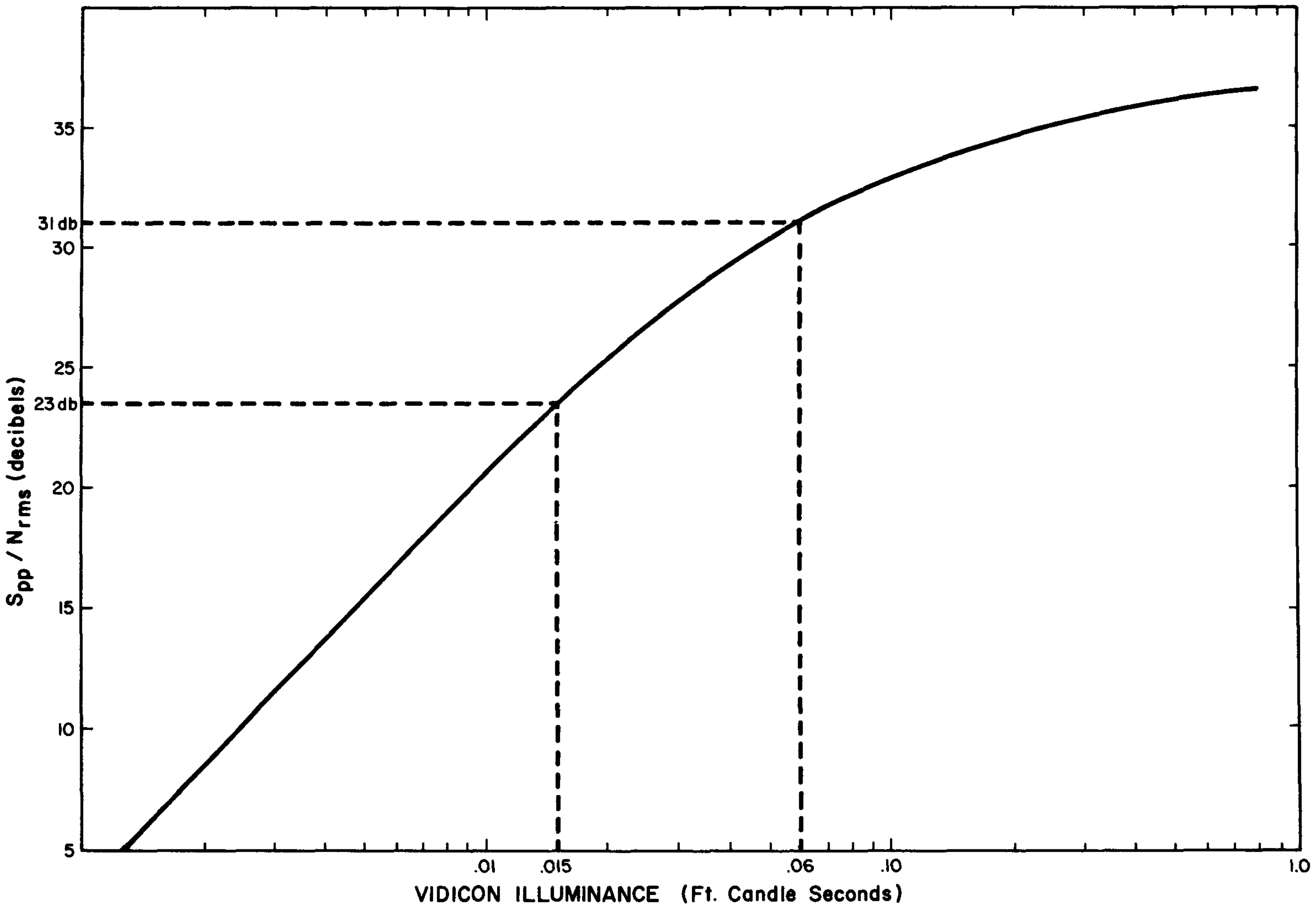


FIGURE 3.

Signal-to-Noise Ratio of a vidicon as a function of illuminance.

(Eq. 3) $$W = \frac{\tau \Delta t}{4(f/\#)^2} B \text{ foot-candle-seconds}$$

where: τ = lens transmission = 0.9, Δt = exposure time = 2×10^{-3} sec, f/# = 2(for the optics of Rangers 6-9). For highlight reflectors, W_1 = .06 foot-candle-second; for average reflectors, W_2 = .015 foot-candle-second.

The dynamic characteristics of an average vidicon shown in Figure 3 yield:

$$S/N_{(W1)} = 31 \text{ decibels}$$

$$S/N_{(W2)} = 23 \text{ decibels}$$

Pictures with a signal/noise ratio of 23 decibels (S = peak-to-peak signal, N = r.m.s. noise) are of marginal acceptability.

Conclusion and recommendations: It is concluded that a Ranger impacter using vidicon sensor tubes will provide pictures of interest if the optical axis impacts along a normal between 25° and 30° from the terminator. This is a reasonable compromise between sensitivity and grey levels.

It was recommended by the Lunar and Planetary group that NASA should initiate development of an image orthicon camera which would yield pictures closer to the terminator. In addition, higher resolution is important. This can be implemented not only through better sensor tube performance but by using high-power transmitters, allowing pictures to be taken and transmitted closer to impact.

A.4 Lunar Photography

S. Sternberg

It is the purpose of this memorandum to investigate the usefulness of chemical photographic film returned to Earth after circling the Moon, in particular its value as a means for recording the physical form of the Moon surface material as compared to what can be expected from a lunar-impact television system.

There are three primary parameters describing the capability of a picture sensor: (1) resolution, (2) sensitivity, (3) grey levels (dynamic range dependent on signal/noise ratio).

The resolution of a picture, R_g, can be expressed in meters/line on the terrain. R_g is a function of three variables: R_s, the resolution of the sensor in optical line pairs/mm; h, the altitude or distance in kilometers from the object being photographed; and F, the focal length of the optical system in compatible units. For an orbiter or lunar turnaround it is assumed that h = 250 km, F = 1 meter and for a high resolution photographic sensor R_s = 100 optical line pairs/mm. For a subpoint of the optical axis on the surface of the Moon a single calculation for chemical photography yields

$$R_g = \frac{250 \text{ km} \times .01 \text{ mm}}{1 \text{ M}} = \frac{2.5 \text{ meters}}{\text{line}}$$

The final picture for the television impactor has been calculated by Jet Propulsion Laboratory investigators as 0.6 meters/line (0.3 meters/T.V line). This picture will be four times better in resolution than the chemical photograph. If a transmitter is used which has ten times the power output, approximately 500 watts, the television picture can be transmitted in 1/10 the time. The altitude for the final television picture will be 1/10 of the existing system and the resolution will then be 0.06 meters/line or forty times better than chemical picture.

The sensitivity of the photoconductor of the vidicon tube is approximately sixteen times better than ordinary silver halide. Since justifying this number would require a detailed explanation of the method of measurement, it is easier to specify an image orthicon which is generally recognized as being many orders of magnitude more sensitive than photographic film. Astronomers are using photoelectron emission devices in their telescopes to increase sensitivity and their effective apertures. In any case, a chemical photographic camera would have to take pictures further away from the terminator than a television camera, thus reducing its effectiveness. (See Report of the Subcommittee for Television Photograph reproduced above as Appendix I, Section A.3.)

The grey-level comparison is a value which I do not know. However, since due to reduced sensitivity it would be necessary to take pictures closer to the Sun line, there would be fewer grey levels to observe. (See Appendix I, Section A.3.)

An advantage to photographic film is its ability to collect a large number of total bits in a single picture, the number depending on the width of the film used (9-10 inches is possible). This does not change the effect of the three parameters described above.

Conclusion: It is highly unlikely that chemical photographic film returned to Earth after circling the Moon will add any new information not available from a television impactor about the physical form of the Moon's surface material.

A.5 Pictures of the Moon

W. W. Kellogg and L. Steg

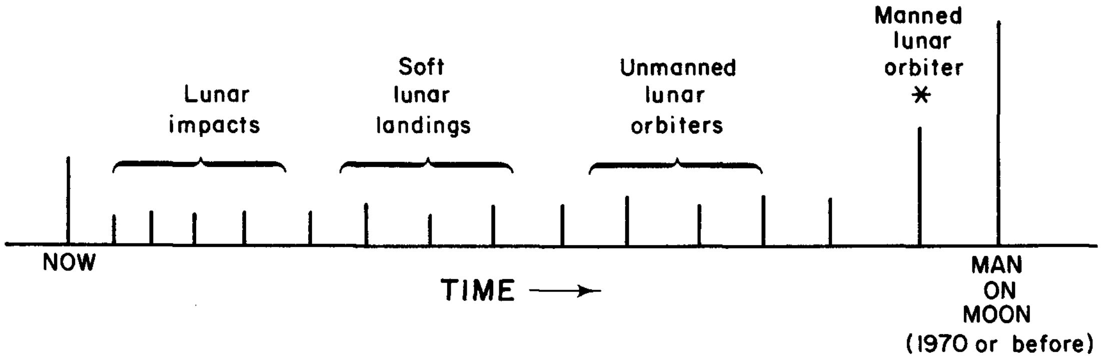
Introduction: All lunar and planetary programs, present and future, depend to a very great extent on obtaining good pictures. The limit for photographic resolution of present telescopes on the ground under very good seeing conditions is about 1" of arc, and this corresponds to a resolution on the Moon of about 2 km, and a resolution on Mars at opposition of about 300-400 km. It is clear that much better pictures must be obtained before one can make detailed studies of either of these bodies.

There is some hope of obtaining 0.2 to 0.3" of arc from a high-altitude balloon-borne telescope. (M. Schwarzschild has reported that original estimates of 0.1'' with the 36'' balloon telescope were probably overly optimistic.) J. Baker and others have suggested novel image motion compensation or correction devices which could enhance photographic resolution on the ground, but these are apparently not being developed. (They probably should be). Certainly from a lunar impacter or orbiter it will be possible to do much better. The purpose of this Section is to outline the factors involved in doing this, and to discuss the relative merits of television and chemical photography (with recovery) for the job.

The primary purpose of obtaining better pictures would be mapping. For the Moon -- and we will restrict the discussion from here on to the Moon -- the need for good topographic maps has long been evident, and a program to prepare rough topographic maps is already being undertaken by the Air Force. A second and equally important purpose for pictures is for geological studies, and geological maps are being drawn by the Geological Survey, with NASA support. Both kinds of maps, topographic and geological, are essential prerequisites to a lunar landing.

In order to improve the present maps <u>any</u> improvement in resolution would be useful. However, even an order of magnitude improvement, giving 100 m, would not be adequate for the very detailed study of the landing site to be finally selected. Thus, we may talk about an order of magnitude increase in resolution (100 m) as a reasonable first goal for pictures of the large areas of interest; following this, a second goal will be still higher resolution (1 m) pictures of areas selected for a landing. With these objectives in mind, we will describe a logical course of action to obtain them prior to the first Apollo landing.

<u>General Considerations</u>. Even with all the present uncertainties in the exact sequence of flights prior to the first manned landing on the Moon, there seem to be certain considerations that dominate. (We are indebted to Amrom Katz, of Rand, for suggesting much of the following argument).



* Not now a part of Gemini or Apollo program prior to manned landing, but still likely to be if there is any delay in the development of the lunar exploration module.

FIGURE 4.

<u>Schematic Outline of Flight Sequence</u>

The unmanned lunar program will continue with the present sequence of Ranger and Surveyor landers. The hard landers will take television pictures as they approach the surface, and the resolution of the last picture before impact will be about 2 m. The soft landers (Surveyor) will take panoramic pictures after landing as well. Thus, rather early in the unmanned program there will be a few high-resolution pictures of limited areas, giving a first detailed look at the surface features.

There are already advanced design studies of unmanned lunar orbiters. There are good reasons for expecting these to be included in the program, since so much can be learned from them. (The Summer Study is strongly recommending that they be included in the program.) These will be able to photograph by television the entire surface of the Moon (including the back side) at the 200 m resolution that we have set as our goal for general reconnaissance. The total amount of information involved is $S \cdot 4\pi r^2/(0.2)^2$, where r is the radius of the Moon, and S is a factor for the number of shades of grey (assumed to be 4 here). This turns out to be about 5×10^9 bits, and with a 100 kc bandwidth (corresponding, for example, to the TIROS bandwidth, though obviously requiring considerably more power in the transmitter), it would take less than a day of transmission time to send this to the Earth. This seems reasonable indeed. Naturally, one could not in fact photograph all of the Moon in this short period, since half of it would be dark, but the lunar orbiter could continue the process until it was accomplished at the end of a month.

A somewhat more sophisticated unmanned orbiter could be commanded to take higher-resolution television pictures of limited areas of interest. This would be a straightforward process, and would permit a better study of potential landing sites (see below).

It is not now planned to send a manned flight around the Moon, but it seems likely that any slip in the development schedule of the landing vehicle and its associated systems would put a gap in the flight schedule that would naturally be filled by a manned flight around the Moon. This would seem to be a desirable step to take in any case. Such a trip, it almost goes without saying, would involve taking pictures, both wide-angle and telescopic. The storage capacity of film, as has been pointed out, is very large indeed. Thus, the astronauts would return with detailed photos of many parts of the Moon, in color if desired.

State-of-the-Art in Photography and Television. If there were no considerations other than the simple choice between photography (with recovery) and television for obtaining pictures from an unmanned lunar orbiter, then (as will be shown) the selection would not be clear. For a given amount of aerial reconnaissance (a certain area to be covered at a certain resolution) one approach seems to be about as good as the other. In Reference 3, M. E. Davies gives a detailed discussion of the potential for photographing the Moon with a recoverable vehicle. However, there are indeed other considerations, and these involve the need for more or less real time views of scenes on the Moon, and this means television. For example, the early lunar impacters must send the picture back just before crashing, devices for making observations on the surface may be monitored by television, and there will be a considerable amount of television development for Apollo. These considerations have led to the tacit assumption that television is the only feasible way to get pictures from an unmanned lunar orbiter (or lander), and the following discussion will demonstrate that this is not so. (See also Reference 3).

In the following tables some representative numbers are given that show what is involved in current film and television systems, the purpose being to obtain high-resolution pictures of areas likely to be selected for landing sites.

Some State-of-the-Art Estimates

a) Sensor characteristics

TV	40 line pairs/mm	(1.25 cm x 1.25 cm) (TV camera target)
Aerial Photo	100 line pairs/mm	

b) Weights and focal length to attain a given resolution.

Chemical Photography

Resolution from 100 km (meters)	Focal Length (inches)	Mapping Camera (lbs.)	Aerial Photo Camera (lbs.)
10	3	165	30
5	6		60
2·5	12	350	110
1	40		150

TV-Data Link

Resolution from 100 km (meters)	Focal Length* (meters)	TV Camera (lbs.)	Transmitter (lbs.)
5	0.5	50	50
2	1.25	100	50
1	2.5	150(?) to be developed	50

*Assuming a 1.25 cm x 1.25 cm TV target

In a weight comparison the chemical photography system must be charged with additional weight of the order of 100 lb representing the system for launching the vehicle out of lunar orbit and recovering it on Earth.

For a lunar orbiter the following might be typical mission requirements for the detailed pictures required before selecting a manned landing place:

Orbit Altitude	100 km - 10 orbits/day
Resolution	1 m
Areas of Observation	10 areas 100 km x 100 km each Orbit $\pm 20^{\circ}$ from lunar equator (These would encompass landing areas numbered 1-8 and 12, selected by Shoemaker and Eggleton)

It is seen from the above weight estimates that the launch payload weights on the primary booster for a lunar orbiter TV-data link and a lunar orbiter (film-cassette) recovery system may be comparable, the weight of the data link (recorder and transmitter) being approximately equal to that of the return system. In the recovery system, as in the data link system, cameras are left in lunar orbit. Calculations indicate that 200 5" x 5" photos/area of observation (50% overlap stereo) would be obtainable.

Reentry and Recovery from Lunar Orbit. Actual recovery of capsules from satellites is now routine (Discoverer). The reentry problem at the escape speed represented by a lunar return results in an increase of at most 25% in heat shield weight (using modern ablative cooling). Some representative numbers for the capsule reentering the atmosphere are as follows:

Film and Cassette	22 lbs.
Heat Shield	15 lbs.
Recovery package	28 lbs.
Total weight	65 lbs.

There are about 125 5" by 5" frames per pound of film and casette.

Degradation of Film by Ionizing Radiation. Present photographic manufacturing processes do not permit the making of film which is sensitive to light for normal exposure without comparable sensitivity for gamma radiation. Thus conventional photographic systems would be severely affected by solar flare and Van Allen effects. Nepela and Nitka (Reference 4) employ a "reverse" processing technique. Their success is evident from an inspection of photographs (available to this working group) in which virtually full contrast and resolution was brought out in processing film exposed to a background of as much as 200 r of gamma rays. It is concluded that this problem is probably not a major one.

Television Data Link Readout Time. The number of bits of information required to represent ten regions, each 100 km x 100 km in area, with a resolution of 1 meter and with 16 shades of grey (a factor of 4 in the number of bits) is $4 \times (100 \times 1000)^2 \times 10 \times 2$ or 8×10^{11} bits. The last factor of 2 is for complete stereoscopic overlap. If we assume 2.5 TV scans per line pair and a bandwidth of 2.5 Mc/S (which would be required anyhow for live TV from Apollo), the time required to transmit this information continuously back to Earth would be

$$\frac{2.5 \times 8 \times 10^{11}}{2.5 \times 10^{6}} \text{ secs}$$

or 8×10^5 secs, or about 9 days. Since the orbiter and receiving station would be out of sight of each other more than half the time, the elapsed time required to complete the transmission would be about 25 days. Actually, it would take longer than 25 days to get the same scenes under various conditions of illumination, so transmission time is not the limiting factor.

Summary Comparison of Television vs. Recovered Film. For a mission requiring the observation from an unmanned lunar orbiter of 10 areas 100 km x 100 km from an altitude of 100 km with a resolution of 1 m, the following table summarizes the comparison of television with recovered film.

Factor	Recovered Film	TV Data Link
Resolution, i.e. camera	available	requires development
Weight on primary booster	about the same	
Operation time for equivalent amount of data.	14 days	30 days
Primary difficulty	attainment of proper return trajectory	long-time reliable operation and power required.

Thus, for the unmanned lunar orbiter, it appears to be roughly as feasible to get the high-resolution pictures (1 m) by recovered film as by television. The choice of television over film has been made for other reasons, but there may be an occasion to reconsider the choice at some time in the future. For example, there may be unexpected difficulties in obtaining high-resolution television pictures, it may be deemed necessary to develop unmanned systems for bringing samples back from the Moon prior to Apollo (the substance of another Summer Study recommendation).

In the latter case lunar reconnaissance pictures could be brought back with the same system. Furthermore, there may be such a slippage in the Apollo program that there will be encouragement to try alternative ways of obtaining data with unmanned systems.

Conclusions. The present program of flights to the Moon will involve many television pictures, both very high resolution ($\sim$1 m) of the selected areas and moderately high resolution ($\sim$100 m) of the entire Moon. The latter resolution is more than an order of magnitude better than has so far been obtained from the Earth. These are essential to the topographic and geologic mapping of the Moon, which should be done prior to the selection of a site for a manned landing. The inclusion of an unmanned lunar orbiter in the program is essential for this undertaking.

There does not appear to be a need at this time for the special development of an unmanned recoverable vehicle for circumlunar photographic reconnaissance, though this would be quite feasible. However, it seems likely that there will be at least one manned circumlunar flight prior to the first landing, and the films brought back from such a journey will supplement the television coverage already obtained by the lunar orbiters.

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A. 6 Measurement of the Tidal Variation of Gravity on the Moon as Part of an Extension of the Unmanned Lunar Soft-Landing Program

L. C. Van Atta

The study of tides in the solid Earth constitutes an important source of information regarding internal constitution of the Earth. Response of the Earth to tidal oscillation provides an intermediate link between high-frequency seismic behavior and secular response determined from the gravitational figure.

Similarly, observation of body tides on the Moon and Mars can yield significant information regarding the internal constitution of these bodies. In addition, simultaneous observation of gravity on both Earth and Moon provide a means of separating planetary effects from variations due to gravitational waves of cosmic origin.

The amplitudes of these gravitational tides on the Earth are surprisingly large, and affect observations of latitude, longitude and the length of the day. They produce changes in gravitational acceleration and the position of the vertical at a point, and cause horizontal strains and volume dilations in the Earth's surface.

Tides on the Moon produced by the Earth's gravitational field are larger than those on Earth produced by the Moon. So the prospect of using tidal observations to gain knowledge of the properties of the Moon's interior is very attractive. A single series of tidal observations of about one year's duration would immediately settle the basic question as to whether the Moon's interior is largely fluid or solid. A similar conclusion from seismic methods would require several sites and, if the Moon is seismically inactive, a series of large artificial explosions.

Both gravity and tilt meters could be used for tidal observations, but the former seems preferable. In particular, the LaCoste-Romberg gravity meter, which has the benefit of 30 years of development and has been used by the University of California since 1956, would be a good starting point for the development of a lunar gravity meter. Gravity measurements on the Moon will be so difficult, however, and existing gravimeters are so sensitive to temperature and even gentle handling, that development of a lunar gravimeter throws the doors wide open for new ideas and techniques.

Gravity measurements on the Moon would suffer from large temperature variations, the necessity for extreme reliability, the limited power supplies available, and the complicating effects of lunar librations. On the other hand, there are certain advantages: the gravity variations are large; the lower value of gravity reduces the effect of changes in the elastic properties of the spring; and there are no ocean tides or atmospheric disturbances to complicate measurements.

Development and use of the gravity meter on the Moon would prepare the way for later use on the planets -- on Mars in particular. Martian measurements would require an increase in sensitivity, but likely means for achieving this sensitivity are already at hand. Measurements on Venus are not promising because of its high surface temperature, and perhaps of less interest if it has one face fixed toward the Sun.

In summary, it is recommended that a program extension of unmanned lunar flights include provision for the measurement of the time variation of gravity on the Moon, and that a gravimeter be developed for use on the Moon. This important measurement has been selected as an example of the type of observation that, because of its duration of a year or more, would be part of an unmanned program, even if manned landings on the Moon had been achieved.

Group B: Earth-Based Observations and Experiments

B. 1 Ground-Based Observations at a Wide Range of Frequencies to Advance our Scientific Study of the Moon and to Support the Apollo Mission

L. C. Van Atta

In keeping with the philosophy of ''Best Preparation'' for the Apollo lunar landing, and in the interest of obtaining scientific and engineering information as soon and as economically as possible, it has been recommended that NASA increase its support of ground-based experiments and observations. The recommendation recognized that short-range gains could be made by increasing its support of many existing activities, but that this should be supplemented as soon as possible by contributions from NASA-established lunar and planetary observatories. The recommendation further recognized that such observatories should encompass, at least administratively, the full spectral range available for observation, including ultraviolet, visible, infrared, radio, radar, and laser techniques.

The purpose of this Section is to present detailed arguments in support of the recommendation. Primary emphasis will be given to study of the lunar surface since the Moon is the closest body and since detailed information about the Moon is needed for the Apollo mission. Some of the techniques to be discussed, however, have already been applied to Mars, Venus, and Jupiter, and further study of the planets is deserving of moderate, but continuing support.

Infrared, Submillimeter, and Millimeter Wave Observations. In this section we paraphrase a statement by E. M. Shoemaker. Observations of the lunar surface at infrared, submillimeter and millimeter wavelengths can reveal detailed information regarding the physical nature of the top layers of the lunar surface, including dielectric constants, thermal conductivities, densities, and specific heats. Different wavelengths provide information at different integrated depths. The result can be a detailed knowledge of the variations of different parameters with depth. Use of high resolution allows comparisons between visible features of the lunar surface, such as highlands, maria, crater floors, and crater walls.

Pioneering observations at many wavelengths have shown that accurate and meaningful information can be obtained in practice. The amount of effort devoted to these studies, however, has been relatively small, and much remains to be done. In particular, many more high-resolution studies are needed in the far infrared and at millimeter wavelengths. Telescopes with apertures of some 30 feet and high sensitivity detectors, such as the cooled germanium bolometer, are available at these wavelengths and should be applied to lunar studies. These telescopes would provide a resolution of at least 1/2 minute of arc, sufficient to clarify greatly our picture of the lunar surface, and to provide very valuable guidance to the first manned landing. (Reference: J. E. Gibson, Proc. I. R. E. 46, 280, 1958).

Radio Emission. Radio emissions are those which can be studied using radio receiver techniques. These measurements yield a radio brightness temperature which can be compared with the surface temperature as measured with infrared techniques. Reflected solar radiation will not cause sizable errors in measurements of lunar radiation at centimeter and decimeter wavelengths, and corrections can be made for it at meter wavelengths. As the wavelength is increased, so is the effective depth at which the radio brightness temperature is measured. Therefore this technique holds the promise of determining electrical and thermal properties of lunar subsurface materials, and the heat flow to the surface.

Therefore we must make sufficiently accurate and complete measurements to set limits on the thermal and electrical characteristics of the surface and subsurface materials of the Moon. In particular, we must make these measurements with the highest obtainable resolution in order to relate the results to lunar latitude, lunar cycle, and visible surface features.

Measurements of radio brightness temperature can be analyzed into a constant component and a variable component. Theoretically, the constant component of radio brightness temperature at the center of the lunar disk could be compared with the average surface temperature at the lunar equator obtained from infrared measurements to determine the steady temperature gradients through the surface. Unfortunately, uncertainties in the radio observations and interpretations introduce a probable error of the same order of magnitude as the observed difference from the infrared temperature. Therefore much additional work will be needed to determine the steady temperature gradient through the surface and to provide the basis for a model of surface and subsurface lunar structure. These measurements will have to be made over the full lunar cycle, and over a range of wavelengths from below one millimeter to about 30 centimeters. Emphasis must be placed on the use of comparable techniques of observation and reduction of data, and particular emphasis must be placed on the highest achievable angular resolution.

The variable component of radio brightness temperature can be expected to show a strong dependence on wavelength. Since radio absorption in rock decreases with increasing wavelength, longer-wavelength radio emissions effectively correspond to subsurface lunar temperatures at greater depths. As might be expected, the observed variable component shows a decreased amplitude and a lagging phase as compared with the surface temperature variation.

In spite of considerable scatter, existing measurements of the variable component exhibit some interesting features. The amplitude appears to decrease from about 60°K at 4 mm to 12°K at 3 cm wavelength. Furthermore, the lunar maria appear to heat and cool more rapidly than do the mountainous regions. The results however, are marginal in accuracy and are submarginal in angular resolution. Therefore careful consistent measurements over the full lunar cycle and over a range of wavelengths from below 1 mm to above 30 cm are needed to establish the value of the variable component as a function of wavelength. In particular, the highest achievable resolution is essential to establish the distribution of this effect over the lunar disk and to relate it to visible lunar surface features.

We may be more specific by quoting here from J. E. Baldwin (Monthly Notices, Royal Astronomical Society, Vol. 122, pp. 513-522, 1961):

> "...three types of observation are now needed:
>
> "(i) Eclipse observations at the shorter wavelengths to determine more precisely the appropriate model of the outermost layers which largely determine the mean surface temperature.
>
> "(ii) Observations in the range 3-21 cm designed to detect any variation with the lunar cycle, preferably by comparison of the brightness temperatures of the two halves of the lunar disk.
>
> "(iii) An investigation of the dielectric properties of stony meteorites in the radio-frequency range."

Note that the third type of observation is concerned with meteorites on the Earth.

As the result of a careful and detailed analysis using all experimental and theoretical results available to him, A. Giraud (Astrophysical Journal, Vol. 135, pp. 175-186, 1962) makes the following very pertinent statements: "No conclusion, however, can be drawn about a possible intrinsic lunar heat flux, since we have averaged values integrated over varying portions of the disk of the Moon. Moreover, the precision of the observations is not sufficient to warrant further investigation of this matter." Also, "Furthering of this work should involve laboratory experiments with materials likely to have been formed or to have originated on the Moon and, as the precision of microwave observations improves, separate analysis of the radio thermal measurements for the lighter and darker parts of the Moon's surface."

Radar Reflection. Radar reflections from the Moon are basically capable of providing information on its distance from the Earth, its figure, its topography, and electrical characteristics of its surface and subsurface materials. Essential features of the radar used for these studies are range resolution (pulse length and shape), angular resolution (antenna beam width), velocity resolution (Doppler frequency shifts), and means for determining the degree of depolarization resulting from reflection at the lunar surface. Particular emphasis must be placed on radar experiments which provide the highest achievable resolution in angle, and good resolution in range and Doppler shift, together with standardized procedures for observation, calibration, and analysis of data.

Radar reflections from the Moon have been measured at wavelengths ranging from 10 cm to 3.0 meters. Unfortunately some of these experiments have used long pulse lengths and broad antenna beams, so that the fine structure of the reflected wave was lost. In general, the reflected wave shows a sharp rise to a peak of high intensity, corresponding to specular reflections from the front face of the Moon, followed by a long low-intensity tail corresponding to back scattering from the limbs.

Those experiments which used a short transmitted pulse broke down the initial high reflection into a series of discrete high-intensity pulses, followed by a low-intensity tail in which many smaller returns were merged. The discrete pulses indicate a series of reflections from properly oriented surfaces of considerable areas near the center of the lunar disk.

The magnitude of these discrete reflections may be determined by the curvature of the reflecting surface or by its area, depending upon the geometry and wavelength. If the surface can be approximated by an ellipsoid and if it includes several Fresnel zones for the wavelength used, the geometrical optical reflection cross-section (independent of wavelength) is:

$$\sigma = \pi R_1R_2$$

If the area includes less than one Fresnel zone, its reflection cross-section is:

$$\sigma = A^2/\lambda^2$$

where A is the contributing area and λ is the radar wavelength. To obtain the actual power reflected, these reflection cross-sections would have to be multiplied by the power reflection coefficient which depends on the permitivity, permeability, and conductivity of the reflecting material.

A given reflecting surface may exhibit a reflection cross-section independent of wavelength for short wavelengths but show the $1/\lambda^2$ dependence at longer wavelengths. In this case, both area and curvature could be determined. For the purpose of determining the nature of the discrete reflecting surfaces as to size, curvature and electrical characteristics, it is essential that the radar reflection measurements emphasized above be carried out over a broad range of wavelengths from below one centimeter to above three meters.

As an indication of the dimensions involved, the two expressions given above for the reflection cross-section σ become equal when the area A equals that of the first Fresnel zone. This area corresponds to a spot diameter of about 300 m at 3 cm wavelength, or 3000 m at 3 m wavelength. A spot of this size will return a reflection equal to that of a smooth Moon.

The excellent theoretical development of T. B. A. Senior and K. M. Siegel (Journal of Research of N. B. S., Vol. 64D, pp. 217-229, 1960), faced with many inaccurate low-resolution and unstandardized measurements, was able to push through to some interesting quantitative conclusions by making certain bold a priori assumptions. Both the assumptions and the conclusions, however, are open to question, as the authors make clear. A program of radar reflection measurements such as that recommended above would make it possible to apply the theory with less ambiguity and more quantitative results.

In discussing lunar mapping by radar, Gordon H. Pettengill (Lunar Studies, Lecture 10 in "Radar Astronomy," by J. V. Harrington and J. V. Evans) makes these statements (pp. 25-26): "Measurements of gross surface properties can yield useful information, as has been shown above, but our understanding of the lunar surface would be vastly increased if it were possible to apply the techniques of analysis selectively to small portions of the lunar surface which could be correlated directly with visual observations." Also, "Thus, by carrying out a sufficiently accurate frequency analysis of the power returned at each increment of delay, one would be able to map the surface reflectivity of the Moon to within a twofold ambiguity."

Laser Reflection. In the preceding discussion of radar reflection, the need for the highest achievable angular resolution has been emphasized repeatedly. As the wavelength is decreased, however, and particularly below 3 cm, microwave radar techniques (power output tubes, waveguide circuitry and antennas) impose increasingly severe limitations on round-trip power obtainable. It has become increasingly clear in recent years that conventional microwave techniques could be pushed only so far, and that some radically new technique was needed to meet systems performance requirements at shorter wavelengths.

Under these conditions the recent invention of the laser was received by electronic systems engineers with unmatched enthusiasm. At the present time laser system engineering is proceeding in parallel with rapid component and technique development. In addition to many important civilian and military applications, the laser has already taken its place among the more promising techniques for the study of the Moon.

Dr. Louis Smullni of Lincoln Laboratories, Massachusetts Institute of Technology, has succeeded recently (not yet published) in detecting a laser reflection from the Moon, though at too low a signal level and too long a pulse length for quantitative measurements. Their transmitted energy was 50 joules, obtained from a single ruby crystal illuminated by a four-barrel flash lamp. Their received signal was twelve quanta. The transmitted pulse length of the order of one millisecond and illuminated area on the Moon of 2-mile diameter were too great to capitalize on the intrinsic precision of the laser technique, but the first big step has been taken.

The laboratory responsible for the first operating laser has now devised a new technique which has already yielded 20 megawatts peak power and a 10-nanosecond pulse length. On the basis of projects already underway, an increase of two orders of magnitude in power available at short pulse length would not be unreasonable. The 10-nanosecond pulse length corresponds to a distance of 10 feet, so range resolution better than this should be possible. Best atmospheric seeing would justify use of a 40-inch aperture, provide 1/2 second of angular resolution, and produce an illuminated spot on the Moon some 1/2 mile in diameter.

With sufficient power and greatly increased resolution in range and angle some very interesting possibilities are opened up for the study of lunar topography and figure. Since the transmitted light from the short-pulse laser is linearly polarized it becomes possible to make sensitive studies of the depolarization associated with reflections from different parts of the Moon. It would even be possible to learn a great deal about the figure of the Earth by making precision measurements of the distance to the Moon from various points on the Earth's surface.

Laser technology is moving forward with great speed and needs no encouragement from NASA. A laser system for lunar study, however, will be drastically different from a laser system for other purposes, and will require components specially developed for special functions. Therefore, to obtain a precision instrument designed to capitalize on the inherent capabilities of the laser principle, NASA should prepare specifications for a laser system for lunar studies. NASA should also encourage and support an active program for laser studies of the Moon.

A study of the feasibility of lunar laser studies leads to the following conclusions:

The study of lunar topography by laser techniques is a potentially very valuable and exciting addition to astronomy and exogeology. It can in principle do the job far better than any known competing method. The experiment is not quite feasible now by any reasonable equipment, definitely unfeasible on a single-pulse basis with optical telescopes.

Improving laser technology will permit a different conclusion probably within a year. Since the experiment requires much advance planning and preparation, the proper time to begin is now.

The experiment will not be simple, as astronomical experiments go, or particularly cheap as laser experiments go. A large receiver, a good-sized collimator, a superpower laser (1962 standards), perhaps significant data-processing equipment, and precision boresighting all will be required. Existing equipment can be used in most cases, and the cost will surely be much less than orbiting a satellite with either optics or radar.

Another experiment of great interest is the measurement of the lunar figure and tidal bulge. It is even possible conventional millisecond pulses could be used, though clearly not as desirable.

An early prototype experiment within capabilities that has high scientific and practical value for terrestrial geodesy is short-pulse laser ranging on a satellite of the Transit or Echo variety. The short distance (≈1000 mi.), and a large cooperative target would bring the experiment into the practical range. Triangulation between three stations in the United States for long arcs and for several passes would determine large-scale gravitational anomalies very accurately -- probably with an improvement of factor 10 over Transit. Simultaneous observations between several stations here and in Europe would fix the intercontinental distance an order of magnitude better than now known.

B.2 Lunar Topographic and Photogeologic Mapping

Hollis Hedberg

It has been recommended that mapping of the topography, geology, and other features of the Moon's surface be accelerated, and that increased emphasis be placed on the early completion of the Air Force LAC series of lunar maps and on the U.S. Geological Survey's photogeologic analysis. Photographic interpretation has already provided the major part of our available information on the geology and probable history of the Moon. The presently programmed Air Force Series of eighty-four 1:1,000,000 topographic maps of the visible portion of the Moon, of which only six are yet available, should be pushed to completion. The geological interpretation of one of these quadrangles by the U.S. Geological Survey (Shoemaker) demonstrates the large amount of information which can be obtained by modern photogeologic methods and the need for expediting photogeologic analysis of other selected quadrangles. The U.S. Geological Survey technique of combining photogeology of the Moon with detailed study of potentially analogous features on the Earth is one of the major sources of new ideas on possible lunar conditions, history, processes, and scientific techniques.

It has been stated that urgent objectives are general topographic and geologic maps of the entire lunar surface (both sides of the Moon) and detailed maps of certain areas of most critical significance. Among the most important contributions to the attainment of these objectives will be comprehensive high-quality television photographic coverage, with stereoscopic overlap, of the entire surface (both sides of the Moon) through lunar orbiters, and detailed television photography of local areas through landers. The program should place particular emphasis on providing adequate support for the large amount of interpretative study and map-making necessary to get the most information from both presently available photographs and those which may be obtained in the future, on assuring adequate Earth-based telescope facilities for those engaged in this work, and on assuring opportunities for comparative study of similar Earth and Moon features. In terms of useful information received per dollar spent, these topographic and geologic mapping studies have been and probably will continue to be among the highest yield investments of the space program.

B.3 Application of Geologic Data on the Earth's Atmosphere to Atmospheres of Nearby Planets

D. Wise and P. Abelson

With the advent of planetary probes, compositions of atmospheres of nearby planets have become more than academic questions. As a model for the mechanisms of atmospheric development the Earth is a good starting point. The materials of the present atmosphere represent only a small portion of the total volatiles which have passed through our atmosphere over geologic time. The components of former atmosphere now trapped in ocean and sediments arethe predominant components of the total atmospheric evolution.

A careful bookkeeping of all volatiles in atmosphere, ocean and sediments was compiled by Rubey (1951, Geol. Soc. America Bull.). The "excess volatiles," or those not accounted for by rock weathering, are calculated by Rubey in units of 10^{20} grams as:

H_2O = 16,600
All Carbon as CO_2 = 910
Cl = 301
N = 42
S = 32
Br, F, B, etc. = 13

It is Rubey's thesis that these "excess volatiles" representing the true atmosphere of the Earth developed slowly over geologic time by escape of volcanic gases from the interior. The proportions are about those measured for the composition of volcanic gases and the present rate of gas production is just about the rate necessary to produce the calculated weights over geologic time.

In addition to atmospheric losses by sedimentary and oceanic processes, the lighter components are escaping to space. Chief of these is hydrogen produced by photodissociation of water in the upper atmosphere with the heavier, more slowly moving oxygen remaining behind. This differential escape is the major source of atmospheric oxygen and the chief oxidizer of the outpouring, moderately reducing, volcanic gases (H_2, CO, S, H_2S). One of the limits to the rate of oxygen production is the rate of water transfer to the upper atmosphere.

Some major modifications are necessary to apply this model to adjacent Mars and Venus. The compositions of volcanic gases should be generally comparable to Rubey's excess volatiles, but the methods of removal from the system would be quite different. The absence of oceans on both planets argues against major precipitation losses by ordinary sedimentary processes. Much larger losses to space are likely because of lower escape velocity on Mars and because of greater temperatures and rates of atmospheric circulation on Venus. Presumably this greater rate of photodissociation of water and escape of H_2 is the reason for lack of oceans.

The rates of degassing and hence of volatile production of these two planets should differ considerably from those of the Earth. The high surface temperatures of Venus necessitate rock melting at shallow depths and hence a thin crust over a large liquid core. Degassing would proceed much more rapidly under these conditions and may be essentially complete at present. Mars, on the other hand, is cooler and smaller than the Earth, making convective movements unlikely and the probable rate of degassing much slower. Since rates of degassing and rates of differential gaseous escape need not be constant with time, the possibility of former oceans on each of these planets should not be ignored.

Spectrographic data on the Venus atmosphere indicate that oxygen is not a major component, a condition which might be expected if all the former water had been destroyed by differential escape of hydrogen. A greater percentage of oxygen may have escaped, but this route is clearly inadequate to account for all the missing oxygen. Some other major sink must be found if the basic model is to stand. The most likely sink is oxidation of surface materials.

Considering that the Venus atmosphere is at least an order of magnitude denser than the Earth's and that temperature differences of up to 200^o are available to drive atmospheric movement, wind erosion, transport, and sedimentation are probably the dominant geomorphic processes on the Venus surface. The surface winds should always blow toward the same region at the subsolar spot because of synchronous rotation of the planet with the Sun. The result should be piling of wind-blown sediments into that subsolar region with consequent sinking into the overloaded thin crust. Isostatic requirements would cause other crustal or core material to rise in the regions of dominant wind erosion. The pattern suggests deep seated density differences which would aid synchronous rotation of the planet as well as subcrustal flow patterns which should produce tectonic belts concentric about the subsolar region. Above all, the mechanism provides for continual exposure of new rock to the atmosphere and subsequent sinking back into the planet of any constituents which could react with the rock. Oxygen is the most likely candidate for reaction with the deeper reducing rock types and consequent removal.

The Venus and Mars atmospheres as deduced from a modified Earth atmosphere model would be composed of Rubey's "excess volatiles" minus most of the water. For Venus compounds capable of burial with the rock dust would be depleted also. The common statement that "Venus and Mars probably have nitrogen as their dominant atmospheric component because this is the dominant component in the Earth's atmosphere" is unwarranted considering the small percentage of nitrogen in the excess volatiles C, Cl, N, S, Br, F, B, H(?) and O(?) should be possible under the influence of intense solar radiation. The model provides some constraints on atmospheric components but does not preclude a variety of strange combinations.

Group C: Special Topics of Scientific Interest

C. 1 Lunar Sample Return

Edward Anders

Laboratory investigation of even a randomly selected lunar sample would provide a great deal of valuable information that cannot be obtained by any other means. If such information became available at an early date, it would not only lead to major advances in our understanding of the Moon's history, but would also provide valuable guidance for more sophisticated sample selection on the Apollo mission.

Information to be Obtained from Laboratory Investigation of Lunar Matter

Age determinations (Rb^{87}/Sr^{87}, Pb^{207}/Pb^{206}, K^{40}/Ar^{40}, $(U, Th)/He^{4}$). These measurements would give the time of crystallization and the time of cooling to a temperature of about 300°-400°K. They would establish the time when the Moon ceased to be geologically active, whether it cooled rapidly or slowly, and whether there was ever any appreciable reheating.

Cosmic-ray exposure history. Measurement of stable and radioactive cosmic-ray-produced nuclides will tell how long the sample was irradiated by cosmic rays. This will provide a measure of the erosion rate of the Moon.

Extinct radioactivity (I^{129}- Xe^{129}). These measurements may give the age of the Moon relative to other members of the solar system with an accuracy of about 10 million years.

Primordial noble gases. These abundances may give information on accretion conditions of the Moon, and the loss of volatiles during formation. With luck it may be possible to decide whether the Moon originally formed near the Earth or whether it was captured accidentally at a later time.

Stable isotope measurements. If the mineralogical composition of the sample is favorable, these methods can provide important clues to the nuclear, chemical, and thermal history of lunar matter, as well as the loss of water from the Moon.

Trace element content. These measurements provide information on the chemical history and the heat balance of the Moon.

Petrography.

Metallography.

Search for shock effects. These three types of measurements (7-9) provide clues to the formation conditions and the later history of the lunar crust.

Gross chemical composition.

Mineralogy. These studies will give information on the chemical, thermal, and pressure history, and may indicate the former presence of water.

Physical properties.

Organic matter. Such material might be of prebiotic or of biological origin. In either case, it would be of extreme interest.

Note: Only the last four types of measurements can be done by Surveyor. Of these, only the chemical composition can be determined with comparable accuracy. In all other cases, the laboratory measurements are more accurate, more diverse, more sensitive, and more conclusive, often by extremely wide margins.

C.2 Libration Point Satellites

L. Steg and E. Shoemaker

Introduction. The following is a discussion of the utilization of the libration points of the Earth-Moon system for scientific purposes. Specifically, consideration will be given to such factors as:

1. Scientific applications of a libration-point satellite and selection of the libration point.

2. Utilization of Venus-oriented space probes which will travel through the libration-point origin.

3. Characteristics of the vehicle's motion with respect to the libration points.

4. Launch and injection requirements for placing a vehicle at the libration point.

5. Propulsion requirements for maintaining a vehicle at a libration point.

Background. The restricted three-body model of the Earth-Moon system is defined as follows: the Earth and Moon describe circular orbits about their barycenter and the mass of the third body is negligible. If the disturbing forces of all bodies in the solar system are also neglected, the equations of motion of the third body in this model show that there are five points at which the centrifugal acceleration on a body revolving around the Earth with the same angular velocity as the Moon is balanced by the sum of the gravitational accelerations. These locations are commonly known as Lagrangian, equilibrium, or libration points. Thus, in a system rotating with the assumed constant angular velocity of the Earth-Moon line, a vehicle placed at a libration point with zero relative velocity will remain at this point forever in the absence of subsequent disturbances.

Two of the libration points, L_4 and L_5, form equilateral triangles with the Earth and Moon. The remaining points, L_1, L_2, and L_3, are collinear. All of the libration points lie in the plane of the Moon's orbit. Since the orbital plane of the Moon stays in the region of $\pm 5^{\circ}$ from the ecliptic, the libration points will be limited likewise. The configuration of these points is shown in Figure 5.

Scientific Applications. Because of their natural stability the L_4 and L_5 libration points appear to be advantageous locations for scientific satellites. It is interesting to note the recent unconfirmed discovery of faint luminosities in the neighborhood of these points by the Polish astronomer K. Kordylewski. Presently extensive investigations of these objects are being conducted by the Hawaiian Station of the National Bureau of Standards and the U.S. Geological Survey.

Sampling the material located at these points might reveal possible lunar or terrestrial origin. This would indicate that "lunar dust" collection might be executed without landing on the Moon, in fact even without establishing a satellite at L_4 or L_5. An Earth-satellite mission or a Venus mission (or in general an interplanetary mission) might be utilized for such a purpose. This might achieve dust collection, but certainly would not take the place of a libration-point satellite which would in addition measure radiation, solar flare characteristics, and the magnetic field, and could furnish valuable information regarding the Moon's mass. A few additional remarks are in order at this point.

Either of the triangular libration points would be an excellent location for a satellite whose objective is to perform long-term solar-flare observations. Besides the favorable long-lifetime characteristic of such a satellite at L_4 or L_5, it would also be free from the perturbations due to the geomagnetic field. A libration point satellite could also be used to gather data on radiation levels and magnetic fields in cislunar space. Information derived from the orbital motion of such a satellite would also be very helpful in determining improved values of the Moon's mass. Such information is essential since the error reflecting our present knowledge of the Moon's mass is approximately 0.1%. The only orbital observation necessary for improving this value is an orbital frequency measurement, and other details of the librational motion of the L_4/L_5 satellite are not essential.

It is interesting to note the advantages of a libration-point satellite (L-satellite) over artificial Earth satellites (E-satellites).

The L-satellite maintains the same relative configuration with respect to the Earth and the Moon; therefore it can be utilized as a communication base for lunar and interplanetary missions. The L-satellite by its nature has a very long lifetime which makes it ideal for long-term scientific observations. One of the main advantages is that it is located outside the immediate geomagnetic field, therefore it is far superior for performing solar-flare and charged-particle measurements than E-satellites are. Since the L-satellite undergoes only very short and infrequent solar eclipses it has the advantage of being in sunlight almost continuously, which simplifies the observing and power supply problem.

The L-satellite enjoys a view over nearly the entire celestrial sphere, while the view from E-satellites is limited. For astrometric purposes, such L-satellites are expected to be ideal for discovering very faint objects not at present included in star catalogues.

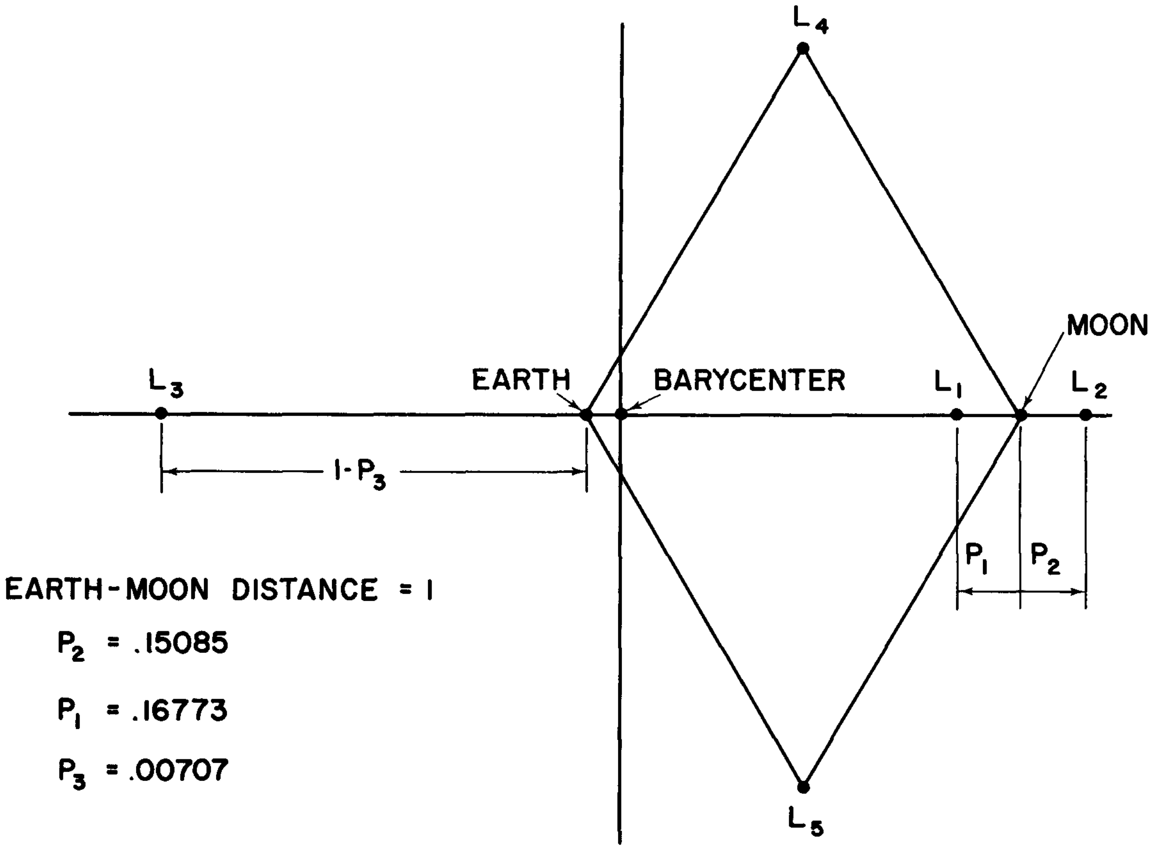

FIGURE 5.

Libration Points

In addition to these, L-satellites have several significant technological advantages over E-satellites such as the previously mentioned power supply problem, stabilization, altitude control, etc.

Motion Near Libration Points. A first-order analysis of the restricted three-body equations of motion show that motion about L_4 or L_5, resulting from small displacements in position and velocity, may be described by trigonometric functions and that the motion will be periodic and stable. On the other hand, the same analysis shows that hyperbolic functions are involved in describing motion with respect to the collinear points, corresponding to unstable equilibrium.

A restricted four-body problem can be formulated by including the effects of the Sun on the motion of the vehicle. The Sun is considered to move in a circular orbit in the Earth-Moon plane. In this model, however, there are no equilibrium or libration points. Analytical studies of the restricted four-body problem have been conducted by writing the equations of motion with respect to the three-body libration points. With this procedure, the influence of the Sun in the four-body problem may be thought of as a perturbation of the motions given by a three-body solution. The solution of the linearized restricted four-body equations of motion again shows that motion is stable with respect to L_4 and L_5, (of the restricted three-body problem) and unstable with respect to the collinear points.

In studying motion about the three-body libration points under the influence of bodies in the actual solar system, numerical methods have been used (i.e., the equations of motion were integrated numerically). It is the results of this study that will be most useful in determining the trajectory of a vehicle when placed at a three-body libration point; whereas the results of the restricted four-body analysis are most applicable to problems such as station-keeping feasibility studies and the determination of those initial conditions which define trajectories whose amplitudes are smaller than those obtained from the restricted three-body initial conditions.

Figure 6 shows the motion of a vehicle about L_4 as computed from the solution of the linearized restricted four-body equations of motion. The trajectory resulting from numerical integration of the restricted four-body equations of motion is shown in Figure 7. In both cases the initial conditions were those for the restricted three-body libration point and an initial Earth-Moon-Sun configuration of full Moon.

These curves show that the effect of the Sun on a trajectory which starts at the restricted three-body L_4 point is quite appreciable. Clearly these three-body initial conditions are not the "best" when the Sun is taken into consideration. Since the analytical and numerical trajectories are quite similar, it is reasonable to use the analytical solution to determine initial conditions. These conditions should yield trajectories of smaller amplitude than the three-body initial conditions would give when the Sun's perturbations are included.

Figure 8 shows a typical trajectory about L_4 compared with an N-body trajectory integration program using ephemeris positions for the Moon and Sun. In this case, the initial position vector of L_4 was equal in magnitude to the Moon's distance from the Earth at t_o, and its direction was 60° ahead of the Earth-Moon line. The initial velocity was that of the Moon but rotated by 60°. The above-mentioned initial conditions for L_4 were also used for the vehicle. Figure 8, depicting the motion in the instantaneous Earth-Moon plane, represents actual trajectory of the vehicle fairly well, since the deviations from this plane were found to be less than 1%. Some

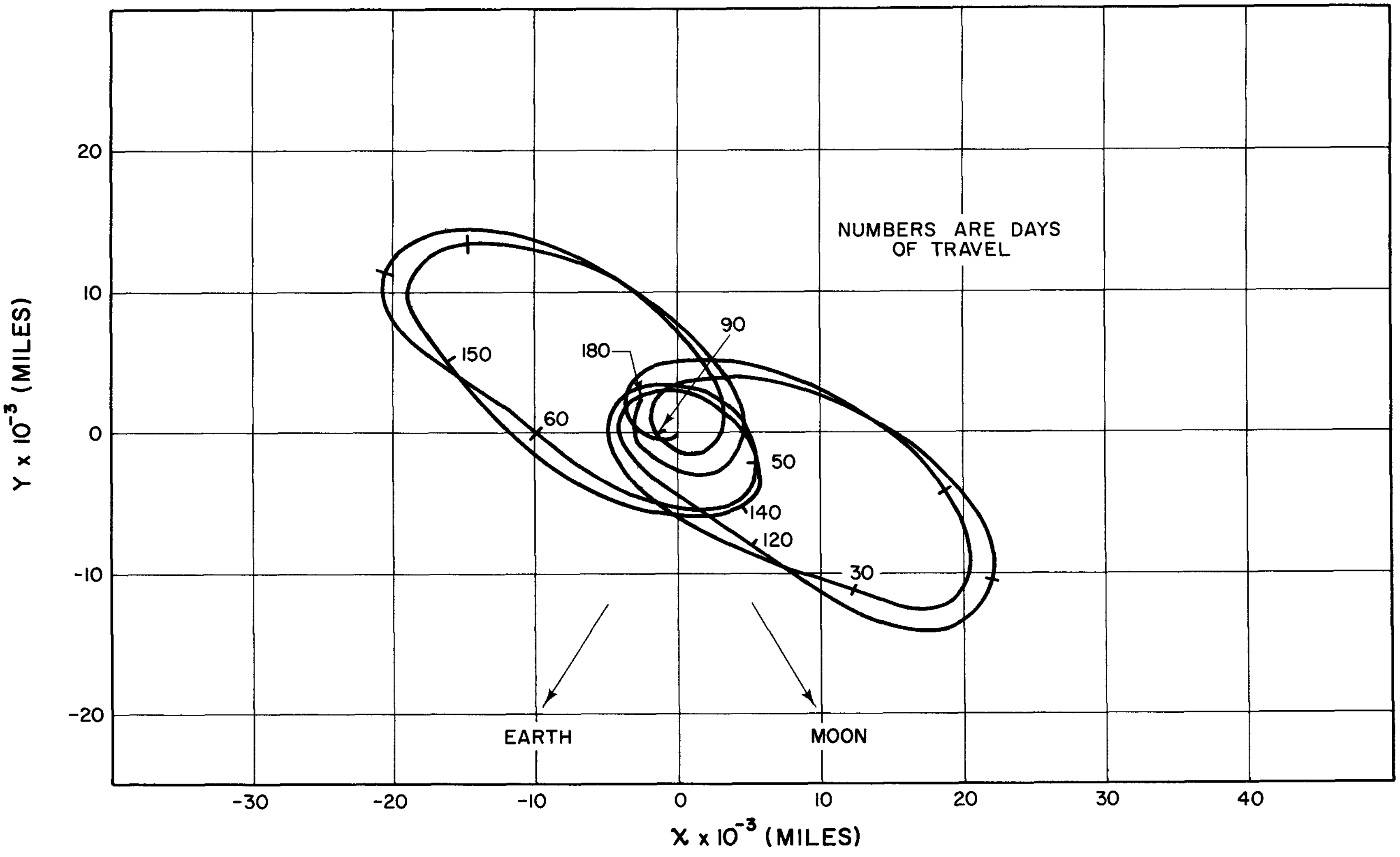

FIGURE 6.

Trajectory Near L_4 Obtained From Linearized Restricted Four-Body Equations of Motion.

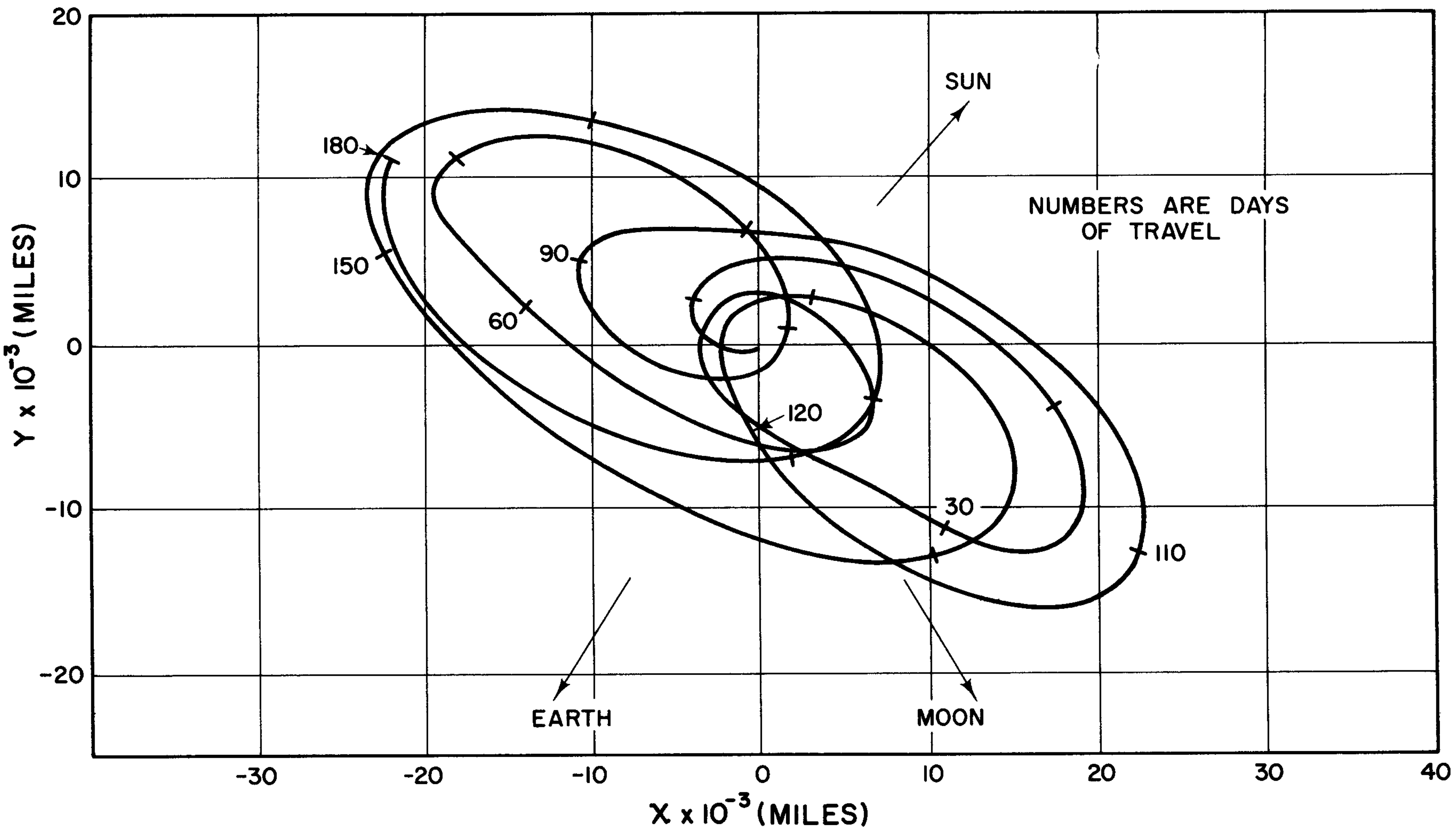

FIGURE 7.

Trajectory Near L_4 Obtained By Numerical Integration Of Restricted Four-Body Equations of Motion.

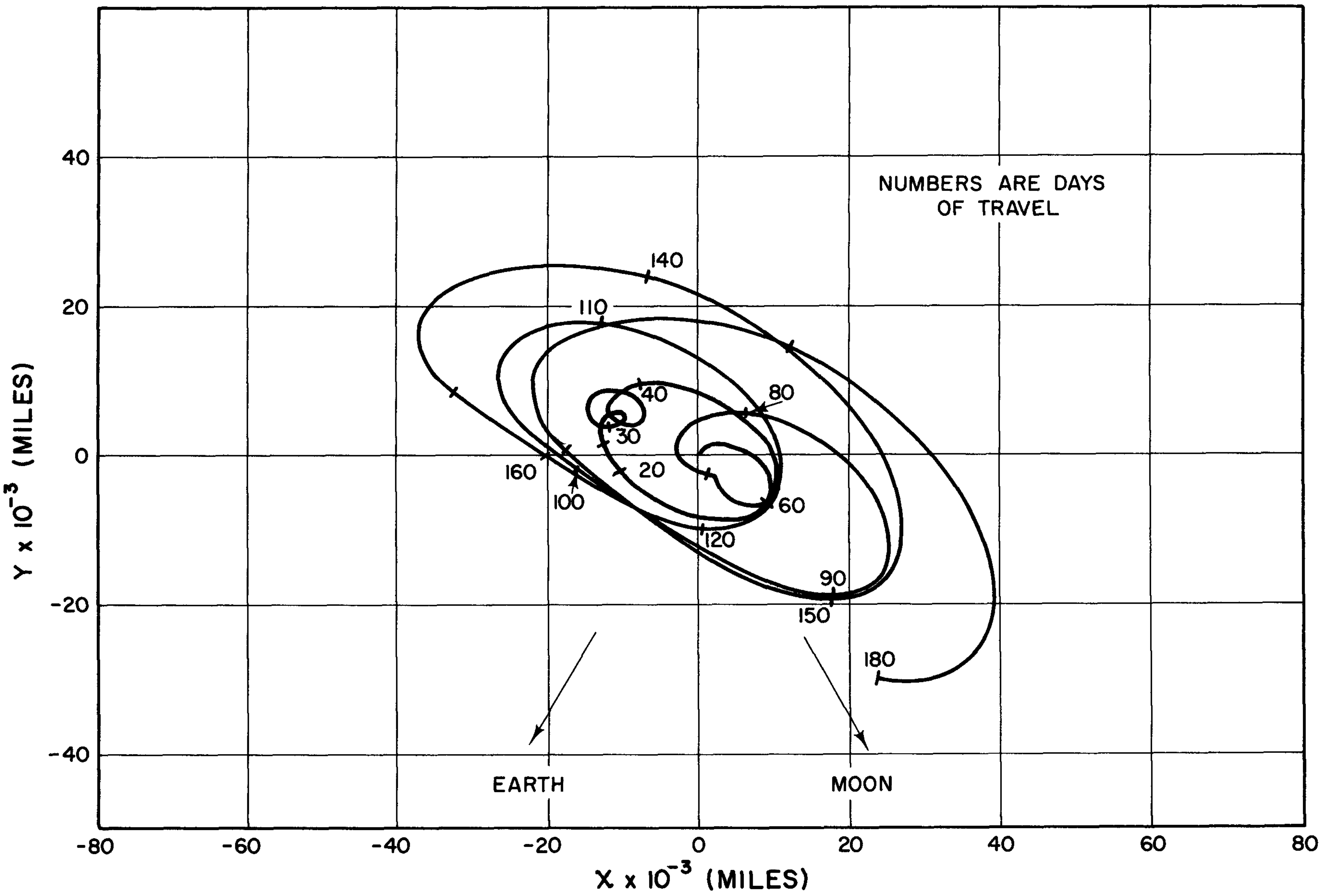

FIGURE 8a.

Motion Of Vehicle With Respect To L_4. Initial Velocity Equal To Moon's Velocity At t_o. t_o = July 2, 1964.

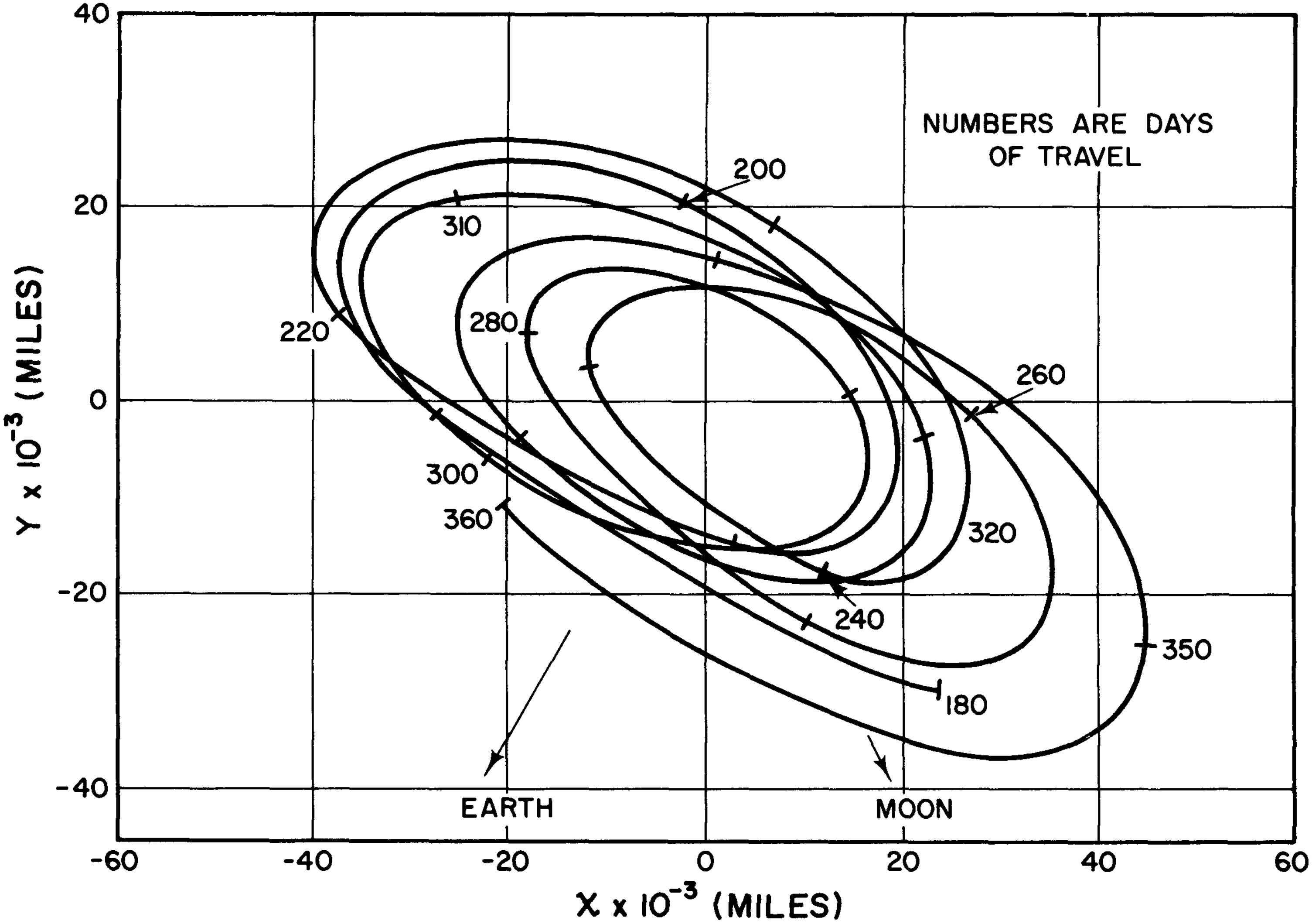

FIGURE 8b.

Motion Of Vehicle With Respect To L_4. Initial Velocity Equal To Moon's Velocity At t_o. t_o = July 2, 1964. Earth, Moon And Sun.

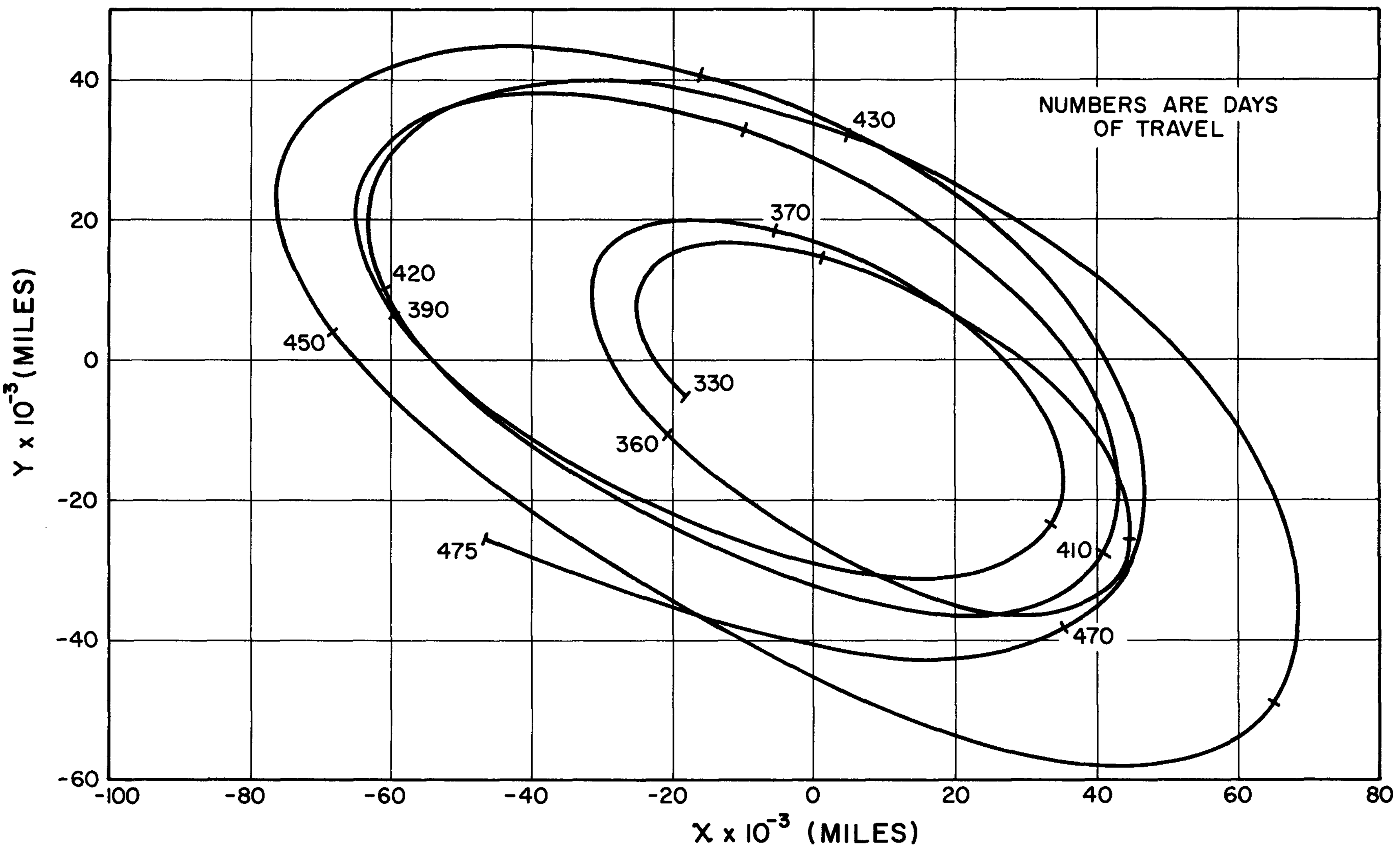

FIGURE 8c.

Motion Of Vehicle With Respect To L_4. Initial Velocity Equal To Moon's Velocity At t_o. t_o = July 2, 1964. Earth, Moon and Sun.

of the characteristics of this trajectory are similar to those obtained in the restricted four-body problem. The most noticeable difference is the amplitude of the motion.

It has been found that the perturbations of the planets of the solar system do not appreciably affect the motion over a period of several years. The motion near the L_5 point is similar to that near L_4, whereas the motion near the collinear libration points has been found to be unstable, as it is in the simplified three-body case.

Instruments, Weights, and Launching Problems. In this section the problem of accurately placing a vehicle at a libration point of the Earth-Moon system will be outlined. The basic task is to develop a set of transfer trajectories that will precisely satisfy a number of physical, engineering, and analytical constraints. In order to achieve this objective every phase of the flight regime must be examined in detail. This entails an analysis of:

(i) the powered ascent trajectory;
(ii) the ballistic Earth-to-libration-point transfer trajectory;
(iii) midcourse guidance and tracking;
(iv) terminal guidance;
(v) the libration orbit.

Specifically, in each of the foregoing phases an idealized nominal trajectory must be established that insures a perfect match in position and velocity at the end-point of each phase and the initial point of the next. Secondly, the accuracy with which the nominal trajectory can be prescribed must be evaluated. Finally, the effects that any deviation from the prescribed constraints will have on the actual trajectory must be determined. Assurance must be found that these effects can either be neglected or corrected, so that in the final analysis the integrity of the mission is not compromised.

The results of the foregoing analysis will clearly delineate the accuracy and precision with which the equipment employed (e.g., tracking radars, guidance propulsion, etc.) must operate.

Preliminary investigations have shown that an Atlas Agena B booster with a payload slightly less than 900 pounds will be capable of placing an instrument package of approximately 70 pounds at one of the triangular libration points. The rest of the payload includes an engine and propellant for midcourse and terminal guidance and for station keeping (360 lb.), structure (100 lb.) and equipment for attitude control (200 lb.), communication equipment (50 lb.), and power supply (90 lb.).

The instrument package might contain a micrometeorite collector and analyzer (30 lb.), a solar and cosmic particle measuring device (25 lb.), instrument for radiation survey (5 lb.), and a magnetometer (10 lb.).

A typical transfer trajectory will require a burnout velocity of 35,763 ft/sec at an altitude of 142 miles; burnout occurs on October 24, 1963, $12^{h}.00$ Universal time and the transfer time is 77.77 hours. The distance to the L_4 point at arrival time is 246,781 miles. For other dates and transfer times different trajectories must be computed, but this is not very sensitive with respect to the burnout velocity. By the use of a parking orbit in the launch phase a launch window of several hours will be available on every day. Figure 9 shows a transfer trajectory projected in the equatorial plane, which is close enough to the orbital plane of L_4 for the purpose of illustration. For injection into the L_4 orbit a velocity increment of 3, 720 ft/sec will be required. Some midcourse guidance will be required since the burnout error of a typical booster leads to an uncertainty in position of as much as 1000 miles at time of injection. Injection accuracy must be better than 10 miles in position and .5 ft/sec in velocity.

Station Keeping. Even when precise insertion has been accomplished (and this may of course be the result of a number of successive velocity corrections), the vehicle will tend to move away from the libration point due to the influence of the Sun, or (in general) due to all perturbative accelerations. These perturbative accelerations are the differences between the actual physical situation and the model which is used to compute the required initial conditions. For this model the three-body problem in which the Moon moves in an elliptic orbit has been chosen. For a vehicle at the triangular libration points the Sun's perturbative acceleration will cause the vehicle to move in periodic orbits around the point. For a vehicle near the collinear libration points the Sun's effect is to move the vehicle permanently away from the point, thereby increasing the natural instability.

The first purpose of any method of station-keeping is thus to counteract the Sun's accelerations. This may be accomplished in two essentially different ways: by continuous low thrust, as may be obtained from an electric propulsion device or a solar sail, or by a controlled on-off high specific-impulse device. For perfect station-keeping at the L_4 or L_5 libration points, preliminary analyses have shown that an average acceleration of 77×10^{-6} ft/sec^2 and a maximum of nearly 100×10^{-6} ft/sec^2 (about $3 \times 10^{-6}_{g}$) would be required from a low-thrust device. This may well be obtained by electric propulsion or solar sail. Propulsion requirements for high specific-impulse devices cannot presently be stated with any generality; specific examples, however, have indicated the feasibility of such systems.

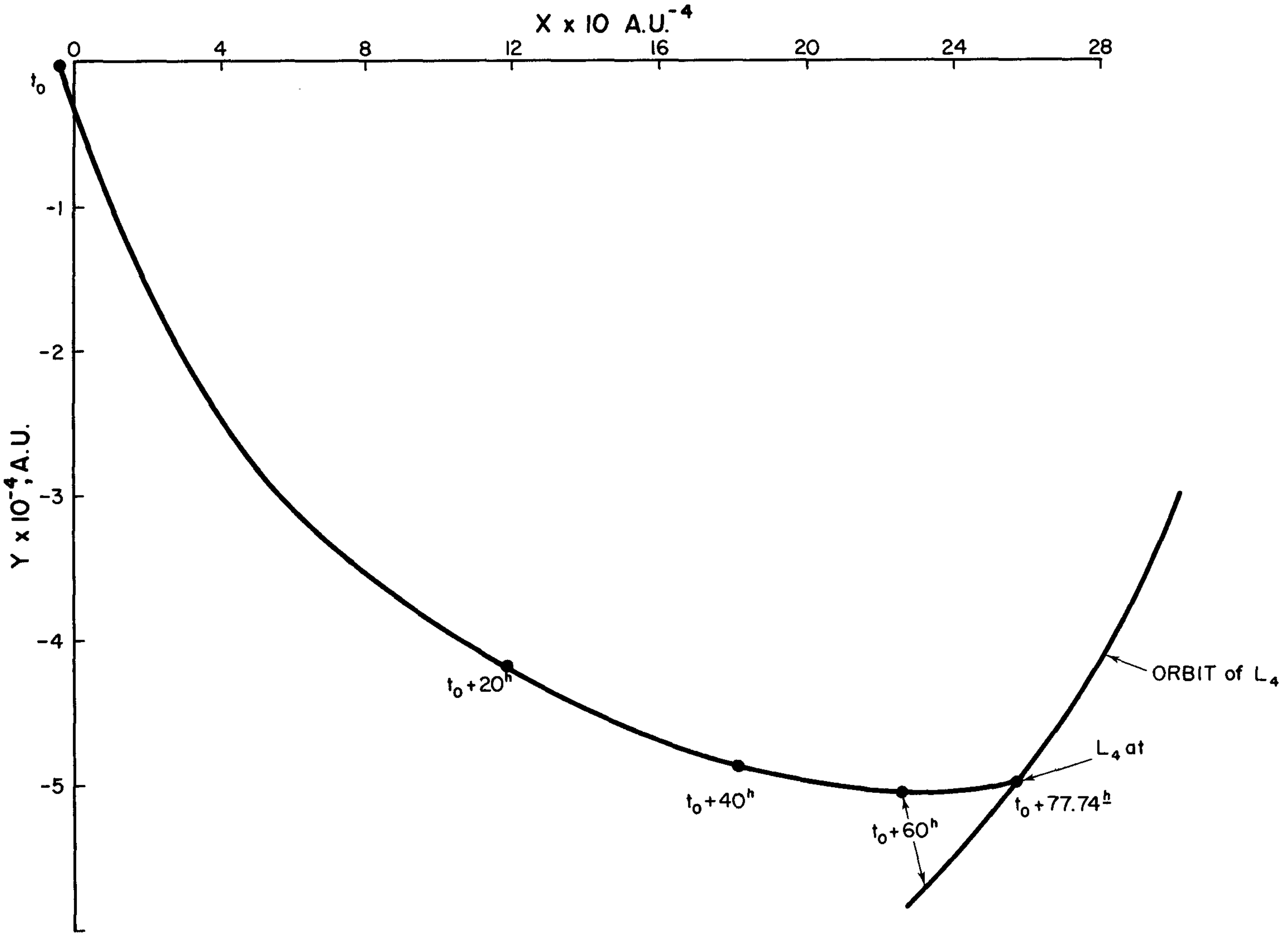

FIGURE 9.

Projection Of Nominal Trajectory On Equatorial Plane.

Appendix II

The principal participants in the Working Group on Lunar and Planetary Research included the following:

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J. R. Macdonald
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Chapter Five

ATMOSPHERES OF THE SOLAR SYSTEM*

I. Solar Radiation Incident Upon Planetary Atmospheres

Outside the Earth's atmosphere the solar spectrum extends from radio wavelengths down to X-ray wavelengths. In the visible region of the spectrum the radiation is roughly that of a black body at 6000° K and originates in the base of the photosphere. As we move into the ultraviolet region of the spectrum, however, emission occurs higher in the photosphere, where the temperature is lower. Around 1300 A to 1500 A the radiation corresponds roughly to a black body at 4700° K, which is the minimum temperature in the solar atmosphere and occurs at the top of the photosphere.

At still shorter wavelengths we are concerned with emission from the chromosphere and ultimately from the corona, where the temperature of the solar atmosphere reaches a value of the order of a million degrees Kelvin. The radiation from the chromosphere consists almost entirely of emission lines. The predominate emission lines are those of hydrogen and helium, the strongest being the hydrogen Lyman-α line at 1216 A. At wavelengths below about 1000 A we are concerned predominantly with emission from the corona. Disregarding detail, the shape of the solar spectrum between 10 A and 1000 A is not unlike that of a black body at a temperature of about 0.5×10^6 °K, corresponding to the lower part of the corona. However, the radiation in fact appears predominantly in lines and the integrated intensity is less than that of a black body at a temperature of 0.5×10^6 °K by about fourteen powers of ten. The X-ray and extreme ultraviolet part of the solar spectrum account for only about a millionth of the total energy emitted by the Sun.

Over a great deal of the solar spectrum there is surprisingly little variation of emission during the solar cycle. However, there are important variations at X-ray wavelengths. In the 44 A to 60 A band there was a sevenfold increase in intensity between the last sunspot minimum and the last sunspot maximum. From 8 A to 20 A the increase was by a factor of at least 45, and in the 2 A to 8 A band the increase involved a factor of several hundred. The extension of the X-ray spectrum to shorter wavelengths seems to require the existence in the corona of local hot spots with temperatures several times that of their surroundings. These hot spots are almost certainly associated with the plage regions around sunspots.

There are also transient outbursts of X-ray emission associated with the flares that are seen sporadically in the plage regions around sunspots using the red light of hydrogen H-α. With moderately large flares the normal X-ray spectrum

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*See Appendix I to this Chapter for list of participants in the Working Group on Atmospheres of the Solar System.

increases in brightness to several times the normal intensity, while at the same time the detectable spectrum is extended down to about 1 A. Large flares are also accompanied by cosmic rays of solar origin and by streams of plasma emerging from the Sun and causing magnetic storms and auroral displays at the Earth (see Chapter Seven, Particles and Fields).

At the radio end of the solar spectrum, radiation cannot emerge from deeper in the Sun than the level where the plasma frequency is equal to the observation frequency. At centimeter wavelengths the radiation comes from chromospheric levels, and at meter wavelengths from coronal levels. Increases in radiation from the Sun at the radio end of the spectrum show correlation with increases at the X-ray end of the spectrum. The radio end of the solar spectrum thus provides a ground-based method for observing those levels in the Sun that are responsible for the important changes that occur at the X-ray end of the solar spectrum.

> RECOMMENDATION: It is recommended that orbiting solar observatories be maintained to measure and monitor radiation from the Sun, particularly in the extreme ultraviolet and X-ray region of the spectrum (see Chapter Two, Astronomy).

II. Interaction of Solar Radiation with the Earth's Atmosphere

Ultraviolet radiation from the Sun between 2000 A and 3000 A is absorbed in ozone and leads to heating of the upper stratosphere. At a height of 50 km the temperature is comparable to that at the surface of the Earth. The temperature falls to a value less than 200°K at 80-85 km, after which it rises again to a value of the order of 1000°K at a few hundred kilometers. The cause of this high-level heating has not yet been satisfactorily identified, although there is little doubt that absorption of solar radiation in the extreme ultraviolet makes a contribution.

Above about 100 km mixing of the atmosphere ceases and gravitational diffusive separation of constituents becomes important. Moreover, ultraviolet radiation from the Sun between 1300 A and 2000 A is absorbed by molecular oxygen, leading to dissociation into atomic oxygen above about 100 km. Atomic oxygen cannot recombine effectively into molecules above 100 km, but must be diffused downwards to lower levels where it can recombine in three-body collisions. Conversely, molecular oxygen, which does not dissociate very much below 100 km, diffuses upwards into the region above 100 km, where it becomes dissociated. However, the rates at which molecular and atomic oxygen diffuse through the predominantly nitrogen atmosphere near 100 km are not known. In consequence, the relative concentrations of atomic oxygen, molecular oxygen, and molecular nitrogen above 100 km cannot easily be calculated, and since they have been measured only inadequately, they are still largely unknown. It is reasonably certain, however, that the oxygen in the atmosphere changes from the predominantly molecular form below 100 km to the predominantly atomic form above 150 km.

As a result of the transition to diffusive equilibrium above 100 km, the atmosphere at several hundred kilometers becomes predominantly atomic oxygen, the heavier molecular nitrogen decreasing in concentration more rapidly with altitude than the atomic oxygen. Moreover, although helium is a minor constituent of the atmosphere below 100 km, it is the predominant constituent at levels of the order of 1500 km, where its presence was discovered in the process of explaining the drag on the Echo satellite. At still greater heights, it is virtually certain that the atmosphere is predominantly atomic hydrogen, generated below 100 km by dissociation of water vapor.

Ionization in the upper atmosphere is produced by absorption of solar radiation having wavelengths less than about 1300 A. In particular, some of the radiation in the band from 850 A to 1300 A is absorbed in oxygen at E-region levels, although this is unlikely to be the principal cause of ionization in the E-region. The most prominent feature of the solar spectrum in this wave-band is the hydrogen Lyman-α line at 1216 A. Since this line coincides with a deep minimum in the absorption spectrum of molecular oxygen and the other constituents are relatively transparent, the solar Lyman-α radiation penetrates the atmosphere down to a height of about 80 km, where it appears to produce the daytime ionization of the D-region under quiet solar conditions. Actually, most of the energy of the Lyman-α radiation is used in oxygen dissociation, which is assumed to lead to the production of nitric oxide as a minor neutral constituent at D-region levels; it is ionization of the nitric oxide by the same Lyman-α radiation that almost certainly produces the normal D-region. It is quite unlikely, however, that such modest enhancement as occurs in Lyman-α radiation during disturbed solar conditions could account for the quite striking phenomena then observed at D-region levels, in particular the extension of D-region ionization to lower heights.

Solar radiation in the extreme ultraviolet range from 200 A to 850 A is almost certainly absorbed most intensely at levels mainly between 150 km and 200 km, producing the F-region by ionization of atomic oxygen. However, the actual ions existing at F-region levels are not simply determined by the primary photo-ionization, but depend quite crucially on secondary reactions, such as charge exchange and ion-atom interchange, leading to the formation of NO^+ as a rather abundant ion. The electron density in the F-region reaches its maximum value at a level significantly higher than the level of maximum electron production. This may be accounted for by means of an "effective" electronic recombination rate that decreases with increase of height more rapidly than the rate of production of electrons, together with the effect of diffusion.

The principal cause of daytime ionization in the E-region is almost certainly absorption of X rays in the band from 10 A to 200 A. Since the process involves the ejection of inner-shell electrons, the absorption coefficient depends on total air density and not on molecular association. Extension of the X-ray spectrum down to wavelengths below 10 A is the most probable cause of prompt ionization in the D-region during solar flares. Since X rays of wavelengths 2.5 A have about the same penetrating power as hydrogen Lyman-α radiation, lowering of the daytime D-region during intense flares would be caused by X rays of wavelength less than 2.5 A. On the other hand, nighttime ionization near the bottom of the ionosphere in polar regions following an intense solar flare is to be explained in terms of nonrelativistic solar cosmic rays, and the augmentation of this ionization during the daytime is to be explained in terms of electron detachment caused by normal solar radiation in the extreme ultraviolet.

At high levels in the ionosphere, ionization produced during the daytime would be expected to disappear only slowly during the night. At low levels in the ionosphere, on the other hand, it should disappear quickly if direct solar radiation were the sole agent. However, a little residual ionization in fact persists throughout the night. This probably indicates the presence of another source of ionization at night such as galactic cosmic rays or meteors.

III. Needed Measurements of the Earth's Upper Atmosphere

Because of the importance of oxygen dissociation and gravitational diffusive separation above 100 km, the neutral atmosphere at great heights is largely controlled by what happens in the levels between 85 km and 200 km. In this height interval the concentration measurements that are most acutely needed are those of O, O_2 and N_2. The height distribution of O_2 has been measured in the past with an optical absorption technique utilizing the Sun as a source, but the concentrations of O and N_2 have not been satisfactorily measured by any method. Atomic nitrogen could also be usefully measured, although theoretical arguments seem to give adequate assurance that it is certainly not a major constituent.

Helium and hydrogen should be measured because of their importance at levels above 1000 km. Although the presence of helium has been inferred from the drag data of the Echo satellite, direct detection of helium in the exosphere has not yet been achieved. What we know about neutral hydrogen in the exosphere has been derived from rocket observations of the absorption and scattering of hydrogen Lyman-α radiation emitted by the Sun. Since a good deal of faith can be placed in the theory of diffusive equilibrium, measurements of neutral helium and hydrogen may be made at lower levels than those where they predominate over atomic oxygen. It would be appropriate to make these measurements at levels of the order of a few hundred kilometers.

Besides an understanding of the neutral composition, we need to have quantitative knowledge of the ionized constituents of the atmosphere. Furthermore we need to know how the temperature of the neutral atmosphere varies with height, and one of the ways of doing this is to measure the temperature of the ions. This is an area of activity where both in-space and ground-based methods of observation are open to exploitation, and the question of how to divide effort between the two approaches has to be faced.

Throughout a good deal of the ionized part of the atmosphere, ground-based radio-sounding methods are now becoming available to measure the ionization density, the ionic temperature, and the electronic temperature (see Section VII). Radio-sounding methods are also potentially capable of measuring other parameters. These include the masses of the ionized constituents, although the feasibility of this particular measurement has not yet been demonstrated in practice. It is also true, however, that the same vehicular instrumentation required to measure the neutral constituents of the atmosphere can also be used to measure the ionized constituents and indeed can do so more reliably. Since vehicular measurements of the neutral and the ionized atmosphere have so much in common, the two sets of measurements should in general be combined into a single program.

IV. Importance of Interesting Laboratory Workers in Upper Air Measurements

In the past it has been assumed that, in the E-region, ionization of molecular nitrogen is largely irrelevant because recombination is so much faster than for molecular oxygen. Recent laboratory measurements imply, however, that the two recombination rates are comparable and that N_2^+ should therefore be as abundant as O_2^+ if their primary production rates are roughly the same. But mass-spectrometer observations in rockets show that the N_2^+ content is not more than 1% of the O_2^+ content. Something has been overlooked in either the laboratory or the atmospheric measurements, or else the primary ionization rates of N_2 and O_2 are surprisingly different.

It is virtually certain that there are many ways in which laboratory workers in the field of gaseous physics and chemistry, including spectroscopy, could assist in studying the upper atmosphere. This applies not only to measuring techniques and to the interpretation of measurements but also to theoretical predictions in aeronomy. For example, it was presumed for years that the ions in the E-region were N_2^+, O_2^+ and O^+ until mass-spectrometer observations in rockets showed that they are predominantly NO^+. The role of NO and NO^+ in the D- and E-regions of the ionosphere is still not understood and should be studied by all available methods. Intensive programs of theoretical study and laboratory measurements are required for the cross sections of most of the gases of interest in aeronomy.

> RECOMMENDATION: It is therefore recommended that no opportunity be lost to increase interest in atmospheric problems on the part of laboratory workers in the fields of gaseous physics and chemistry, including spectroscopy, and to support those of demonstrated ability and interest.

V. Airglow and Aurora

Ideas about the airglow are in a state of flux. The intensity of green airglow (5577 A) appears to have a maximum at about 90 km, although there is significant green airglow at substantially greater heights. On the other hand, the intensity of the overhead red airglow (6300 - 6364 A) does not change up to 150 km. Above 150 km no measurement has yet been made, and the emitting layer is thought to be located around 300 to 400 km. Moreover, arcs of red-line emission have been observed in temperate latitudes and in equatorial regions at high altitudes, and these observations are not theoretically understood. The N_2^+ airglow emission at 3911 A should also be investigated, particularly because it may be a good gauge of the excitation of the atmosphere by particle impact.

> RECOMMENDATION: It is recommended that observations of airglow be made on appropriate high-level rocket flights, and that these observations be directed particularly at the wavelengths 6300 - 6364 A and 3911 A.

Similar investigations should be made of the emission of auroral light. Furthermore it is expected that most of the energy of the aurora will appear in the ultraviolet spectrum between 1000 A and 2000 A, where it cannot be seen from the ground. It is important, therefore, to monitor the auroral ultraviolet luminescense of the atmosphere from above, and to ascertain how closely it is related to the input of particle energy.

> RECOMMENDATION: It is recommended that observations of the ultraviolet luminescence from the auroral zone be made in an

appropriate satellite having a polar orbit. The measurements should be made in the same vehicle used for detecting the particles that cause the aurora (see Chapter Seven, Particles and Fields).

VI. Rocket Aeronomy

The height range from 85 km to around 200 km contains, to a large extent, the key to aeronomy of the neutral atmosphere. Since this height is too low for satellite investigations, it follows that the aeronomy of the neutral atmosphere must be largely developed by rocket methods. However, the measurements are by no means easy, especially as one moves above 150 km, because of the contamination carried along with the rocket and because of recombination which takes place on the vehicle or apparatus, giving rise to an incorrect indication of the atom-to-molecule ratio. In addition to making measurements of ionic temperature, electronic temperature, and the intensity of airglow, rocket observations should be particularly directed at measuring the height distributions of O, O_2, N_2, NO, O^+, O_2^+, N_2^+ and NO^+. Mass spectrometry seems to offer the best hope for measuring these concentrations, although the optical absorption method should also be pursued. For the latter method simultaneous use of an orbiting solar observatory to monitor the variation of incident solar radiation during the time of flight of the rocket is advisable.

Flights should be adequately instrumented to measure all the important interconnected physical parameters of the atmosphere. In many experiments elaborate vehicles involving stabilized or oriented platforms are required. In the immediate future great importance attaches to flights aimed at obtaining a detailed quantitative understanding of a given vertical column of atmosphere.

On a longer time scale it is possible to look forward to the necessity for making aeronomy measurements at different latitudes, different times of day, different seasons of the year, different epochs in the sunspot cycle, and during both the presence and absence of unusual solar activity. A large number of rockets will then be required, and they must be capable of being fired from a number of locations having only modest range facilities. This will require the development of a reliable and predictable solid-propellent rocket, capable of carrying perhaps 10 kg of payload to a height of at least 150 km. The ultimate development of a suitable aeronomy rocket should be correlated with the more immediate development of a similar meteorology rocket for use from balloon altitudes up to about 100 km (see Chapter Eight, Meteorological Rockets and Satellites). The meteorology and aeronomy rockets together should cover the entire height-range from 30 km to roughly 200 km.

The importance of rocket investigations in the height range from 30 km to 200 km has recently been pointed up by the Committee on Atmospheric Sciences of the National Academy of Sciences in the following words:*

"The height range 30-200 km is of great concern to the aeronomist. It is in this height range that X rays and most of the ultraviolet rays and energetic particles incident upon the upper atmosphere are absorbed, that most of the airglow and auroras occur, that meteors burn up, and that high-frequency radio waves are most strongly

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*The Atmospheric Sciences 1961-1971, Vol. III, Goals and Plans for Aeronomy.

absorbed. It also includes the region through which any coupling that exists between the upper atmosphere and regions of concern to meteorology must exert itself, together with such diverse phenomena as noctilucent clouds and sporadic E. It includes the transition regions between the non-ionized and ionized regions of the atmosphere, and between a lower atmosphere of constant chemical composition and an upper atmosphere in which dissociation and diffusion control the composition. With all these (and many other) problems occurring in this height range, it is indeed unfortunate that such altitudes are too high to be reached by balloons and are too low for satellites. Although it may be anticipated that ultrahigh balloons and small dense satellites may narrow this range to perhaps 50-150 km, in situ measurements can only be taken over this range by means of rockets, or rocket aircraft of the X-15 or 'Dynasoar' type. Such measurements, typically involving highly sophisticated payloads, will be required in considerable abundance during the next decade if adequate progress is to be made, despite the high cost of rocket observations per minute of data.

''In addition to their ability to explore an otherwise inaccessible region of the atmosphere, rockets have the great advantage over other observational methods of providing detailed information as a function of height. Satellite, ground-based, and balloon observations can give only relatively coarse height profiles in comparison with those obtainable using vertical rocket probes; a significant number of rocket probes to considerable heights will therefore be required. In many cases more complex vehicles than are currently available will be required, involving (for example) stabilized or oriented platforms.''

> RECOMMENDATION: It is recommended that rocket experiments in the height range from 85 km to around 200 km be pursued to measure O, O_2, N_2, O^+, $O_2{}^+$, $N_2{}^+$, NO, NO^+, ionic temperature and electronic temperature. It is further recommended that the highest priority be given to measurements of O, O_2 and N_2. Both the mass spectrometer and the optical absorption methods should be used.

VII. Ground-Based Aeronomy

Revolutionary strides have been made in recent years in the ability of ground-based radio sounders to make measurements on the ionized constituents in the ionosphere and the magnetosphere. By the use of high-power pulse transmitters and highly directional antennas looking more or less vertically upwards, back scattering from the thermic fluctuations in ionization density are received that delineate ionization density as a function of height. Construction of one such aeronomy radar is virtually complete at Lima, Peru, where the Earth's magnetic field is practically horizontal. Construction of another radar of somewhat different design is under way at Arecibo, Puerto Rico. The Lima radar is already measuring ionization density up to a height of 9000 km and has not reached the limit of its capabilities. The extent of the breakthrough that has occurred in radio sounding of the Earth's outer atmosphere is illustrated in Figure 1, which is drawn roughly to scale. There is little doubt that magnetospheric radio sounders will become the principal means of monitoring ionization density in a large part of the magnetosphere.

Magnetospheric radio sounders are also capable of measuring the ionic and electronic temperatures as functions of height. The frequency spectrum of the back

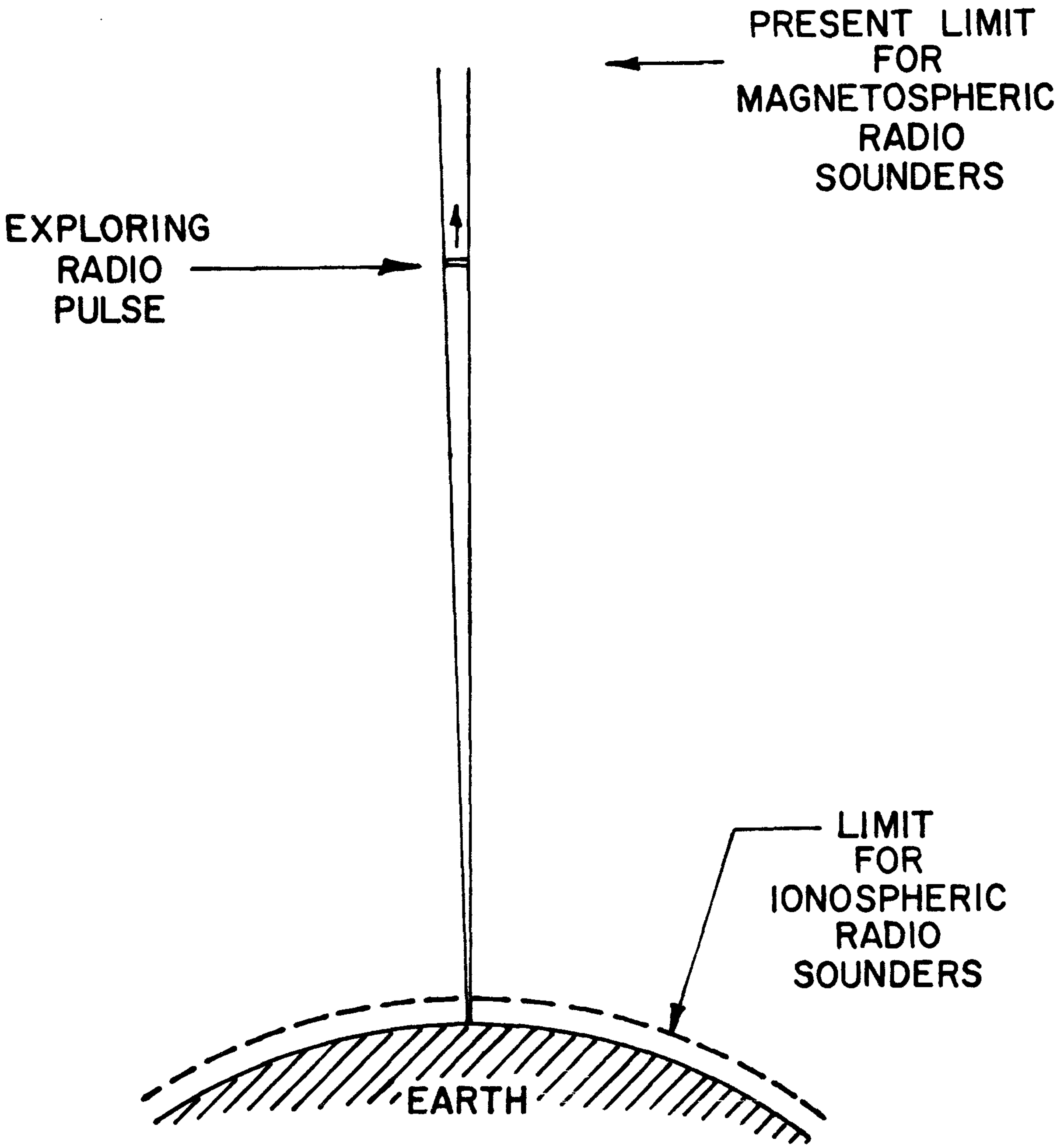

FIGURE 1

Magnetospheric Radio Sounding

scattering has a shape somewhat like that of a spectral doublet. From the overall width of the spectrum and from the degree of resolution of the doublet may be deduced the ionic and electronic temperatures. That the spectrum in fact has this theoretically predicted shape has been demonstrated experimentally by the Lincoln Laboratory, Massachusetts Institute of Technology, for back scattering from a height of about 300 km. It is not yet known to what height ionic electronic temperatures can be measured by magnetospheric radio sounders, but the maximum height is more likely to be in the thousands of kilometers than in the hundreds. If so, it will be possible to observe the changes from O^+ to He^+ to H^+ as changes in the spectrum of the back scattering.

Theoretically, by looking almost exactly perpendicular to the Earth's magnetic field, a magnetospheric radio sounder should see the spectrum of the back scattering break up into spectral lines whose separations are the ionic gyrofrequencies of the ions. In this way the ionic masses could be deduced for a given magnetic field strength, and variations in the strength and orientation of the magnetic field could be detected. How far this technique may be practical remains to be demonstrated with the vertically looking magnetospheric sounder at Lima on the magnetic equator, or with sounders that have steerable antenna beams that can be directed perpendicular to the magnetic field.

NASA has so far played no part in the development of magnetospheric radio sounding. At the time of NASA's inception virtually everyone took it for granted that progress lay almost exclusively in the use of vehicles for in situ measurements. Radio sounding of the upper atmosphere at that time was restricted to levels below about 300 km and existed primarily for predicting the performance of ionospheric radio communication links between different parts of the world. Involvement in this activity was certainly not in NASA's interest. Moreover, even the few people outside NASA who were thinking seriously about the development of magnetospheric radio sounding could not then reliably predict the success illustrated in Figure 1. In any case, what prevented NASA from playing a role in the development of magnetospheric radio sounding was its emphasis on supporting work that required a vehicle launched by a rocket.

Today the situation is somewhat different. One of NASA's primary missions is to put man in space and in particular on the Moon. This makes some parts of space more relevant to NASA's mission than others. The magnetosphere is clearly a part of space that NASA is under an obligation to investigate. In particular, NASA has plans under consideration to install a magnetospheric radio sounder at Wallop's Island.

There are some who argue that this move would open up the possibility of NASA support for all kinds of ground-based investigations of the universe. We do not think, however, that NASA will have any serious difficulty in distinguishing between ground-based observations of parts of space directly relevant to its primary mission and ground-based observations of remote parts of space where the relation to NASA's primary mission is tenuous.

Another argument frequently used is that a method of ground-based investigation, such as magnetospheric radio sounding, should be developed, but by a federal agency other than NASA. It is indeed quite possible that NASA may decide that a development such as magnetospheric radio sounding should be handled by transfer of funds to another agency, such as the National Bureau of Standards. The question of how to divide available federal funds between different approaches, however, especially between vehicular and non-vehicular investigations, needs to be handled by a single agency cognizant of the facts. For the part of space directly relevant to NASA's primary mission it is difficult to see that the overall responsibility can lie with any agency other than NASA.

> RECOMMENDATION: It is therefore recommended that, because of the relevance to its main mission, NASA support (by transfer of funds if appropriate) research into the technique of magnetospheric radio sounding and the installation of additional magnetospheric radio sounders, without regarding this action as committing NASA to all kinds of ground-based investigations of the entire universe.

VIII. Satellite Aeronomy

For the measurement of atmospheric hydrogen and helium, both neutral and ionized, mass-spectrometer techniques based on use of the high speed of a properly oriented satellite vehicle are most attractive. Orientation of the satellite increases the efficiency of making measurements by a large factor, perhaps a thousandfold. For the neutral component, it is not necessary to make observations at enormous heights, since reliance on theoretical extrapolations based on gravitational diffusive separation seems adequate. A circular polar orbit at a height of a few hundred kilometers would therefore be satisfactory. A lifetime as short as one week would be valuable if this is necessary to achieve constant attitude, although a longer lifetime is desirable.

For the ionized component of the atmosphere, extrapolation in height is dangerous because of the influence of the Earth's magnetic field and the effects of charge transfer. Circular polar orbits at various levels from a few hundred kilometers up to many thousands or even tens of thousands of kilometers would therefore be required. It should be noted, however, that the new developments in magnetospheric radio sounding described in the previous section will make big contributions to our knowledge of the ionized constituent up to many thousands of kilometers. It is nevertheless important that satellite measurements of the ionized component be made in the vehicles used for the neutral component. If comparison of satellite results with the new radio-sounding methods proves satisfactory, some of the money that might have been spent on satellites in higher orbits to measure ionized components should instead be spent on the development and extension of a ground-based radio-sounding network for the magnetosphere.

> RECOMMENDATION: It is recommended that satellite experiments using a circular polar orbit at a height of a few hundred kilometers (more circular than POGO) be pursued to measure He, H, He^+, H^+, O, O_2, N_2, O^+, NO^+, N^+, N_2^+, ionic temperature, and electronic temperature. It is further recommended that the highest priority be given to measurements of He, H, He^+, and H^+. The molecular beam technique for measuring composition on an oriented satellite is recommended.

IX. Space-Probe Aeronomy

Measurements of neutral and ionized hydrogen out to the edge of the magnetosphere and beyond could be made with space probes used for other investigations, such as those discussed in the following section and in Chapter Seven on Particles and Fields. A particularly interesting and relatively simple experiment would involve measurement of the diffused scattering of Lyman-α radiation from the Sun. This should be done as a function of the height of the probe and of direction as seen from the probe. This experiment has in fact been included in some of the Ranger payloads, but the measurements have not yet been made.

X. Planetary Aeronomy Using Space Probes

The two planets that we can look forward to exploring by space probes during the next decade are Mars and Venus. A report on the atmospheres of these two planets has recently been issued by the Space Science Board, and it is from this report that the following outline of possible experiments is taken (see "The Atmospheres of Mars and Venus," NAS-NRC Publ. 944, 1961). Additional planetary atmosphere experiments are mentioned in Section XII and described in Chapter Six, Bistatic Radar Astronomy.

RECOMMENDATION: We recommend that planetary observations and experiments of the kinds listed immediately below be carried out.

A. Early Fly-By Observations

1. Mars. Infrared spectra of both reflected and emitted radiation, with resolution to a fraction of a micron and with enough spatial resolution to distinguish among the major surface features.

Ultraviolet spectra down to about 1000 A with better than 10 A resolution, and with enough spatial resolution to distinguish limb effects and study the terminator.

Photoelectric photometry and polarimetry at a number of wavelengths from the near ultraviolet through the near infrared, providing enough spatial resolution to study limb and terminator effects. Note that phase angles greater than 43° cannot be observed from the Earth, so fly-by observations are essential to obtain the complete characteristic polarization curve for any part of Mars.

Magnetic fields.

Energetic particles, which would be associated with any radiation belt and would be coupled with the magnetic field.

Micrometeorites.

2. Venus. Same experiments as for Mars, plus: Radiometer to measure the microwave emission in the wavelength range from a

few millimeters to several centimeters, with enough spatial resolution to observe limb and terminator effects; and active radar.

B. Observations with Early Atmospheric Entry Probe During and After Descent

Pressure.

Temperature.

Density, which may be derived from other measurements during descent.

Radar altitude, or, alternatively, a continuous record of acceleration during descent.

Composition, e.g. quantitative tests for water vapor, carbon dioxide, oxygen, and ozone. This might be done by direct sampling, or by observing the absorption of sunlight at specific wavelengths as the capsule descends.

Measurements of sky brightness and polarization at several wavelengths in the visible and near infrared, to give total depth of atmosphere and distributions of clouds and haze.

Wind direction and speed, after landing only.

Succession of low-resolution TV pictures taken downward during descent (to identify the landing place, to indicate wind drift, and to give a hint of the detailed surface conditions in the landing area -- may be impractical below the cloud deck of Venus).

Biological experiments.

C. Observations with Advanced Entry Probes

Same experiments as above, plus: Continued monitoring of ''weather'' from the surface, with daily transmission directly back to Earth.

D. Observations with Advanced Orbiting Vehicles

Considerable advances will have been made in meteorological satellites and orbiting geophysical observatories in the next two or three years. This experience will serve as the basis for the design of orbiting observatories around the planets. Many of the fly-by observations described above will also be applicable. The purpose of such planetary orbiters would be to monitor the gross changes in the planet's atmosphere, to observe storm systems, map the general circulation, observe seasonal and secular changes in surface features, etc.

Many of the above experiments are included in NASA's plans for a series of probes to the planets in the coming years.

RECOMMENDATION: It is recommended that NASA's current plans for investigating planetary atmospheres by space probes be pursued on the highest priority.

Much of our knowledge of the planets has been and will continue to be based on lessons learned from studying our own planet. With this in mind, it is clear that no opportunity should be lost to test out planetary probe experiments from rockets and Earth satellites. In addition to serving as "field tests" for new equipment and techniques, these tests can be valuable scientific experiments in their own right, and in all likelihood will give vital information about our own planet.

XI. Planetary Aeronomy from the Earth or the Vicinity of the Earth

All of our information about the composition of planetary atmospheres, such as it is, comes from observing the spectrum of the sunlight reflected from the planet. The near infrared, at wavelengths shorter than about 1 micron, is by far the most commonly used, but the near ultraviolet and infrared beyond 1 micron are also observable. With the continued improvement of astronomical techniques, especially of the detectors used, these spectral observations can and should be refined.

A great deal of our information about planetary temperatures comes from observations of the far-infrared emissions. Here again, better techniques and detectors may permit improved observations of the total flux in the atmospheric window (8-13 microns) and of the far-infrared spectrum.

The Earth's atmosphere contains some of the same molecules as the atmospheres of the other planets (although in different proportions), so absorption and emission spectra must be carefully corrected for telluric absorption and emission. Some very important gases, notably water vapor and oxygen, are so abundant in our atmosphere that the Doppler shift technique is not enough to eliminate the effect of the terrestrial atmosphere. We must therefore get above the Earth's atmosphere by use of balloons or aircraft. The only measurement ever made of water vapor in the Venus atmosphere was from a balloon, and there is a need for much more of this kind of observation. Existing balloon instruments should be flown more often or duplicated so that more flights can be made (see Chapter Two, Astronomy).

One of the newest tools of the astronomer, the radio telescope, has already been of great value in observing the planets. By simply monitoring the microwave emissions from the planets, one may deduce the temperature. Since Venus is covered with clouds, we cannot "see" into the surface except with radio waves. Therefore, the only surface temperature observations come from the microwave radiometric measurements. Continued ground-based radiometric observations of Venus at a number of wavelengths, some corresponding to CO_2 and H_2O absorption lines, should be made in order to observe the change in apparent temperature with changing phase.

Using powerful ground-based radars, echos can be obtained from Mars and Venus. In this way it is possible to deduce a planet's radio reflectivity, its rate of rotation, its surface roughness, and even the locations of surface irregularities. Its ionospheric characteristics may also be determined. The state of the art of radar astronomy is such that the last three items are still either marginal or unachievable for the planets. But instruments are already being designed and built which may be able to do some of these things quite well. Such planetary radars can also function as lunar radars and as magnetospheric radio sounders. Moreover, in conjunction with a receiver in a space probe, ground-based radars can form an essential element in a bistatic radar system for investigating the planets as mentioned in Section XII and described in Chapter Six, Bistatic Radar Astronomy. With the development of these instruments, the current controversy about the rate of rotation of Venus should be brought to end.

In order to maintain continuous observation of the planets, observing facilities should be distributed around the Earth in longitude. This point should be borne in mind in locating new facilities. At the same time, it is highly desirable to locate new facilities for planetary research at places where active thinking about planetary science is in progress. Priority should therefore be given to locations where active planetary research exists and where at the same time the location would contribute to an international scheme for continuous planetary observations (see Chapter Two, Astronomy).

> RECOMMENDATION: It is recommended either that existing balloon telescopes be flown more frequently for observing the planets or that they be duplicated so that more flights can be made. It is further recommended that NASA support ground-based optical, radio, and radar observations of the planets in locations where active thinking about planetary science is in progress, and especially where at the same time the location would contribute to an international scheme for continuous planetary observations. NASA should not, however, regard this action as committing it to support of all kinds of ground-based investigations of the entire universe.

XII. Bistatic Radar Astronomy with Transmitter on the Earth and Receiver in a Space Probe

In all areas of space sciences there have been long discussions of the relative merits of ground-based and space-borne observations and experiments. The general feeling prevails that experiments fall into one category or the other — that the instruments are either here or there. However, there is a class of radar experiments which inherently requires both ground and space components. An almost trivial example is the use of beacon transmitters in a satellite together with receivers on the ground to measure the integrated electron density below the orbit. The deep-space version of this technique would involve high-power transmitters and large directional antennas on the ground combined with simple receivers in space. While expensive ground-based facilities would be required, they could be used for many different probe missions and could even be used as steerable lunar, solar, planetary, and magnetospheric radio sounders when no spacecraft were in operation.

High-powered radio transmissions from the Earth to a space probe can measure the integrated electron density between the Earth and any location within a vast volume of interplanetary space. Moreover, the receiver in space would obtain not only direct transmission from the Earth but also echos from neighboring bodies such as the Moon, the Sun, or one of the planets. In this way the solar atmosphere and the ionospheres of the planets can be investigated by simple receivers on space probes that do not even penetrate the atmosphere concerned. Moreover, besides information on the surface roughness of the Moon and the planets, information on the surface dielectric constant could be obtained by measuring the Brewster angle of incidence.

NASA's Ionospheric Physics Sub-Committee of the Space Sciences Steering Committee considered in February and May, 1961, the use of bistatic radar astronomy for investigating the ionosphere of Venus. However, no reference to this technique appears in the NASA long-range space sciences "Thinking Document" dated May, 1962. Furthermore, interest in the technique is not confined to investigation of planetary ionospheres. The possibilities inherent in bistatic radar astronomy have been investigated by a special working group composed of people interested in the solar atmosphere, in planetary atmospheres and ionospheres, in planetary surfaces, and in the lunar surface, as well as people knowledgeable about tracking and telemetry, and the results of their thinking have been recorded in detail in Chapter Six, Bistatic Radar Astronomy.

> RECOMMENDATION: It is recommended that NASA give support to the technique of bistatic radar astronomy using elaborate transmitters on the ground and simple receivers in space (see Chapter Six, Bistatic Radar Astronomy).

XIII. Guided Propagation of Electromagnetic Waves

Even today, our knowledge of the ionization density at geocentric distances of several Earth radii is confined largely to the equatorial region and is based almost entirely on quite simple ground-based observations involving electromagnetic radiation from lightning flashes at audio frequencies that is guided around the lines of flux of the Earth's magnetic field from one hemisphere to the other. Moreover, interaction between fast particles and the plasma in the magnetosphere appears to generate very low-frequency electromagnetic radiation that is observed at the surface of the Earth. Again, recent tests with topside ionospheric sounders at frequencies of the order of a few megacycles per second have given evidence of echoes that seem to be guided along the Earth's magnetic field, in addition to echoes at normal incidence from the ionosphere. These and other phenomena are believed to arise from guidance of electromagnetic waves along irregularities of ionization density that are aligned with tubes of magnetic flux. If it is possible, in certain circumstances, for tubes of magnetic flux near the Earth to be connected to the Sun, guided propagation of electromagnetic radiation along them may occur. It is necessary to remain alert for developments of this sort that may make new methods of investigation feasible, with either ground-based radio equipment or equipment installed in a space vehicle.

XIV. Regional Field Stations for Aeronomy

While the immediate need in rocket aeronomy is for sophisticated vehicles fired from the big national ranges, a number of objectives involving aeronomy will be attainable when it is possible to establish in North America about half a dozen aeronomy field stations suitably distributed in latitude. The following points should be noted:

(i) There will be a need for the development of simple aeronomical and meteorological rockets and of places from which to fire them (see Chapter Eight, Meteorological Rockets and Satellites, and Chapter Fifteen, International Cooperative Programs).

(ii) There will be a need for distribution in latitude in the use of both aeronomical and meteorological rockets.

(iii) There will be a need for distribution in longitude in the use of meteorological rockets, and possibly also of aeronomical rockets.

(iv) There will be a need for the establishment of magnetospheric radio sounders distributed in latitude.

(v) There will be a need for intercomparison of measurements made by rockets and by radio sounders (ionospheric and magnetospheric).

(vi) There will be a need for routine use of aeronomical and meteorological rockets.

(vii) There is already a need to make a host of special experiments with aeronomical and meteorological rockets, and many of these experiments could originate at universities.

(viii) There is a need to provide universities with easier access to aeronomical and meteorological rockets than is possible at the big national ranges.

(ix) There is a need in aeronomy and meteorology to provide for a cooperative program involving NASA, the Department of Commerce, the National Science Foundation, and the universities.

A great deal of progress in these directions can be made when it is possible to establish in North America about half a dozen aeronomical field stations suitably distributed in latitude. Each station could be equipped to fire the new aeronomical and meteorological rockets when they are developed. Each station could have an ionospheric and a magnetospheric radio sounder. While NASA would have to take the iniative in providing the sites, the rockets, and the firing arrangements, other government agencies could be involved in the provision of ionospheric and magnetospheric radio sounders and in the routine measurements. Moreover, if sites were appropriately located, each station could operate in association with a regional group of universities which might be responsible for conducting special rocket experiments under conditions likely to interest students in space science as a career. In this way, NASA would have an over-all responsibility for the operation while other interested agencies and the universities could play a substantial role.

RECOMMENDATION: It is recommended that NASA anticipate the necessity, in the long run, for aeronomical rockets that can be fired to an altitude around 200 km from sites with modest range facilities. This requirement should be borne in mind during the more immediate development of meteorological rockets and of sites from which to fire them. Such sites, if suitably distributed, could contribute to the solution of a number of physical and organizational problems, including relations with regional groups of universities.

Appendix I

Principal participants in Working Group on Atmospheres of the Solar System included the following:

H. G. Booker, Chairman
W. W. Kellogg, Vice Chairman
D. Alpert
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Chapter Six

BISTATIC RADAR ASTRONOMY*

I. Introduction

A bistatic radar system is one in which the transmitter and receiver are at different locations, in contrast to a monostatic radar system in which their locations are identical.

Monostatic radar astronomy studies are being conducted with transmitters and receivers on the ground, and can in principle also be conducted by means of instruments in space probes. An important class of radar astronomy experiments, however, inherently requires both ground and space components. As compared with the use of space components alone, this bistatic approach appears to have more scientific merit and flexibility, as well as greater technological feasibility. Large transmitters can be built on the Earth, while only a small receiver would be needed on the space probe. When not being used in the bistatic mode in experiments described below, the ground radar equipment could be used in other valuable ways. For instance, it would have considerable in common with space probe tracking and telemetering systems and could be so used, although it should be regarded primarily as a research system, divorced from the routine operational needs of deep space probe tracking. More importantly, the ground radar equipment alone could obtain strong echoes from the Sun, Moon, and planets and could also be used as a steerable magnetospheric radio sounder. Thus, both during flights of space probes, when the equipment operating in the bistatic mode would make a unique contribution to science in space, and at other times, when operating as a powerful monostatic radar, such a system would be in operation at all times as a research instrument and as a means of involving graduate students in space science.

The principal feature of bistatic radar experiments is the involvement of three bodies -- the Earth as a platform for a powerful radar transmitter and a large tracking antenna, the object of study (such as a planet), and a receiver on a space probe near the planet. Radar pulses would travel from the transmitter to the receiver on the probe by the direct path and also by a path reflected off the planet, like a carom shot in billiards. Several or many frequencies could be transmitted simultaneously to measure the dispersion of the propagating medium and other physical properties, e.g., the reflectivity or roughness scale of a planetary surface, which requires wide spectral coverage. The combination of signals that have followed the two paths (direct and carom) and their reception at the probe, together with the multiplicity of frequencies provide unique possibilities for: (i) the self-calibration of the measurements, since instrumental delay times and delays imposed on both signals by the interplanetary medium would be nulled out; (ii) the detection and measurement of effects that

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*See Appendix II of this chapter for list of participants in the Working Group on Bistatic Radar Astronomy.

depend on the angle of incidence and reflection and frequency (on Earth, with monostatic radar, we are limited to angles of incidence and reflection close to 90^{o}); (iii) occultation phenomena, in which a signal is detected and measured after traversing paths of varying length through a planetary atmosphere.

Objects of study in this class of radar investigations are lunar and planetary surfaces ("hard targets"), and planetary atmospheres and ionospheres, a possible residual lunar atmosphere or ionosphere, the solar corona, comets, and the interplanetary medium ("soft targets"). One might also include in the program precise range and range-rate measurements applicable to outstanding problems in celestial mechanics (e.g., the length of the astronomical unit in km; see Appendix VI of Chapter Three) and to the detection of certain relativistic effects not hitherto accessible.

The foregoing subjects are treated in more detail in Section II below, and in Appendix I.

II. Brief Outline of Possible Experiments

A. Experiments Using Space Vehicles

1. Planetary Fly-By. Group and phase path length measurements would determine the magnitude and time variation of the integrated electron density along both the direct and reflected path. The difference in the effects for the two paths would arise largely from the concentrated plasma (ionosphere) near the planet, while the direct-path measurement would give information on the interplanetary plasma. As the angle of incidence and reflection changes, an abrupt change in reflectivity and range on the reflected path would indicate a sudden jump in the level of reflection from the ground to the layer of maximum electron density in the planetary ionosphere. The shift in differential path length and the angle of reflection at which it occurs would give a measure of the height and density at this ionosphere maximum. Polarization and signal strength measurements would be used to deduce reflectivity and roughness parameters for the various reflection points on the planetary surface. A particularly sensitive measurement could be made of the Brewster angle (the angle of reflection where only the horizontally-polarized wave is reflected) which would provide a measure of the surface dielectric constant. Accurate range and range-rate data would help determine the planetary mass (especially for Venus).

2. Lunar and Planetary Orbiters. The same measurements described above could be carried out, with added weight on the ability to determine planet mass and moments, and the elements of the orbiter. In addition, the probe may be periodically occulted, so that measurements of the absolute and relative times of occultation at the various frequencies would give sensitive information on both ionospheric and atmospheric density, profile, and structure.

3. Deep Space Probes. With facilities which could be constructed now, the integrated interplanetary electron density between Earth and probe could be measured along paths that extend over essentially the whole of the solar system. Of special interest would be the value and change of the integrated density during times of solar activity. Also of special interest would be probe missions which could be fired through comets.

4. Solar Probe. A probe mission that goes near and/or is occulted by the Sun would make possible both direct- and reflected-path measurements of the density, structure, and dynamics of the corona (≥ 1.2 solar radii); the direct path could be used to measure the same quantities in the outer corona to the order of a hundred solar radii. The use of relatively low frequencies is probably of critical importance in these measurements.

B. Experiments Using Only Ground Facilities

Some radar studies of the nearest planets have been conducted with existing ground equipment. More powerful equipment is required, however, for more detailed measurements and for measurements at greater distances. A logical specification for the ground facilities is that the signal-to-noise ratio at the receiver be approximately the same at all frequencies of interest. This leads to the critical problem of obtaining high sensitivity at relatively low frequencies.

The percentage modification of radio signal characteristics by space plasmas (soft targets) often varies with frequency f as f^{-2}. Thus, for an accurate measurement relatively low frequencies are required. For studies of the Sun, it appears that attenuation in the corona may be so severe above about 50 Mc/s that radar echo studies are possible only at lower frequencies. Low frequencies are also required for studies of the characteristics of other targets. Since these characteristics will be dependent on frequency, they should be studied over a range of frequencies from as low as possible to as high as possible.

Cosmic noise varies approximately as $f^{-2.3}$, and antenna gain for a fixed aperture varies as f^2. Thus, it is difficult to obtain high signal-to-noise ratios at low frequencies. For example, while radar echoes have been obtained (on the Earth) from Venus at several frequencies from 440 Mc/s to 2300 Mc/s, it is not possible to detect this planet using several existing radars which operate at frequencies lower than 40 Mc/s, even though they have much greater power and antenna size than the higher-frequency systems.

It is important that the ground antenna beams be steerable, so that the planet can be illuminated at the time of probe occultation or during a critical fly-by period. If such critical times are controlled to occur when the planet is near the meridian of a tracking and command station, then the bistatic radar astronomy facility should be near that same longitude.

If the required facilities for the bistatic radar studies were constructed, they would also be uniquely suited for monostatic radar astronomy experiments. These experiments include lunar, solar, and planetary echo studies which would: (i) promptly provide intrinsically important scientific information; (ii) aid in planning probe missions designed to make more detailed measurements; (iii) provide more continuous observations to complement transitory fly-by measurements. In addition, many upper-atmospheric studies would be possible, particularly magnetospheric sounding experiments. The steerable antenna beam would be important in this last example for studying the magnetic field effects, such as the predicted ionic gyro resonances in the scatter spectrum. If these are detectable, it would be possible to have a "radar mass spectrometer" for measures of ionic species as well as electron density and electron and ion temperatures in the ionosphere and magnetosphere.

These ground-based aspects are particularly important in the training of graduate students in space science. Some experimental studies could be conducted at any time, with considerable expansion of these studies during flights of probes instrumented for bistatic experiments. A systematic program of observations involving fairly routine operation at many frequencies and the development and test of new experimental techniques and devices, would constitute a powerful method of advancing the art and providing many suitable subjects for graduate research.

III. Conclusion and Recommendation

Some aspects of bistatic radar astronomy have been discussed in various publications, meetings, and conferences during the past several years, including in particular the June, 1960, meeting at the Jet Propulsion Laboratory on electromagnetic studies of the Moon and planets using space probes, and the February, 1961, meeting of the Ionospheric Physics Sub-Committee of the Space Sciences Steering Committee of NASA, held at Stanford University. However, there has apparently not yet been a general appreciation of: (i) the broad range of applications of this technique for research in space sciences, and (ii) the importance of supporting research which could be conducted using the same ground equipment as a monostatic radar.

> RECOMMENDATION: NASA should give consideration to the broad applications of bistatic radar astronomy for space science research, and the supporting role of monostatic radar astronomy using the same ground-based facilities. The initial steps aimed at developing and building more powerful ground-based equipment for these applications should be taken as soon as possible. In due course NASA should fund instrumentation of space probes for use in connection with the ground-based radar research equipment.

Appendix I

Bistatic Radar Astronomy: Some Numerical Examples

Measurements Based on Group Velocity. Free electrons in interplanetary and ionospheric plasmas slow pulses of radio energy so that they require 40 $Nc^{-1}f^{-2}$ seconds more to propagate between two points than they would have required in a vacuum. In this expression, N is the number of electrons in an imaginary column extending between the two points and having a cross-sectional area of one square meter, c is 3×10^8 ms^{-1}, and f is the radio frequency in cycles per second. Since this extra delay is frequency-dependent, the use of two frequencies in a measurement makes it possible to compute the columnar content directly from the difference in time of arrival of two simultaneously transmitted pulses.

The vertical columnar electron density of the Earth's ionosphere varies from about 10^{16} to 10^{18} m^{-2}, depending primarily upon solar activity, latitude, season, and time of day. The following table illustrates the extra time of propagation of pulses at several frequencies. The ionospheric effect is shown for an assumed columnar density of 10^{17} m^{-2}. The effect of the interplanetary medium over Earth-Moon and Earth-planet distances is shown for an assumed volume density of electrons of 25×10^6 m^{-3}, or 25 cm^{-3}.

Table I

Frequency in Mc/s	Extra delay in microseconds		
	Earth's Ionosphere	Earth-Moon Interplanetary Gas	Earth-Planet Interplanetary Gas
12	100	10	>1000
37	10	1	> 100
120	1	0.1	> 10
370	0.1	0.01	> 1

The table illustrates the importance of using relatively low radio frequencies for the interplanetary gas to have a measurable effect upon propagation delays. Schemes for measuring extra delays on the order of microseconds would be relatively easy, but it would be difficult to detect extra delays on the order of tenths or hundredths of a microsecond.

The table also illustrates that the average interplanetary gas density could be measured with good precision over interplanetary distances, even if the ionospheric contribution were not known very accurately. However, for distances as short as that from Earth to Moon, the ionosphere might mask the effects of the interplanetary gas unless the ionospheric content were known with good precision. Fortunately, there is an independent method (based on Faraday polarization changes in a plasma in a magnetic field) for measuring the ionospheric content alone. Experience from Moon-echo and satellite-monitoring experiments indicates that it can be determined with a precision of several per cent. Thus, it would appear that measurements of the average density of the ionized interplanetary gas between Earth and space probes as near as the Moon might be made with fair precision if polarization measurements were made on the same signals to determine the ionospheric contribution to the total group velocity delay.

Since the proposed method of measuring the content of planetary ionospheres is based upon the difference of the content along the direct and reflected paths, it would appear that a planetary ionosphere having a columnar content of only about 10^{16} electrons m^{-2} could be detected by this group-velocity method.

The height and maximum density of planetary ionospheres might also be determined. Assume for illustrative purposes that the ionosphere of Venus has a maximum electron volume density of 10^{13} m^{-3} (10^7 cm^{-3}) at a height of several hundred kilometers, and that a radio frequency of 37 Mc/s is being used. When the angle of incidence for specular reflection to the space probe increases to about 40^o, the reflection point will shift abruptly from the surface to the level of ionization maximum density, and the time delay along the reflected path will decrease several milliseconds. The angle at which this discontinuity occurs is related in a simple way to the value of the maximum electron density in the planetary ionosphere, and the angle together with the value of the discontinuity in the time delay is related to the height of the layer and the distribution of electrons above the maximum.

Measurements Based on Phase Velocity. When the columnar content of electrons over a fixed path changes with time, the phase velocity of radio waves changes so that the frequency of the received wave exceeds the transmitted frequency by $40\,\dot{N}c^{-1}f^{-1}$ cycles per second, where $\dot{N}$ is the time rate of change of the columnar content in electrons in $m^{-2}s^{-1}$. To measure this small change in frequency at a receiving site, and to cancel the effect of relative motion of the two terminals, one would simply transmit harmonically related frequencies and note the departure from a precise harmonic relationship at the receiver. This has been done in satellite studies of the Earth's ionsophere.

At a radio frequency of 40 Mc/s a change of one cycle per second corresponds to a change of 3×10^{14} electrons m^{-2} s^{-1} in the path between transmitter and receiver. Normal diurnal changes in the ionosphere and ionospheric changes during magnetic storms are only about a quarter of this value, as has been determined by Moon-echo and satellite measurements. Here again there is an independent method of determining changes in the ionospheric content, so that it would appear that the sensitivity of this method at 40 Mc/s is such that electron content changes as small as 10^{13} m^{-2} s^{-1} in interplanetary space and near other planets could certainly be detected. For a planetary orbiter or fly-by, changes in electron content along the reflected path will result primarily from the change in obliquity of the incident ray (i.e., path length through the ionosphere in and out again); this method might therefore be made to yield a more precise absolute content for the planetary ionosphere than the group-velocity delay method.

For the interplanetary gas, the foregoing figure for the sensitivity threshold corresponds to average volume density changes of less than 0.02 cm^{-3} s^{-1} over

Earth-Moon paths, and less than 0.0002 $\mathrm{cm}^{-3}\ \mathrm{s}^{-1}$ over paths to the planets. It would appear that the effects of solar streams intercepting these paths would be easily detected.

Lunar and Planetary Surface Measurements. Ground-based radio and radiometric measurements of the Moon have led to divergent conclusions regarding the surface dielectric constant. Suggested values for the relative dielectric constant, ε_r, are grouped either between 1.1 and 1.5, or in the neighborhood of 3.0. These different numbers imply widely different material and structure for the surface of the Moon. It appears that bistatic radar offers a sensitive technique for resolving this uncertainty.

By way of example, consider the different intensities of the polarized components reflected toward a lunar orbiter. For a smooth spherical Moon, the reflected component which is polarized in the plane of propagation goes to zero at an incident (Brewster) angle of about 50° for $\varepsilon_r = 1.5$ and at 60° for $\varepsilon_r = 3.0$. $(\theta_{\text{Brewster}} = \tan^{-1}\sqrt{\varepsilon_r})$ For a near-lunar satellite, about 200 seconds of time would elapse between reception of waves whose angles of incidence differ by 10°.

The Moon, of course, is not smooth. Ground-based radar measurements indicate that average slopes of 3-5° exist over scales on the order of a kilometer, and about 5% of the surface appears rough at meter scales. These effects would blur the Brewster angle measurement. However, the dielectric constant affects not only the value of this critical angle, but also the strength of both polarized components at every angle of incidence. Thus by averaging the measurements for both components at all angles and comparing with theory, it appears that the dielectric constant could be determined to an accuracy of a few per cent. In addition, the spread in the measurements at a particular angle would yield information about the lunar roughness in the region of reflection.

If the lunar surface consisted of a lossless dielectric, the reflectivity measurements would be the same at all radio frequencies. Conversely, by noting the differences of reflectivity over a wide range of frequencies, it should be possible to determine the conductivity of the lunar surface material. The use of a wide range of frequencies would also make it possible to determine the surface roughness at various scales corresponding to the various sensing wavelengths.

Ground-based (monostatic) radar studies of Venus indicate that it may be somewhat smoother than the Moon, and have a higher dielectric constant. No radar echoes have yet been obtained from Mars. Future bistatic and monostatic radar studies of these planets would aid very greatly in determining their surface characteristics before any instrumented landings.

Occultation Experiments. The most sensitive determination of the upper limit for the density of the lunar atmosphere is based upon measurements made of the radio occultation of the Crab Nebula. The structure and density of the solar corona out to distances from the Sun of the order of a hundred solar radii have been inferred from radio-star occultation measurements. Our knowledge of the scale height of the upper atmosphere of Venus is based upon the optical occultation of the star Regulus by this planet. It is apparent that much more could be learned about the solar corona and lunar and planetary atmospheres and ionospheres from radio occultation experiments using a wide range of frequencies. The use of coherent controlled-polarization, point-source, harmonically related signals has obvious advantages over experiments based upon the occultation of extended natural noise sources. These features should be studied in detail in preparation for possible bistatic radar experiments with the receiver in lunar, planetary, and solar orbiters.

Appendix II

Principal participants in the Working Group on Bistatic Radar Astronomy included the following:

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Chapter Seven

PARTICLES AND FIELDS*

I. Foreword and Summary Recommendations

This Chapter will (i) examine the state of knowledge and the state of ignorance of that component of space science dealing with particles and fields; (ii) in the light of this examination, evaluate NASA's program in this area; and (iii) make recommendations on vehicles, on orbits, and on operational policies deemed desirable for the effective pursuit of this field of space science.

FINDING: Overall, the NASA space science program in the area of particles and fields is one which has already yielded substantial results. The existing plans, assumed to be going forward, are endorsed. The succeeding findings and recommendations relate to the bolstering of areas which appear to have been neglected.

FINDING AND RECOMMENDATION: The important geostationary (24-hour period) satellite appears to have been unduly delayed. As outlined in the body of the report, there are many vital experiments in the particles and fields area which await the availability of a satellite in an approximately geostationary position. It is recommended that a 24-hour orbiter be scheduled at an early date, without waiting for the development of the Centaur.

FINDING AND RECOMMENDATION: There are a number of important uses for an accurately circular, polar orbit at an altitude of about 300 km, viz.: (a) further investigation of the recently discovered low-energy electrons at low altitudes over the central Pacific (see Section V. A. 10 of this Chapter), (b) comprehensive auroral observations which are homogeneous in altitude, and (c) measurements of the neutral atmosphere (see Chapter Five, *Atmospheres of the Solar System*). It is recommended that NASA schedule a gravitationally oriented satellite into an accurately circular orbit at an altitude of about 300 km. A satellite of the Discoverer type is probably suitable; no recovery is required.

FINDING: The general concept of block allocation of payload space to groups of proven competence and the application of this concept to both small and large satellites are discussed in detail in Chapter Fifteen. It is felt that particle and field measurements, in particular, would flourish under such a policy, since carefully coordinated systems of detectors are required in order to obtain fully significant results.

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*See Appendix I for participants in the Working Group on Particles and Fields.

FINDING AND RECOMMENDATION: Despite the fact that NASA is sponsoring significant observational programs on the terrestrial aurora and airglow, the Working Group noted that no such programs are listed in the "NASA Long Range Space Sciences Thinking Document." It is recommended that this omission be remedied in revised issues of this document.

II. Magnetic Fields in Space

A. Extent and Significance of Magnetic Fields

It appears that most if not all of the universe is pervaded with magnetic fields. Their existence is known by direct measurement on and near the Earth and inferred from a variety of evidence on other planets, on the Sun and other stars, between the planets, and in interstellar space. The configuration of these fields is of interest for the information that they give about the electric currents which produce them which are usually inaccessible to more direct measurement, and because of their effects on charged particles in space. They may provide acceleration or deceleration, guidance and deflection, and intensity modulation of the particles.

Three principal types of magnetic fields are of interest in space research: (i) planetary and lunar fields (including the Earth's), (ii) the solar field, which fills the inner part of the solar system, and (iii) the galactic field. Transition regions between these fields are also of particular interest.

B. The Geomagnetic Field

The geomagnetic field, in its gross features, is similar to the field of a uniformly magnetized sphere. Such a simple model is inadequate in detail, however, for two principal reasons. First, the relatively steady mean field contains, in addition to its predominant dipole component, important contributions from higher multipoles and numerous irregularities of local origin. The most accurate simple representation of the field is given by the eccentric dipole, which is both tipped relative to the Earth's axis and displaced from its center. Second, the field varies continuously with time in an irregular manner.

The Earth's main magnetic field is commonly supposed to originate by dynamo action in the fluid motion of the molten metallic core of the Earth. This fluid motion is unstable; it changes slightly from year to year to produce the secular variation, which requires hundreds of years to produce a significant change in the geomagnetic field. Transient variations, which take place in times less than one year (some occurring in a small fraction of a second) have their sources outside the Earth, and are produced chiefly by the interaction between solar plasma and the geomagnetic field.

Scientific observations of geomagnetic field have been made for the past several hundred years. For example, the secular variation was discovered in 1635 by means of data obtained as early as 1580. The transient variations were discovered in 1722. The first magnetic observatories were constructed during the late eighteenth century for the purpose of making systematic observations over widely separated geographic positions. Since that time, enormous amounts of data have been gathered. An outstanding job of describing, summarizing and analyzing the data to 1940 may be found in the two-volume set, Geomagnetism by Chapman and Bartels.

It is customary and convenient to specify the geomagnetic field by means of its spherical harmonic analysis. Many such analyses have been made since Gauss calculated the first one a century ago. The generation of new analyses every few years is the best way to specify the secular variations in the field.

Past harmonic analyses have been calculated from data collected at a large number of magnetic stations on the surface of the Earth. With the advance of space technology, it should soon be possible to acquire the necessary data more quickly and with more nearly uniform world-wide coverage by means of a satellite magnetometer in a low polar orbit. Such a mission is included in the NASA program with the POGO satellite in 1964. If such a survey is to have maximum utility, the requirements for accuracy in the determination of the orbit and the orientation of the satellite are severe.

> RECOMMENDATION: Therefore, for subsequent magnetic survey missions, consideration should be given to using the Transit Doppler system for position measurement and a precise orientation system such as that being developed for the OAO.

Magnetic surveys at higher altitudes can reveal the presence of permanent and transient electric currents within the magnetosphere. These measurements will be made by various satellites in the NASA program. When magnetic anomalies are discovered, it will be important to be able to investigate them in greater detail with high-altitude sounding-rocket missions and perhaps also with special-purpose satellites having made-to-order trajectories.

C. Geomagnetic Field Variations and Storms

Research on the geomagnetic field since 1940 has been directed mainly toward an understanding of the transient variations. The greatest progress in this direction has come about through applications of the principles of hydromagnetism. In a 1956 paper based on these principles, Parker pointed out that "the high electrical conductivity of the region surrounding Earth, inferred from the observations of atmospheric whistlers and the zodiacal light, requires abandoning the customary models for producing geomagnetic (transient variations)... It becomes necessary to adopt a purely hydromagnetic approach wherein one focuses his attention only on the magnetic lines of force of the geomagnetic field and their displacement with the conducting gas surrounding Earth."

The transient variations arise from changing electric currents flowing in the ionosphere or the magnetosphere or from hydromagnetic waves generated by interactions between the geomagnetic field and ionized gas (plasma) moving out from the Sun. This gas, which is thought to flow radially outward, is referred to as the solar wind. This solar wind will push into the geomagnetic field roughly to the point where the kinetic energy density of the solar wind is equal to the magnetic-field energy density and form an elongated cavity around the Earth. The geomagnetic field is contained inside this cavity, which is often called the "magnetosphere."

Of the many observed types of transient variations, the largest (and most carefully studied) are diurnal variations and geomagnetic storms. The diurnal variation is attributed to tidal motion in the ionosphere. At a given location, the diurnal variation is reasonably predictable and usually involves field changes of the order of 0.1 per cent of the total field. The geomagnetic storm, while frequently following a general pattern, is much less predictable, both as to time of occurrence and detailed characteristics.

Many quantitative theories to account for the observed characteristics of geomagnetic storms have been presented, but none has yet been checked in detail by experiment. Since the mechanisms that drive the magnetic storm are thought to exist in the region above the ionosphere and within about 10 Earth-radii, they can be investigated directly by means of rocket-borne instrumentation.

The average features of a geomagnetic storm in the order in which they occur and the currently most acceptable explanations may be listed as follows:

1. Sudden Commencement: a relatively sudden increase in the horizontal component H, of the Earth's magnetic field in low and temperate latitudes. This increase, typically 20 to 30 gammas (1 gamma = 10^{-5} gauss), is largest at equatorial stations and has a rise time of 2 to 6 minutes. The sudden commencement is attributed to the sharp impact of solar plasma on the geomagnetic field. The effect of this impact is carried to the lower ionosphere and the Earth's surface by hydromagnetic waves. The sharp leading edge of the impacting solar plasma is maintained by the weak magnetic field and the plasma normally present in interplanetary space.

2. Initial Phase: the period during which H is above its normal undisturbed value (NUV). This phase lasts from 1 to 8 hours. It is usually explained as due to the increased solar wind pressure that follows the sudden commencement. This increased plasma pressure causes a world-wide compression of the geomagnetic field.

3. Main Phase: the period during which H is below its NUV. Usually H decreases 50 to 100 gammas below the NUV. After H reaches its minimum value, it recovers toward the NUV with a rate of recovery that increases with time, i.e., the H vs. time curve is saucer-shaped. This phase lasts from 12 to 24 hours and tends to be magnetically noisy with positive and negative excursions of H with amplitudes up to several hundred gamma and periods of approximately a half hour. (These variations do not appear in averaged magnetic storm data.) The cause of the main phase is perhaps the most controversial of all the proposed geomagnetic storm mechanisms, but only one quantitative model has been put forth. In this model, thermal protons (and possibly some electrons) that are normally present in the geomagnetic field are accelerated by hydromagnetic shock waves up to energies of the order of 1 keV. These comparatively energetic particles are then trapped in the geomagnetic field and execute trajectories similar to Van Allen radiation particles. The particles exert an outward stress on the geomagnetic field both from centrifugal force as they oscillate back and forth along lines of force and from mutual repulsion between the Earth's magnetic moment and the magnetic moment due to the particle's cyclotron motion. The trapped particles, which form a diamagnetic ring current, are thought to be distributed between about

4 and 8 Earth-radii. The large-amplitude fluctuations are probably due to turbulence in the solar wind that excites natural periods of oscillation of the geomagnetic cavity.

4. Recovery Phase: the period during which H returns to the NUV. During this phase, the rate of recovery decreases with time so that H approaches the NUV somewhat exponentially. The recovery time constant is usually between 1 and 3 days. Often there are no magnetic disturbances during this phase other than the slow recovery. The only quantitative model that has been proposed to explain this phase is the removal of the diamagnetic ring current through transfer of the energy of the trapped protons to neutral hydrogen in the geocorona by means of the ion-atom charge-exchange process.

After many years of calculation and speculation, the opportunity afforded by rockets to perform transient geomagnetic variation experiments in situ will soon lead to significant advances in our understanding of these variations. The magnetic surveys in the present NASA program should go far toward providing the required data, and no changes in the program are suggested. The study of the geomagnetic cavity (see next section) will also contribute. It should be reiterated, however, that specially designed sounding-rocket or satellite missions, which cannot be planned in detail at present, will surely be required to explore particular regions in the magnetosphere and to make crucial checks of the theory as our general knowledge increases. One such special mission of particular interest is the measurement of field fluctuations at a fixed geographic position from a 24-hour satellite.

The question of the role of hydromagnetic waves in the magnetic storm phenomena is a very important one, and one which will require special techniques for its investigation. Consideration should be given to the use of pairs or groups of satellites or sounding rockets making simultaneous magnetic field measurements with high time-resolution. More sophisticated magnetometers, capable of measuring gradients as well as magnitudes, may be required.

D. The Geomagnetic Cavity

Our present concept of a geomagnetic cavity which excludes the external "superconducting" plasma is based upon a few indirect physical observations coupled with a number of careful and far-reaching inferences, as has already been indicated. As the solar wind is probably "supersonic" (see next section), the situation resembles the flow of a high-velocity compressible fluid around an obstacle. The model which seems most probable considers the geomagnetic cavity as such an obstacle. The cavity is roughly hemispherical on the side facing into the solar wind, with the distance from the Earth at the stagnation point being typically approximately 10 Earth-radii (R_E). The distance is determined by the balance between the solar-wind pressure and the magnetic-field energy density at the boundary. At locations away from the stagnation point, the wind is not normal to the boundary, and the distance to the boundary becomes greater. On the back side of the Earth the solar wind cannot push against the field, but instead is thought to extend the cavity into a long tail. The cavity has an unknown shape and length in this region, but it is thought to be closed eventually by the random transverse thermal velocity of the solar wind.

Outside the cavity one would expect a collisionless shock front, the position of which should be near 14 R_E near the stagnation point. Between the shock and the cavity boundary the solar plasma should be thermalized on the sunward side and should flow parallel to the boundary, becoming supersonic again after passage through a second shock. The general configuration of the geomagnetic cavity and the shock wave in the solar wind are shown in Figure 1.

Seven satellites and space probes have so far carried magnetometers through regions where the geomagnetic cavity boundary could possibly be observed. Lunik I and Lunik II carried fluxgate magnetometers to the Moon. Pioneer I and Pioneer V carried search-coil magnetometers to distances of 11 R_E and 5600 R_E, respectively. Explorer VI carried a search-coil magnetometer to 8 R_E on the dark side of the Earth. Explorer X provided close-in measurements with a rubidium-vapor magnetometer and component measurements with fluxgate magnetometers out to approximately 48 R_E at 150 degrees from the Earth-Sun line. Explorer XII was equipped with three orthogonal fluxgate magnetometers, and apparently made repeated passes through the cavity boundary within its apogee distance of 12 R_E near the Earth-Sun line.

Although some data from the Lunik magnetometers near the Earth and near the Moon have been made available, no information about observation of the boundary has been forthcoming.

Pioneer I measured the component of the magnetic field perpendicular to the satellite spin axis. The results were consistent with the computed geomagnetic field out to 10 R_E. Between 10 R_E and 14 R_E the observed field was larger than the computed, and exhibited large fluctuations. Near 14 R_E the field decreased abruptly. This abrupt decrease was interpreted as the geomagnetic boundary, but it now appears more probable that the fluctuating field was in the thermalized plasma region between the boundary and the shock as shown in Figure 1.

Since Explorer VI was on the dark side of the Earth, boundary penetration was not expected within its apogee at 8 R_E and was apparently not observed. The search-coil magnetometer, similar to those on Pioneers I and V, gave results consistent with the Earth's computed field at low altitudes. Near apogee large departures were observed, which were first interpreted in terms of a quiet-day ring current, but further analysis discredited this explanation.

Pioneer V made only intermittent measurements, the first near 8 R_E and the second near 15 R_E. The results were consistent with a boundary between these two limits.

The Explorer X results were in reasonable agreement with the computed field from 5 R_E to 10 R_E. From there out to 20 R_E the field was larger than computed and became approximately radial from the Earth. Near 20 R_E the field, for the first time, decreased abruptly and fluctuated considerably in magnitude and direction, coincident with the sudden arrival of plasma as seen by the MIT plasma probe. The transition was interpreted as penetration of the geomagnetic boundary. Subsequently the field alternated between periods of relative quiet with magnitude approximately 20 gammas when plasma was absent, and periods with magnitude less than 10 gammas and with large fluctuations in magnitude and direction when plasma was observed. One very appealing interpretation of these results is that the satellite was traveling close to the boundary, and that fluctuations in the solar wind caused the boundary to move in and out past the satellite, but there are

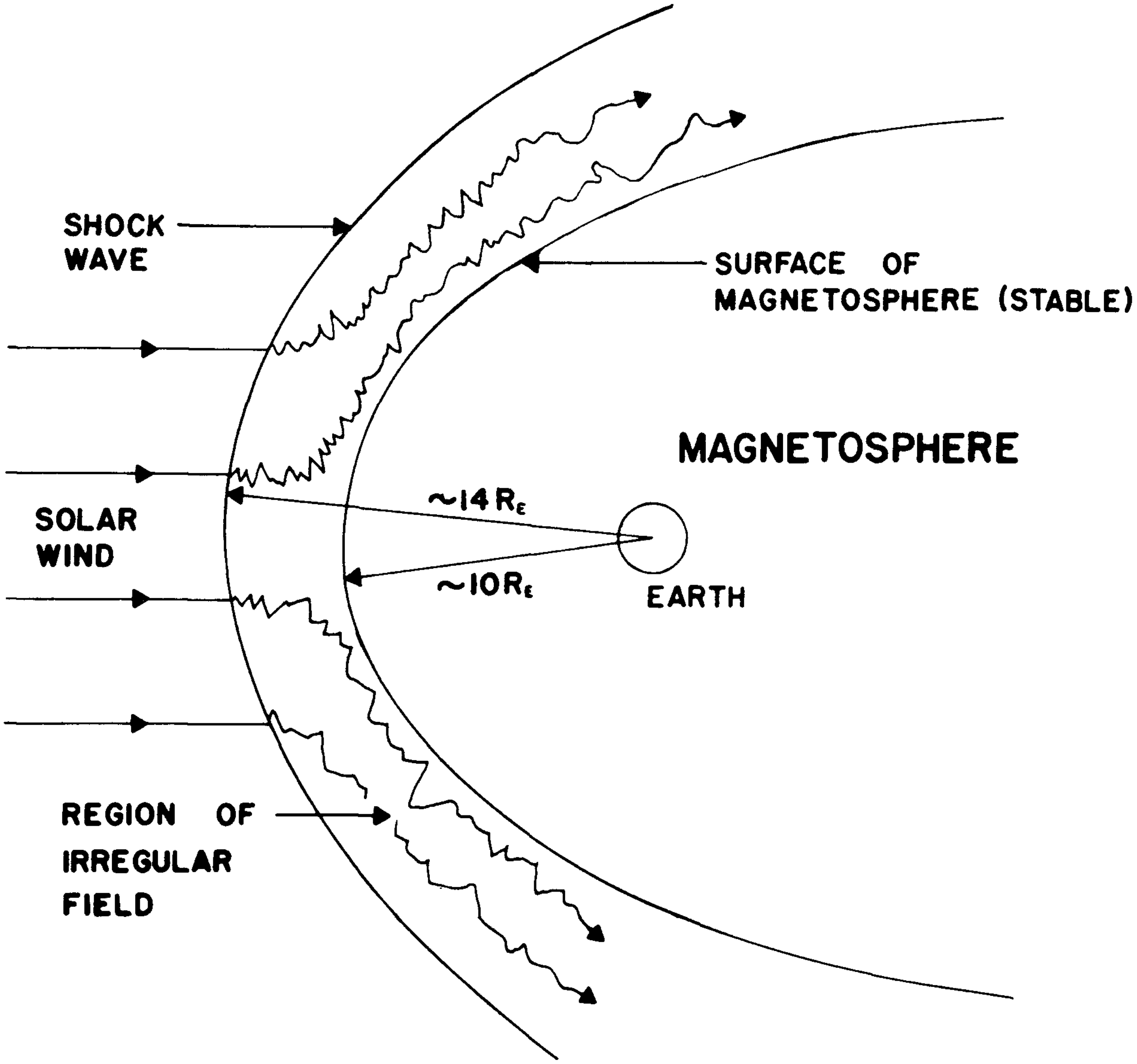

SHOCK WAVE
SURFACE OF MAGNETOSPHERE (STABLE)
MAGNETOSPHERE
SOLAR WIND
~14 R_E
~10R_E
EARTH
REGION OF IRREGULAR FIELD

FIGURE 1

theoretical difficulties in understanding the magnetic field directions on this picture. The direction of the "external" field was variable, a typical direction being in the plane of the ecliptic but at a large angle to the "internal" field.

Explorer XII, with its apogee near the Earth-Sun line, detected a boundary, characterized by a large change in field direction but little change in magnitude, near 10 R_E during magnetically quiet periods. During moderately disturbed periods, the boundary was observed as close as 8 R_E, and during the recovery phase of a magnetic storm it had retreated beyond 12 R_E. The boundary varied between 100 km and 1000 km in thickness. The external field, approximately 30 gammas in magnitude, was usually observed to be perpendicular to the ecliptic plane and pointing south. Fluctuations in the amplitude of the exterior field were much greater than those of the interior field. Observations late in the fourth month of the satellite's lifetime, which have not yet been analyzed, should extend the boundary survey to beyond 90 degrees from the Earth-Sun line.

Investigation of compression of the field during magnetic storm sudden commencements would be facilitiated by a geostationary satellite. From a satellite near 7 R_E on the sunlit side of the Earth one should be able to observe the boundary being pushed in past the satellite during the sudden commencement and initial phase. The subsequent retreat of the interface as the main phase developed could be observed. Moreover, it should be possible to follow the diurnal variation of the field and thus investigate the form of the magnetosphere in a manner not possible in any other type of orbit.

> FINDING: A variety of magnetometer instruments should be employed in investigations to determine the structure and boundaries of the geomagnetic cavity. Vector instruments are desirable. Instruments of moderate sensitivity (resolution, 10 gammas) are adequate for monitoring the boundary location. Investigation of the irregular magnetic field region beyond, and interplanetary fields of low levels, requires instruments capable of observing 1 gamma fluctuations.

E. Distant Magnetic Fields

Our direct information on magnetic fields far beyond the magnetosphere has come almost entirely from the measurements of Pioneer V in the spring of 1960. In these measurements, only field components perpendicular to the spin axis were detectable, and the direction of even this part of the field is known only inferentially, The principal result -- that the quiet field has a magnitude of approximately 2 gammas and a direction perpendicular to the ecliptic -- poses such formidable theoretical problems that its confirmation or contradiction is eagerly awaited. Its other result -- the appearance of very much larger fields at times which are correlated with magnetic disturbances on Earth -- although of considerable interest and significance, also serves primarily to whet our appetite for more such measurements, preferably with greater detail and with measurements of the plasma as well.

The Lunik magnetometers had too little sensitivity to measure the interplanetary field; the lunar magnetic field was inferred to be less than 100 gammas. It may fairly be said that the field of distant magnetic measurements is wide open.

RECOMMENDATION: Within the foreseeable future, all lunar, planetary, and interplanetary probes should carry appropriate magnetometers whenever possible. Only by widespread and frequent measurements can we expect to unravel the complicated temporal and spatial variations in magnetic field that Pioneer V revealed.

III. Solar Plasmas and the Solar Wind

A. Introduction

The presence in the solar system of streams of plasma flowing approximately radially outward from the Sun (the solar wind) is indicated by a variety of indirect evidence. The constant occurrence of magnetic fluctuations and aurorae near the poles is believed to be the result of the continuous flow of solar plasma past the Earth, and the presence of approximately 27-day variations in these phenomena are interpretable by the hypothesis that not all portions of the Sun emit plasma at the same rate. The occurrence of geomagnetic storms, accompanied by Forbush decreases in the galactic cosmic-ray flux and correlated in time with the appearance of large chromospheric flares on the sun, is best understood as resulting from the passage of a plasma cloud of particularly high energy density, which was produced by the flare. The excitation and acceleration of certain types of comet tails also find their explanation in terms of solar plasmas.

The study of plasmas in space is closely connected with the study of interplanetary magnetic fields. In fact, it is preferable to think of a plasma measurement and a magnetic-field measurement as two aspects of a single experiment; the high electrical conductivity of the plasma causes the magnetic flux lines to be effectively "frozen" into the plasma. In the normal case in interplanetary space, the kinetic energy density of the plasma is greater than the magnetic energy density, so that the magnetic field is carried along by the plasma motion. Near the Earth the situation is reversed, and the solar plasma is excluded from regions where the geomagnetic energy density is greater than the energy density in the plasma, so that a cavity (the magnetosphere) is formed.

B. History of Solar Plasma Research

Among the principal milestones of solar plasma research to date, the following should be mentioned. Lindemann in 1919 originally suggested that streams of ionized gas from the Sun are responsible for the geomagnetic storms and the aurorae, and the idea was given substance by the extensive work of Chapman and Ferraro for a number of years beginning in 1931. Biermann, beginning in 1951, considered the effect of "solar corpuscular radiation" on the motion of Type I comet tails. As a result of Biermann's analysis, Parker in 1958 was led to the formulation of his hydrodynamic theory of the efflux of gas from the corona, which he called the "solar wind." Since then, theoretical papers on the subject have proliferated.

In late 1959, Lunik II and Lunik III achieved the first direct detection of low-energy particle streams in regions of space which are presumably beyond the magnetosphere, but the data were too meager to afford any test of the theory.

In early 1961, Explorer X, carrying magnetometers and a plasma probe, obtained extensive simultaneous measurements of magnetic field (including magnitude and direction) and plasma flux (including intensity and energy), and apparently delineated the boundary of the magnetosphere at the time in one direction.

C. Intensity of the Solar Wind

The original work of Biermann[3] on comet tails appeared to indicate a plasma density in space of approximately 10^3 protons and electrons per cm^3 and a plasma velocity near 500 km/ sec. Later work reduced these numbers to 10 cm^{-3} and 300 km/ sec; these figures may be taken as typical average values.

The measurements on Luniks II and III gave a flux of approximately 2×10^8 cm^{-2} sec^{-1}. They were made with four hemispherical charged particle traps on a tumbling vehicle, and yield no information on the direction of motion of the plasma. The quantity actually measured was the excess of the current of positive ions with energies greater than 15 eV over the current of electrons with energies greater than 200 eV striking the space vehicle. On the assumption that no such high-energy electrons were present, the measurement gives the solar-wind proton (and alpha-particle) flux.

The plasma probe on Explorer X saw plasma fluxes up to 4×10^8 cm^{-2} sec^{-1}, and gave a rough energy analysis of the protons, a typical mean energy being about 500 eV. From this it is concluded that the average plasma density (on those two days) was between 10 and 20 per cm^3 and the velocity was typically 300 km/sec. (Here again low-energy electrons were excluded. Nothing can be assumed about electrons above 150 eV, because their presence or absence could not have materially affected the results.) The plasma flow direction, which was not unequivocally determined, was undoubtedly affected by the presence of the magnetosphere. Explorer X also recorded a sudden marked increase in the plasma flux coincident with a sudden magnetic storm commencement on Earth. The mean energy also shifted upwards, so that the bulk of the plasma appeared between 800 and 2300 eV.

IV. Solar Cosmic Rays

A. Introduction

The discovery of high-energy charged particles which originated at the Sun and impinged upon the Earth was made by Forbush and Lang in 1942. These particle streams have come to be designated as solar cosmic rays. We are still far from a complete understanding of their origin, composition, propagation, and terrestrial and extraterrestrial effects.

The solar cosmic rays should probably be distinguished from the solar wind by their mode of production or acceleration in the chromosphere, but this is not yet possible. The differentiation herein is made arbitrarily on the basis of energy, it being assumed that the solar wind comprises protons (and presumably other ions) in the energy range of hundreds or thousands of electron volts, while solar cosmic ray energies range from perhaps 0.1 MeV to a few BeV. Further research will no doubt show whether there is in fact a clear distinction or a continuous gradation between them.

B. Origin and Characteristics

Solar cosmic rays originate in solar flares in the chromosphere. Nearly every solar cosmic ray event so far observed can be correlated at least moderately well with a solar flare, but not nearly all flares produce particles that can be detected on (or near) the Earth.

The early ground observations of solar cosmic rays limited the range of detectability to particles of energy above $\sim$1 BeV. Balloon measurements reduced this threshhold to $\sim$80 MeV, and satellite detection has lowered the limit still further, to $\sim$0.1 MeV. The number of particles generally increases sharply as the energy decreases: in an integral spectrum of the form $N(>E) \sim E^{-\alpha}$, values of gamma ranging from 1 to 6 have been reported over various energy intervals. It is now clear that the power law spectrum is at best a convenience, that the spectrum is frequently different from one event to another, and indeed that it changes within a single event as a function of time. The nuclear composition of the radiation also varies from event to event, consisting typically of $\sim$90% protons, nearly 10% alpha particles, with a fraction of a per cent of heavier nuclei.

Solar cosmic ray events vary widely in intensity, from those at the threshold of detection ($\sim$0.2 particles/cm^{2}sec) to huge events in which fluxes of $> 10^5$ particles/cm^{2}sec of energy $\gtrsim 1$ MeV have been observed. The largest integrated flux yet observed from a single solar cosmic ray event occurred November 12-16, 1960, and gave $\sim 2 \times 10^9$ particles/cm^2 of energy >30 MeV for the entire period of the event.

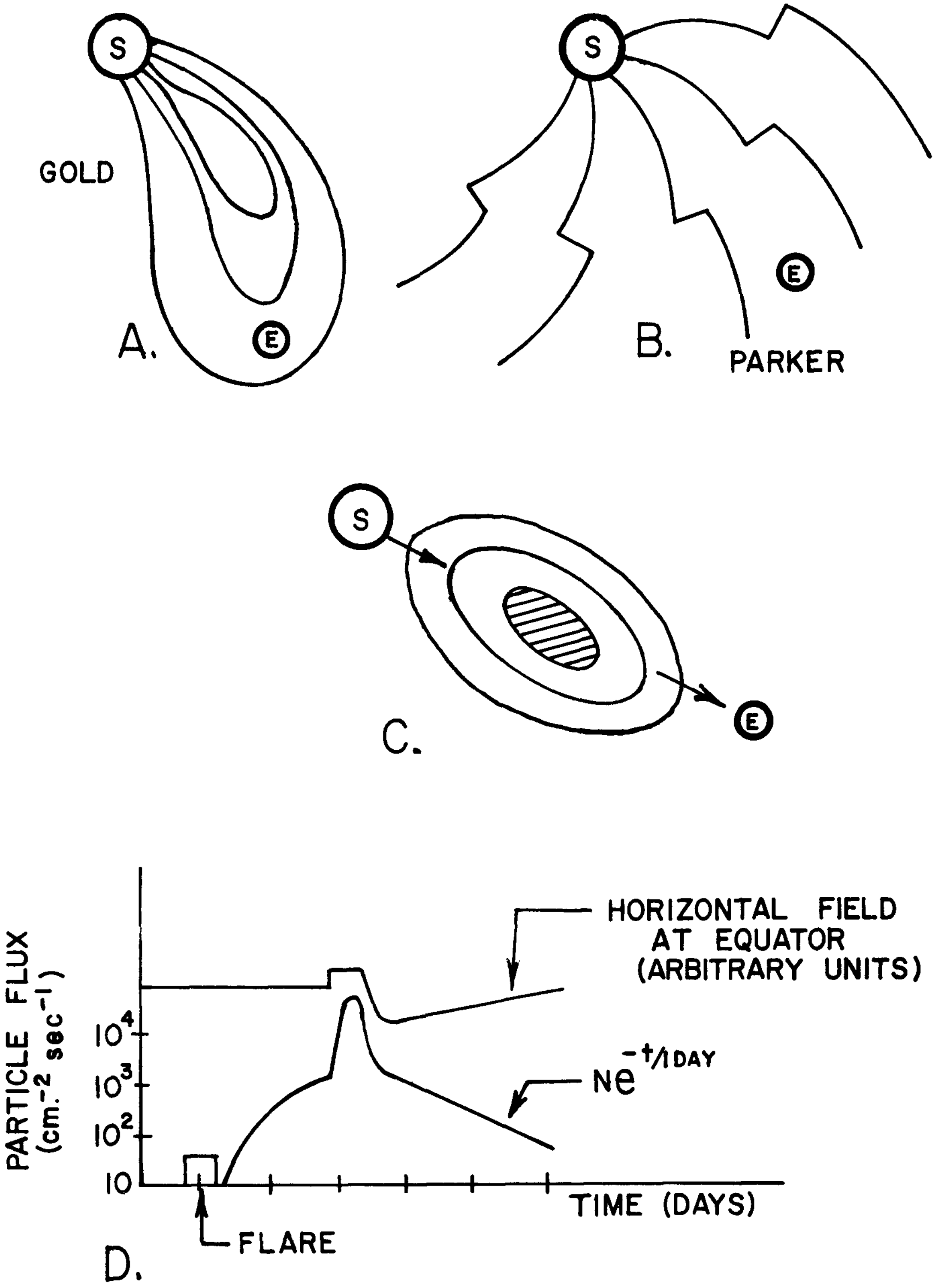
C. Propagation

Solar cosmic rays of high energy ($\gtrsim$1 BeV) travel with delays ranging from 15 minutes to a few hours from the Sun to the Earth. Those of lower energy take longer to propagate, of course, and in addition are believed by some observers to be contained in space by the expanding solar magnetic field. There is some difficulty with the containment concept because of adiabatic cooling inside the expanding solar field region. Perhaps the observed lower-energy particles were ambient ones in the interplanetary region that have been accelerated by the magnetic shock front. In any event, the arrival of the main body of low-energy particles ($\lesssim$10 MeV) may be delayed by 24 to 48 hours or even longer after the flare. This delay is explicable on the basis of the "magnetic bottle" concept of Gold and on the "blast" model of Parker (see Figures 2A and B).

In times of considerable solar activity, the arrival near the Earth of solar cosmic rays from a second flare in the same solar region as a recent predecessor flare usually is quicker than the arrival of the rays from the first flare. This fact is taken as evidence *against* the formation of a bubble of magnetically contained plasma whose field lines are detached from the Sun (see Figure 2C). The field lines apparently are continuous all the way back to the Sun and thus form a magnetic duct through which later particles can readily propagate.

D. Observations

Solar cosmic rays are observable and their characteristics determinable in interplanetary space by the use of space probes containing normal radiation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C.
E
PARTICLE FLUX (cm.$^{-2}$ sec^{-1})
10^4
10^3
10^2
10
HORIZONTAL FIELD AT EQUATOR (ARBITRARY UNITS)
$Ne^{-t/1\,DAY}$
TIME (DAYS)
FLARE
D.

FIGURE 2

detection devices. They are similarly observable by Earth satellites in space inside the geomagnetic region ($\lesssim 10$ to 14 R_E) where in addition recoverable rockets and satellites, especially those containing nuclear emulsions, are highly useful. Solar cosmic rays and their secondary products (e.g., nuclear gamma rays) are detectable at various levels of the Earth's atmosphere through the use of instrumented balloons and sounding rockets. Finally, these radiations are detectable at the Earth's surface, largely through the secondary effects of their absorption in the atmosphere -- by ground-based neutron monitors and through measurements of ionospheric radio-wave absorption at various wavelengths. For very high-energy particles, direct detection at the Earth's surface is possible and impact zone effects can be observe.

E. Effects of Solar Cosmic Rays

Solar cosmic rays have many effects on the Earth and its surrounding geomagnetic field region; these effects are generally not well understood. The highest-energy solar cosmic rays usually arrive shortly after the flare and are frequently completely gone by the time a magnetic storm begins. The arrival of the main body of the solar plasma in a major solar-cosmic-ray event is expected to coincide with the sudden commencement of a magnetic storm. In several recent observations, the arrivals of considerable fluxes of ~ 10 MeV particles have been found to coincide with the initial phase. These fluxes were then observed to dissipate rapidly early in the main phase of the storm and then gradually decay away in the recovery period over the next several days.

The reduction of the solar cosmic-ray flux as a function of time has been represented by a power law in time by many observers, with exponents from $-1/5$ to -4 derived for various energy intervals. Recently several cases involving ~ 10 MeV particles have shown an exponential decay with a time constant of ~ 1 day. No simple functional dependence is expected to hold over any extended range of energy or time.

The temporal correlation between the solar cosmic-ray flux and the phases of the magnetic storm indicates that there is an intimate connection between these phenomena (see Figure 2D). Possibly the solar cosmic rays are the source of the long-sought ring current, thought to be responsible for the main and recovery phases of the storm, but there are no clear ideas of how this is accomplished. During the main phase of the storm, normal geomagnetic cutoffs are shifted toward the equator by considerable amounts: e.g., particles of ~ 10 MeV are normally observable (pre-storm) only at $L \gtrsim 5.5\ R_E$ at 1000 km altitude; during the main phase of a storm, they are found at $L = 3$ to 4 R_E, where L is the geomagnetic shell parameter. Particles of other energies have their cutoffs similarly shifted.

The fact that solar cosmic rays are observable at such low values of L leads to immediate consideration of the possibility of their being an important source for the geomagnetically trapped radiation; however, no sensible injection mechanism by which this possibility might be shown to be valid has been suggested. The number of particles above 30 MeV found in the geomagnetically trapped region does correlate roughly with solar cosmic-ray activity; but how much of this correlation is due to injection and how much to the change in energy of the trapped particles is not clear.

The arrival of solar cosmic rays in the vicinity of the Earth results in polar cap absorption and occasionally in spectacular auroral displays. The former phenomenon is monitored by the relative ionospheric opacity metering (riometer) network. It has been thought that this method is most sensitive to the arrival in the upper atmosphere of ~20 to ~100 MeV protons, but recent correlation of the spatial extent of the polar cap absorption and the distribution in space of protons of various energies during an event indicates that particles in the range ~1 to ~20 MeV may be more important.

Solar cosmic rays in interplanetary space do not appear to be of very great importance for manned space travel. It has been suggested that the thoughtful distribution of the payload necessary for sustenance of manned space flight can provide about 10 gm/cm^2 shielding for the traveler without ad hoc additional shielding. With 10 gm/cm^2 protection, the space traveler would have received ~20 rads of exposure in the largest solar cosmic ray event yet observed. (The estimate that currently gives the highest flux predicts a dose of about 60 rads.) It is important that the significance of this number be emphasized. A dose of 20 to 60 rads is almost certainly not an unreasonable amount for an astronaut to risk, and it would apparently have been experienced only once during the last ten years. It is clear that continuous monitoring and detailed study of the solar cosmic rays should be carried on in the interest of the safety of men in space. It should be recognized, however, that a considerable amount of research has already gone into the establishment of the figure quoted, and the existence of much higher estimates which may have been obtained by uncertain extrapolations or promulgated by the "big booster" advocates should not be allowed unduly to increase the cost and complexity of our man-in-space effort.

F. Suggestions for Further Research

Although we have by now obtained considerable quantitative data on solar cosmic rays and can begin to formulate detailed theories about their origin, propagation, and interaction with the magnetosphere, several fundamental questions remain. Among these are:

What is the actual mechanism of production of solar cosmic rays in the Sun, and what flare conditions and propagation conditions produce solar cosmic rays of various energies?

Where do the low-energy particles originate?

What is the spatial extent of a solar cosmic-ray event?

What are the relationships between the solar cosmic rays and (i) magnetic storms, (ii) the ring current, and (iii) the geomagnetically trapped particles?

* * * * * * *

A 24-hour orbiting satellite at 6.6 Earth radii is presently listed in the NASA program on S-64 and S-64a, to be launched by the Centaur vehicle in late 1963 and 1964. The orbit will be an excellent one to observe the impact of the solar plasma on the magnetosphere and the relation of solar cosmic-ray events to aurorae and to the geomagnetically trapped radiation. Should the Centaur schedule slip, it would be desirable to reduce the payload weight as required for a Delta launch in order to get this satellite into orbit without delay.

V. The Van Allen Radiation Belt

It is intended to summarize here the current state of knowledge of the Van Allen radiation belt and to point out notable areas of ignorance.

A. Quasi-Stationary State

1. Quantitative description. A complete description of the particle contents of the geomagnetic field may be thought of as expressible in terms of a set of seven-parameter functions:

$$j_i(r, \varphi, \theta, \alpha, \beta, E, t),$$

wherein j_i is the differential unidirectional intensity of particles of type i (viz., electrons, protons, alpha particles, etc.); at the point r, φ, θ; in the direction specified by angles α and β; in unit range of energy at E; and at the moment of time t.

It is well known that the time dependence of the j_i's is of very great interest in understanding many aspects of the origin, acceleration and loss of trapped particles. Nonetheless, it is of expository value to consider first a description of the quasi-stationary state in time.

In such a state, it may be shown that three parameters suffice for a complete description of the distribution for each type of particle. For example, the unidirectional intensity may be written as follows

$$j_i(\alpha_o, L, E),$$

where α_o is the "pitch angle" at the equator (angle between the magnetic field vector $\vec{B}$ and the momentum vector $\vec{p}$ of the particle) on a magnetic shell whose equatorial crossing radius is L times the radius of the Earth (in the adiabatic equivalent dipole field).

An equivalent (and observationally preferable) way of describing the particle contents is by means of "structure functions"

$$J_o^1 (B, L, E)$$

where J_o^1 is the differential omnidirectional intensity of a given component on a magnetic shell specified by L and at a point whose scalar magnetic field value is B. Alternatively, the integral omnidirectional intensity of particles of energy greater than E_o may be written as

$$J_o(B, L) \int_{E_o}^{\infty} f(E, B, L) dE$$

where f(E, B, L) is the differential energy spectrum chosen so that the integral is normalized to unity at some selected lower limit E_o^*. Despite a series of experiments during the past four and one-half years, only a fragmentary knowledge of the structure functions of electrons and protons exists. Figure 3 shows a pictorial representation of four structure functions. Each of the four diagrams depicts current observational knowledge in the form of meridian cross sections of the Earth and its vicinity, ignoring departures of the geomagnetic field from that of a dipole and assuming rotational symmetry around the geomagnetic axis.

2. Some Sample Absolute Intensities

In heart of the inner zone ($L \sim 1.4$, $B = 0.12$)

Protons ($E > 30$ MeV), $J_o \sim 3 \times 10^4/\text{cm}^2\text{-sec}$

Electrons ($E > 600$ keV), $J_o \sim 2 \times 10^6/\text{cm}^2\text{-sec}$

Electrons ($E > 40$ keV), $J_o \sim 10^8/\text{cm}^2\text{-sec}$

In heart of the outer zone ($L \sim 3.5$)

Electrons ($E > 40$ keV), $J_o \sim 10^7/\text{cm}^2\text{-sec}$

Electrons ($1.5 < E < 5$ MeV), $J_o \sim 10^4/\text{cm}^2\text{-sec}$

Protons ($0.1 < E < 5$ MeV), $J_o \sim 10^8/\text{cm}^2\text{-sec}$

Protons ($E > 1$ MeV), $J_o \sim 10^7/\text{cm}^2\text{-sec}$

Protons ($E > 75$ MeV), $J_o < 0.1/\text{cm}^2\text{-sec}$

3. Spectra. Present knowledge of the energy spectra of trapped electrons and protons is still rather meager. But it is clear that the energy spectra of both components vary markedly with position (and in some instances with time). Representative differential number energy spectra are as follows:

Protons in lower edge of inner zone:

$$J(E)dE \sim E^{-1.8}dE,$$

for energy range $75 < E < 700$ MeV.

Protons at outer edge of inner zone:

$$J(E)dE \sim E^{-4.5}dE$$

Electrons in lower portion of inner zone:

$$J(E)\,dE \sim e^{-E/160}\,dE,$$

with E in keV., for energy range $E > 40$ keV.

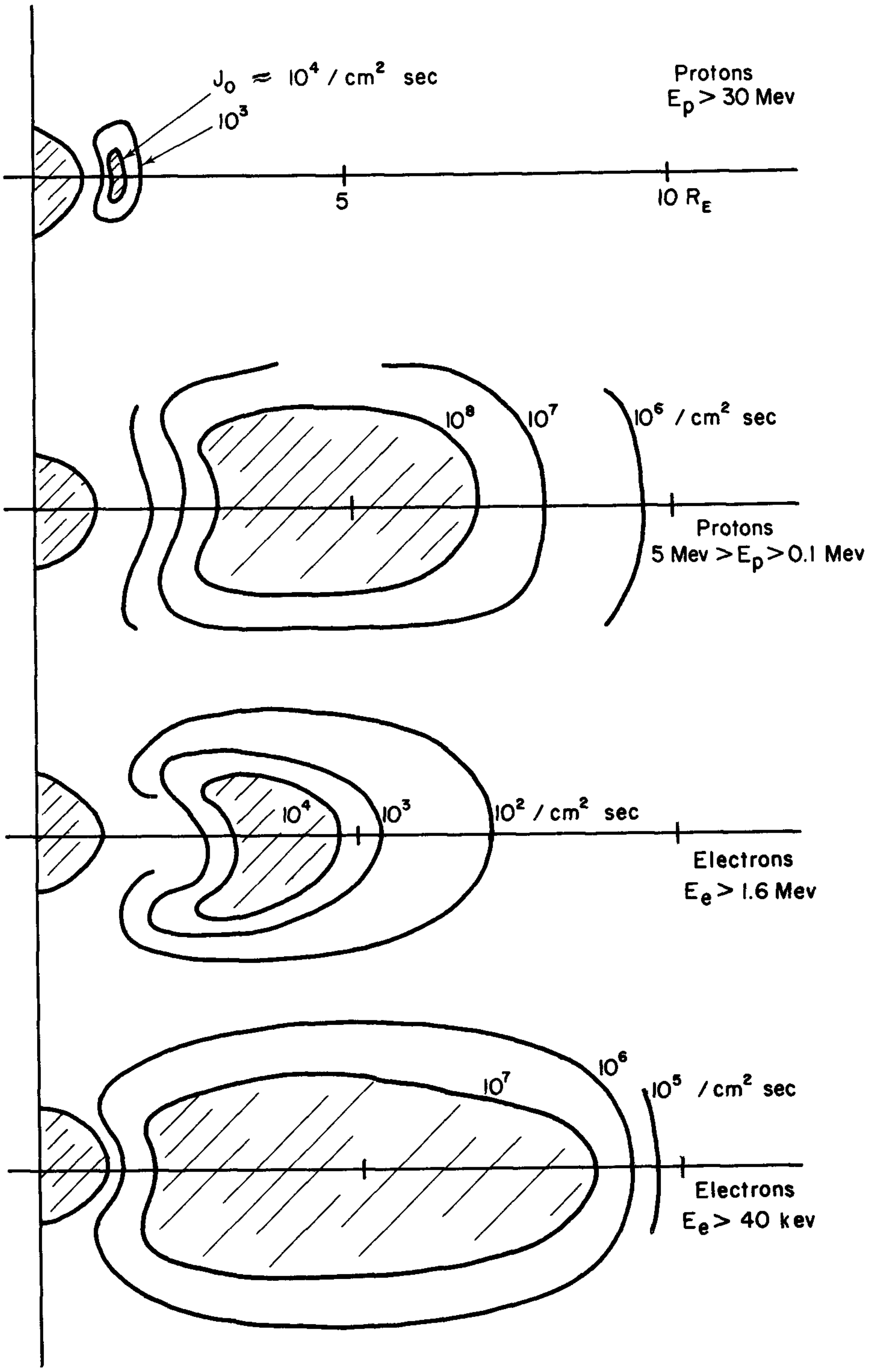

FIGURE 3

Some Sample Structure Functions

Electrons in heart of outer zone:

$$J(E)\, dE \sim E^{-1}\, dE,$$

for energy range $40 < E < 150$ keV.

$$J(E)\, dE \sim E^{-5}\, dE$$

for energy range $300 < E < 5,000$ keV.

Protons in heart of outer zone:

$$J(E)\, dE \sim e^{-E/100}\, dE$$

E in keV, for energy range $100 < E < 5,000$ keV.

Nothing resembling a comprehensive knowledge of the positional dependence of energy spectra yet exists, even for the quasi-stationary state.

4. Time Variations. Generally speaking, the time fluctuations of particle intensities increase with increasing L, by factors which are of the order of:

(i) unity for $L < 1.8$ for protons with $E > 20$ MeV

(ii) ten for $1.8 < L < 2.2$ for electrons with $E > 40$ keV

(iii) ten for $2.2 < L < 15$ for electrons with $E > 40$ keV

(iv) one hundred for $2.2 < L < 15$ for electrons with $E > 1.5$ MeV.

These observations are understood in a general way as being due to the increasing relative importance of perturbations of the geomagnetic field by solar plasma as one goes to higher L values (and hence to weaker fields).

Recent detailed studies of time fluctuations show that such fluctuations are significantly, though not simply, correlated with solar-geophysical influences, probably by way of the agency of solar plasma impinging on the geomagnetic field.

> FINDING: A much fuller study of spatial and spectral variations is strongly needed, in order to develop a body of knowledge which is susceptible to conclusive theoretical interpretation (i.e., a unified understanding of the essential physical processes which are of importance).

5. Geomagnetic Cavity. Quite illuminating new observations of the magnetic and particulate nature of the interface between the geomagnetic field and the interplanetary medium have been made with satellites in eccentric orbits. It has been found by direct observation, for example, that the effective outer boundary of the geomagnetic field on the sunward side of the Earth varies between about 8 to 12 Earth-radii; these variations are closely related to variations of the flow of solar plasma.

6. Origin of Particles. The understanding of the physical dynamics of the radiation belt is still in a quite primitive state. The leading theory concerning the origin of the energetic protons in the inner zone is that they are injected into the geomagnetic field by the beta decay of neutrons flying outward from nuclear disintegrations produced in the atmosphere by galactic cosmic rays (neutron albedo hypothesis). Some hold that this theory yields an expected intensity of the proper order of magnitude and a spectral form resembling that observed in the lower edge of the zone. The concomitant expectations of this theory for electron intensities and energy spectra have a much less satisfactory resemblance to the observations.

Recent work on the small but significant time variations in the inner zone suggests either that there are other sources of particles or that there are physical processes that have not yet been considered explicitly.

For the region beyond L ~ 1.8, the general correlation of the time variations with solar activity (and the resulting geomagnetic activity) leads to the reasonably conclusive belief that local acceleration processes resulting somehow from the impact of solar plasma on the geomagnetic field are dominant in producing the bulk of the energetic particle population observed. The time variations, particle compositions, and energy spectra are so foreign to the simple neutron-albedo picture as to require major modifications or abandonment of this theory in this region.

Solar cosmic rays probably make a significant contribution to the trapped proton intensity at high L values, through the intermediate process of the secondary production of neutrons over the polar caps, which in turn decay into protons and electrons as they fly outward from the atmosphere. Direct injection of solar-flare cosmic rays seems to be a less attractive process because: (i) There is no apparent mechanism whereby particles of solar origin can be directly injected into and trapped by the geomagnetic field, and (ii) the flux and spectrum of trapped protons in the outer zone are quite different from the observed solar-flare cosmic-ray flux.

7. Artificial Injection of Particles into the Geomagnetic Field. The Argus, Teak, and Orange tests of 1958, which comprised the bursting of atomic devices at high altitudes, injected fission decay electrons into trapped orbits in the geomagnetic field. The study of the time history of these particles yielded quite valuable new knowledge on the physical dynamics of trapped electrons resulting from controlled experiments.

The recent (9 July 1962) atomic burst at about 400 kilometers altitude over Johnston Island in the Central Pacific is currently yielding important new satellite observations on the lifetimes of electrons in the near-Earth field.

> FINDING: The controlled injection by artificial means of a known number of particles with known energy spectra and at known positions in space is a technique of considerable technical value in clarifying the dynamics of trapped electrons as a function of L value, geomagnetic perturbations, and the nature of the outer part of the geomagnetic field. International cooperation in the development of ingenious injection mechanisms, experimental methods and the analysis of results could contribute significantly to this end. An adequate system of satellites (at least one in a high-inclination orbit) is required for observing the effects.

8. Relationship of Auroras to Trapped Particles. It has been conclusively shown that the energy stored in the Van Allen radiation belt is inadequate to sustain high intensity auroras for the periods of time that are observed; the spectra of auroral electrons and Van Allen radiation belt electrons are very dissimilar; and the flux of trapped electrons is several orders of magnitude less than the electron flux in a bright aurora. It may well be that the trapped particles in the outer zone are only incidental and not central to the auroral process. In either case an acceleration mechanism is required for the aurora. The driving energy is derived, amost certainly, from that of the solar plasma which impinges on the geomagnetic field. None of the many auroral theories put forth have yet received any degree of critical acceptance.

9. Relationship of Geomagnetic Storms to Trapped Particles. It is rather widely believed that the main phase of geomagnetic storms is due to a ring current around the Earth at a radius of several Earth-radii and that the longitudinal drift of low-energy trapped particles provides the microscopic foundation for this current. This hypothesis has a substantial level of quantitative plausibility. Yet there is still an inadequate body of magnetic observations and of particle observations to render this hypothesis conclusive.

10. Acceleration Processes. Throughout this report, the concept of an as yet unknown local acceleration mechanism continually appears. The nature of this particle acceleration mechanism is one of the leading questions in this report. This acceleration process takes several forms that may be related:

(i) Russian measurements in Space Vehicle II (1960 λ 1) showed the presence at 300 km over the Pacific of electrons with $E > 30$ keV. These measurements have essentially been confirmed in the Air Force program where ion traps have detected the ionospheric effects of such particles. This is a very striking phenomenon, as important and difficult to understand as the radiation belts or the aurora, to which it may be related. Now that the existence of the phenomenon seems to be well established (by three flights -- one Russian and two American), it seems entirely warranted to expect some reorientation of the NASA program to explore this phenomenon more fully.

(ii) Acceleration of auroral particles.

(iii) Dynamic characteristics of the Van Allen radiation.

FINDING: Much information on items (i) and (ii) can be obtained with an oriented, 300-km altitude, polar-orbiting vehicle. Note that the POGO orbit is not well suited for these measurements. EGO is in a good orbit for investigating item (iii).

11. Radiation Belts Around Other Planets. Radio-astronomical evidence makes it appear reasonably certain that there is a huge radiation belt of electrons (at least) around the planet Jupiter. It will be some years before direct space probe measurements around Jupiter will be possible.

Meanwhile, there is considerable interest in exploratory observations which are in preparation for the detection of trapped particles that may be around Venus and Mars. (Radio detection techniques, such as used for Jupiter, have not detected any radiation belt around either Venus or Mars.) If the results of these attempts during the next several years are positive, more thoroughgoing investigations will be desirable.

VI. Aurora and Airglow

An aurora is the nighttime emission of light by atmospheric constituents at high altitudes in sufficient brightness to be visible to the human eye with surface brightness of more than about 10^9 photons/cm^{2}sec^{-1} of green light or 10^{10} photons/cm^{2}sec^{-1} of red light. Any weaker emission in the night sky is called airglow. By historical definition: if you can see it, it is an ''aurora''; if you cannot see it, it is ''airglow.''

The wavelengths of the emitted light identify it as coming from atoms and molecules of oxygen and/or molecules of nitrogen which are excited by some source of energy. In airglow, nitrogen emission is relatively very weak. This may be because it requires more energy to excite a nitrogen emission than an oxygen emission. Emissions of metals such as sodium are also seen in airglow and auroras.

Auroras occur most often between magnetic latitudes of about 65° and 70°. Airglow is world-wide all the night, with a maximum intensity of some emissions at magnetic latitudes of about 16°. The intensity of the light in auroras may change by orders of magnitude in less than one second, but airglow is generally more stable except around magnetic latitudes of about 16°. An auroral form may extend thousands of kilometers in longitude, but only tens of kilometers in latitude. Structures as small as 0.3 km thick have been measured.

Aurora and airglow intensities are greatest at altitudes around 100 km, with significant brightness occasionally up to 1000 km. The altitude of the monochromatic red arcs seen at mid-latitudes during magnetic storms is about 400 km.

Auroras are brightest and are seen at lowest latitudes during times of severe world-wide magnetic storms which follow perhaps 30 to 40 hours after a large solar flare. This correlation has led to many speculations that particles coming from the Sun cause auroras. Since auroras are confined in magnetic latitude, and individual features are often oriented along magnetic field lines, it is concluded that the primary particles are charged, and are either electrons or protons. In order to produce peak emission of light at an altitude of 100 km, an electron must have about 10 keV of energy and a proton about 200 keV. Such particles would take less than an hour and less than six hours respectively to travel directly from the Sun, so it is generally concluded that they do not have the foregoing energies in the magnetic-storm cloud which hits the Earth.

Balmer emission is observed in auroras; rocket flights into active auroras showed that there are intense fluxes of particles of the foregoing energies bombarding the atmosphere where auroras occur. In particular, McIlwain showed that there was about one thousand times as much energy deposited in the atmosphere by an intense electron flux as was emitted as visible light during a bright aurora through which his rocket flew. It is generally concluded that the dominant excitation of bright active auroras is by electron bombardment, with proton bombardment and perhaps other mechanisms, such as electric discharges, being important in some auroras.

The cause of excitation of airglow is not known. It is possible that on occasions the electron fluxes causing auroras are produced by acceleration mechanisms within the ionosphere. Observations with the Injun satellite show, however, that sufficiently intense fluxes of electrons are precipitated to auroral altitudes of ~100 km from the region above 1000 km altitude, and it is reasonable to conclude that the magnetosphere is the prime source of auroral electrons. This source region must therefore be explored with rockets and satellites.

> FINDING: It was thought at one time that the trapped radiation discovered with satellites might be the prime source of auroral particles. This now appears very unlikely, and it is essential to have more satellite observations within the source regions in order to advance our understanding.

The theoretical problems in auroral studies awaiting solution include:

(i) Explanation of how the rapid variations in auroral color, intensity, and structure can be understood to result from electron bombardment or other mechanisms;

(ii) Explanation of how bombarding electrons can be given their energy and how they are precipitated into the atmosphere;

(iii) Explanation of how the electron fluxes can be occasionally so extensive in longitude and narrow in latitude, and how they can achieve such tortured morphology as is observed;

(iv) Explanation of such gross features as the latitude variation in the frequency of occurrence, and how the frequency is controlled in diurnal and solar-cycle effects.

In many cases solution of the foregoing problems is hindered by inadequate experimental data.

The experimental problems amenable to attack by rocket or satellite instrumentation are many. The principal one is simply the observation on a given satellite of the nature, energy, and angular distribution of particles being precipitated into the atmosphere down to auroral altitudes, and the simultaneous observation on the same satellite of the resultant optical emissions. The crudest way to measure the latter is with photometers equipped with filters to separate out selected wavelengths. Present technology has essentially reached the stage, however, where an image converter on a satellite can photograph the spatial extent of an aurora. Satellite orbits which are polar and near-circular at altitudes of about 300 km are probably the best.

In order to investigate the source of the precipitated particles one requires suitable satellites at the same time at higher altitudes. A geostationary satellite such as S-64 is an excellent vehicle for study of a number of such auroral problems, since the magnetic-field lines which pass through it plunge down into the auroral zones. The Sun evidently exerts considerable control on the aurora, and some examples are known of auroral structures being constant in local time. A geostationary satellite is the ideal vehicle for investigating diurnal or local-time effects.

A satellite photometric or photographic study of aurora and airglow has the following advantages over ground-based observations:

(i) Ultraviolet emissions such as the forbidden oxygen line of 2972Å can be seen from a satellite but not from the ground because of their absorption in the ozone layer in the atmosphere.

(ii) Cloud cover is no disadvantage for a satellite, but it generally cuts possible ground-based observations by 50 per cent.

(iii) The satellite takes repeated cuts of auroral phenomena over a wide range of longitude and latitude. It also can make observations in rapid sequence in both hemispheres.

(iv) The variation of light intensity with latitude can be studied from a satellite in essentially the time-stationary state. The latitude variation is presumably very significant, but no theoretical explanation of even the present knowledge has been given.

(v) Lastly, an integrated experiment of photometers on a satellite with particle detectors can be the ideal observation of both cause and effect. Such a vehicle should be oriented with respect to the geomagnetic field vector.

Visual inspection of an active auroral display is disturbing for the theorist, who does not know which particular feature he should attempt to isolate and explain, and for the experimentalist, who is similarly burdened with an excess of riches for his detectors. One might expect that observational and theoretical studies of the primary exciting particles might make both tasks simpler. Such observations can be made only from rockets and satellites, with satellites giving the considerable advantage of wide geographical coverage.

VII. Dim Light from Space

A. Introduction

Balloons, satellites, and space probes which get above the atmosphere of the Earth have greatly enhanced our knowledge of particles and fields in space. In recent years, a great deal of interest has arisen in the study of the composition of the interplanetary medium with respect to dust, electrons, neutral and charged atoms, and magnetic fields. Admittedly, the best way to study these properties is to make direct measurements in the interplanetary medium. The disadvantage of these measurements is that usually they are made locally and the properties that are being measured change with space and with time. It has been known for many years that the dim-light phenomena of space, when understood, should give a means for studying the gross aspects of the interplanetary material. Unfortunately, up to this time, very little effort has been spent in applying satellite techniques to the study of the dim-light phenomena. Recently, at Minnesota, the capability of plastic balloons has been used to carry photometric equipment to an altitude of 30 km, above all of the dust and haze in the atmosphere, and above most of the air; but the understanding of the dim-light phenomena will ultimately necessarily involve the use of satellites and space probes. The dim-light phenomena are listed below in order of decreasing brightness:

(i) Polar aurora,
(ii) Zodiacal light,
(iii) Airglow,
(iv) Star light,
(v) Gegenschein.

Of these phenomena, the aurora is by far the brightest. Airglow, starlight, and zodiacal light are comparable with average intensities expressed in various units as 10^{-13} of the Sun's surface brightness, a hundred tenth-magnitude stars per square degree, or 200 Rayleighs (where one Rayleigh is 10^6 photons/cm^{2}sec). The various phenomena will be briefly discussed in order.

B. Polar Aurora

The properties of the polar aurora are fairly well known and very complex. The aurora has been studied mostly from the ground where the ultraviolet features cannot be observed because of the extensive ozone layer above the Earth. Because of the brightness of the aurora, the instrumentation for satellite or spacecraft observations of the aurora is relatively simple, and the light is adequate to allow observation of the aurora in narrow spectral regions which include interesting atomic lines of oxygen and nitrogen and molecular bands. A satisfactory theory of the aurora does not exist that explains the following facts: (i) the aurora occurs in a region of the geomagnetic field that is relatively well shielded from the plasma clouds in space, and (ii) the magnitude of the energy required to excite the polar aurora is extremely large. It has been estimated that the total energy stored in the external magnetic field of the Earth could run a good aurora for only a matter of hours, and the trapped radiation in the entire magnetosphere could keep a good aurora running for only some tens of minutes. There is probably an intimate connection between the auroral phenomenon and the radiation belts. Discovery by Winckler and his collaborators of high-energy X rays associated with auroral phenomena and the observation (in rocket shots by McIlwain) of high-energy electrons in the auroral zone show that the aurora involves not only low-energy excitations but rather high-energy electrons.

C. Zodiacal Light

The zodiacal light was first observed by Cassini in 1683 and since that time has been studied by a number of astronomers. The zodiacal light is seen as a luminous cone rising from the western sky after sunset or seen in the eastern sky before sunrise. The striking features of this light that have been established are:

(i) It is approximately in the plane of the ecliptic;

(ii) It has approximately the same color as sunlight;

(iii) It is very probably polarized, reaching values of at least 10% at some elongations.

The fact that the zodiacal light is essentially the color of sunlight is an extremely important observation. In order for this to be true, the scattering of light by the particles in space must be either Thomson scattering of free electrons, or Mie scattering by rather large dust particles. The presence of any appreciable quantity of dust smaller than a micron or bound electrons that give Rayleigh scattering would lead to a Rayleigh scattering factor that would make the color of the zodiacal light blue. We therefore must consider that the zodiacal light is a mixture of scattered light by free electrons and relatively large dust particles. The polarization of the light observed on the ground is frequently as high as 10%. This polarization arises from a mixture of (i) scattering by electrons, (ii) scattering by dust, and (iii) atmospheric scattering. Balloon flights made at Minnesota have shown that approximately half of the polarization observed at relatively large elongations is due to atmospheric scattering. However, an appreciable residual polarization (5 to 10%) of the zodiacal light persists. Although Blackwell and others have tried to infer from sea-level measurements that the polarization is a function of elongation, this is at best an extremely risky procedure because of the unknown contribution of the airglow background to the zodiacal light observations.

Various points of view have been taken with respect to the polarization of the zodiacal light. One view, first proposed by Behr and Siedentopf, is that all of the polarized light arises from Thomson scattering by free electrons. If this assumption is taken seriously, and the sea-level measurements are used to calculate the electron densities in the vicinity of the Earth's orbit, they come out to be in the order of $600/\text{cm}^3$ — a value which is now believed to be far too large. If half of the polarized light arises from atmospheric scattering, the remaining polarized light will still require something of the order of 300 $\text{electrons}/\text{cm}^3$, which is more than other measurements in the vicinity of the Earth seem to indicate. The magnitude and variations of the intensity of polarized light outside the atmosphere can be observed with satellites and space probes and should give the means for detecting fluctuations in the electron density in the solar system. A zodiacal-light experiment from a satellite can detect the order of 10 to 100 electrons per cm^3 in a cloud an appreciable fraction of the size of 1 astronomical unit. The ambient electron density in the solar system at the position of the Earth must be of the order of 5 to 50 electrons per cm^3, and these free electrons must give a polarized scattered sunlight contribution to the total zodiacal light phenomena.

If the majority of the polarization of the zodiacal light comes from the scattering by dust, there is a problem because it is difficult to reproduce the relatively large polarizations required in laboratory experiments with suspended aerosols. In order to obtain reasonably large polarization it is necessary to make the aerosols quite small, in which case the scattered light takes on colors different from the illuminating light, thereby violating the observations of the zodiacal phenomenon. It is conceivable that polarization is introduced by some sort of alignment of asymmetric dust grains in the solar system which could enhance the polarization. In any event, a large fraction of the zodiacal light must be contributed by dust scattering and the source of this dust in the solar system must be extremely strong. The Poynting-Robertson effect removes dust from the solar system rather rapidly by causing it to spiral into the Sun. For example, a one gram dust grain at the Earth's radius spirals into the Sun in a matter of ten million years. Kellogg and Ney have recently calculated that a much more efficient mechanism exists for removing the dust, namely the energy loss drag on the charged dust particles. Because of the solar ultraviolet, any dust that exists in the solar system must be charged to a potential of roughly 20 electron volts.

Because of this charge, the dust passing through the interplanetary medium loses energy to it in the same way that a charged particle does in passing through matter. This energy loss drag causes the dust particles to spiral into the Sun in much the same way that a satellite is finally brought back into the Earth by the drag of the air it passes through. The energy loss drag is much larger than the Poynting-Robertson effect; for example, with an electron density of approximately 10 per cm^3, the energy loss drag is of the order of 100 times larger than the Poynting-Robertson effect. It can be seen then that the rather fast removal of dust by this mechanism requires an enormous source for the replenishment of the dust in space which produces some of the zodiacal light scattering. The absence of Rayleigh scattering in the zodiacal light can be understood, since the radiation pressure from the Sun blows out dust particles smaller than about a micron in diameter.

The exact geometry of the zodiacal light is of some interest. It has been shown by Blackwell that when the ecliptic is vertical, the zodiacal light is very nearly in the plane of the ecliptic. However, balloon observations of the zodiacal light over a six months' period at Minnesota have shown that when the ecliptic is tilted, the zodiacal light is definitely displaced away from the horizon as much as 10°. This displacement of the zodiacal light away from the horizon was first observed by the Rev. George Jones in 1856, who studied the zodiacal light from the USS Mississippi in Perry's expedition to Japan. His observations are compatible with the idea that the zodiacal light shows an antiparallactic property. The axis of symmetry is pushed away from the ecliptic whenever the ecliptic is tilted. As yet no satisfactory explanation has been found for this phenomenon except the presence of atmospheric extinction. In balloon experiments at 35 km, only one half per cent of the atmosphere is above the balloon and yet the observation of the effect of the horizon on the zodiacal light agrees qualitatively with that observed from the ground. At the balloon altitudes the extinction should be very small, unless some unknown layer exists above 30 km. In order to evaluate definitely the parallax or lack of parallax, one needs observations from two stations widely separated in latitude. Simultaneous balloon flights in Australia and in the United States are planned during 1963 to carry out parallactic observations on the zodiacal light. If there is indeed atmospheric extinction still present above 30 km altitude, it will be essential to obtain observations of the zodiacal light from satellites flying well above any atmospheric layers.

D. Airglow

The light of the night sky observed with a spectrograph is seen in the visible region of the spectrum to consist of a continuum and of a green line, a red line, and a yellow line. The yellow line arises from the sodium D lines and is quite variable throughout the year. It is considerably less bright than the red or green lines. The green airglow line, λ 5577, arises from the 1D-1S forbidden transition in atomic oxygen, and the red line at λ 6300 arises from the 3P-1D forbidden transition in atomic oxygen. The contribution of the continuum to the total airglow is quite uncertain since absolute measurements of the continuum intensity are extremely difficult in the presence of the background starlight.

The balloon experiments at Minnesota have recently carried very long-exposure, very large-aperture cameras which made it possible to photograph the airglow, zodiacal light, and aurora in color. The few such flights made thus far have shown that the airglow is either red or green, and the implication is quite strong that the two colors are due to the two oxygen transitions. The intensity of both colors varied by more than a factor of ten, probably 100. The green airglow line arises (as has been shown by Meredith and his collaborators) at a height of 90 km. The red airglow is produced mostly above this altitude, and Roach's observations seem to indicate a height of approximately 400 km for the red airglow arcs which occur around lines of constant geomagnetic latitude or constant L values. These red arcs are definitely not part of the polar aurora.

The association of the red λ 6300 airglow arcs with intense activity in the radiation belts is a matter of some interest. The connection between the appearance of a red auroral arc and the dumping of the radiation belts has been indicated in an experiment by Roach and Van Allen, but the direct excitation of the red airglow line by particle release from the radiation belts cannot be accomplished without the simultaneous excitation of the green line which apparently does not appear in these red airglow arcs. An advantage of satellite experiments on the airglow is that the satellite may fly at an altitude well above the airglow layers, and therefore observe the layers in profile on the horizon, clearly delineating their altitudes and geographical extent.

E. Starlight

In the measurement of the other sources of dim light in space, the starlight is essentially a background which must be corrected for. Observations of the star densities throughout the sky have been carried out by astronomers by making star counts in selected regions. Most of the night skylight comes from rather dim stars of the order of 10th to 15th magnitude stars, and is of course concentrated very largely in the Milky Way, but is present throughout the sky. Satellite observations of the light from the Milky Way compared with the star counts will allow one to decide whether all of the light observed actually arises from stars, or whether there is in addition some sort of diffuse galactic light.

F. Gegenschein

The gegenschein is the dimmest of the phenomena of the night sky, and is so dim that it is seen only with very great difficulty with the completely dark-adapted eye. So far it has not been successfully photographed, but photometer measurements have shown its presence and approximate extent. It is located in the anti-Sun direction, and at this time there is no knowledge of the color of the gegenschein as compared with the color of sunlight. Recent observations of the geomagnetic cavity, or the boundary between the magnetosphere of the Earth and interplanetary plasma, have indicated that this cavity is elongated away from the Sun because of the action of the solar wind. Studies of the gegenschien may throw some light on the properties of the geomagnetic cavity in the anti-Sun direction. Because of the rather large contribution of starlight compared with the intensity of the gegenschein, however, it is not clear that it will be possible to separate the gegenschein clearly and to measure its properties in the presence of star background even with satellite measurements.

Note Added in Proof

For a more complete discussion of some of the following topics and experiments connected therewith, which are not treated in detail in this report, see "Science in Space," a report of the Space Science Board (NAS-NRC Pub. Feb. 1960), McGraw-Hill Book Co., New York, 1961:

Galactic cosmic rays (origin, composition, etc.)

Large-scale dynamics of the solar system (cosmic-ray modulation phenomena, shock phenomena, cosmic-ray intensity and composition gradients, etc.)

Composition of the solar wind, solar neutron fluxes, etc.

Appendix I

Principal participants in the Working Group on Fields and Particles included the following:

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Chapter Eight

METEOROLOGICAL ROCKETS AND SATELLITES*

I. Rockets

A. Introduction

Synoptic meteorology is the study of the atmosphere as a whole, in four dimensions. It may be said to have begun over 100 years ago with the first weather map, made possible by the invention of the telegraph and the ability to transmit observations quickly and easily. The next great advance was made with balloons that could probe the atmosphere vertically. Advances in electronics led to the balloon-borne radiosonde, an instrument now in routine use throughout the world.

Although balloons can probe up through about 99 per cent of the atmosphere's mass (that is, to about the 10mb level at 30 km), it is becoming increasingly clear that the events in the top one per cent of the atmosphere are not only very intriguing but may be of great significance to the behavior of the whole atmosphere. In the last few years meteorology has made another major observational advance upward, and now a modest network of small rockets has extended synoptic coverage to 50 or 60 km. A few rockets have gone much higher; and there are also some other means for observing high-level wind flow, such as following the motions of meteor trails by radio reflections. We have now seen many of the events that occur in the region between the ionosphere proper and the lower atmosphere, though we are far from understanding them -- even farther from predicting them.

B. High-Level Observations

As we gain some insight into the higher regions of the atmosphere (30 to 100 km), here are some of the things that we observe:

1. The flow and the thermal structure in the upper stratosphere (30-50 km), the mesosphere (50-80 km), and lower ionosphere appear to be at least as complex as those that we see on our usual upper air maps for the lower regions. There are moving disturbances, and at certain times there are dramatic and violent changes in the flow that appear to start at the higher levels and progress downward. One such phenomenon known as ''stratospheric sudden warming,'' occurring in the late winter or early spring, has been observed repeatedly.

2. A well pronounced semiannual variation of the wind and horizontal temperature gradient has been deduced from meteor-trail observations at 80 to 100 km, and this

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*See Appendix I for list of participants in the working group on Meteorological Rockets and Satellites.

suggests that in some respects the lower ionosphere is coupled with the regions above, where a similar semiannual effect has been observed from satellite drag measurements.

3. At the same time, the fact that synoptic scale disturbances at 80 to 100 km are sometimes in phase with the circulations at around 16 km and below for periods of 10 days or more suggests the existence of a strong link between the lower ionosphere and the rest of the atmosphere below it (Kochanski, 1962).*

4. There are many pieces of evidence to show that the lower ionosphere is strongly affected by changes in solar radiation and by charged particles flung out from the Sun during solar storms.

In summary, the region between 30 and 100 km is known to be the seat of many phenomena that are related to the rest of the atmosphere; it is a link between the part of the atmosphere that is most sensitive to solar changes and the lower atmosphere, and most of it is inaccessible with present observing systems.

C. World Meteorological Network

We note that a number of independent scientific committees have strongly urged a program of meteorological rockets to probe the atmosphere above 30 km on a synoptic basis, among these the ICSU's Comite Internationale Géophysique (CIG) and Committee on Space Research (COSPAR), the World Meteorological Organization's Committee on Aerology, and the National Academy of Science's Space Science Board and Committee on Atmospheric Sciences. Meteorological rockets are specified as an important part of the International Year of the Quiet Sun (IQSY). A Northern Hemisphere map showing the "Potential Meteorological Rocket Network" (as proposed by COSPAR May, 1962) is appended. COSPAR strongly urged an increase in the number of stations from the present 20 to 25 to at least 50 in the Northern Hemisphere. With such a virtually universal endorsement, the question is: What is being done to provide such a capability?

D. Findings and Recommendations

We have reviewed the excellent NASA survey of meteorological rockets by William Spreen, and concur in his findings, which can be summarized as follows:

> FINDING: Present Arcas and Loki II meteorological rockets give data on winds below 60 km (occasionally higher), and the Arcas rocketsonde gives reliable temperatures below about 45 km. The Arcas falling sphere (Robin) may give densities to an uncertain degree of accuracy (perhaps 5 per cent) from 60 km (occasionally higher). Thus, we are now using a meteorological rocket system that has a ceiling at about 60 km, a ceiling imposed by both rockets and payloads.

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*Kochanski, Adam, "Circulation and temperatures in the 70-100 km height region," submitted to J. Geophys. Research, 1962.

FINDING: The rockets with their payloads are between 50 and 75 per cent reliable, there having been runs of poor rockets and some runs of nearly complete successes. Statistics on this point are hard to evaluate.

FINDING: Since the rockets are usually fired from test sites, quite large radars are available for tracking and wind determination. The most common wind observation is with falling chaff. There have been a few firings in which double radio tracking with two GMD-1's has been achieved, and this system gives reasonable results when everything works. A rocketsonde compatible with the GMD-2 has been tested, and gives range, azimuth and elevation from one ground station. This adaptation of a balloon system is difficult to use with rockets, however, since it is a phase-comparison technique and any loss of transmission therefore results in ambiguity in range. In short, there is no satisfactory rocketsonde system capable of giving winds at sites where no big radars exist.

FINDING: Special techniques have been developed for wind determination with larger rockets, the most commonly used being the vapor trail (which can be used only at twilight under clear weather conditions) and the series of grenades (which requires accurate tracking, an array of microphones on the ground, and complex data reduction). Winds have also been measured from stabilized rockets to heights over 100 km by observing the differential pressure on the sides of the rocket, thus giving the sideways component of the relative wind past the rocket. (These other techniques were not covered in Spreen's report.)

FINDING: A direct measurement of temperature with an exposed element is probably impractical above 45 or 50 km because of radiation effects, but pressure has been measured to heights well above 100 km on many occasions. Densities have been measured from 100 km down with various kinds of falling spheres.

Using the current meteorological rocket systems from a network of well equipped firing ranges has given meteorology its first chance to enter the region above 30 km on a synoptic basis. Nevertheless these systems are far from adequate. We therefore recommend:

RECOMMENDATION: That NASA and the Department of Defense pursue a joint development program to provide a first-generation meteorological rocket system capable of reliably probing the atmosphere between 30 and 60 km, such a system being able to measure winds, and pressure or temperature or density versus height. It should be a self-contained system, capable of operating without the help of a large radar. Development programs already underway by the Department of Defense, given some adequate financial support and direction, can probably be expected to produce such a system in time for the IQSY.

RECOMMENDATION: That NASA initiate a development program to provide a second-generation meteorological rocket system capable of reliably probing the atmosphere between 30 and 100 km or above, such a system being able to measure winds, pressure or temperature or density versus height, and electron density (the last only above 60 km).

If possible, it should be a self-contained system not requiring large radar support. This program will involve the development of a new rocket that costs under $2000, can carry about 10 kg to over 100 km, is stable in flight, and has a 3σ dispersion on impact of less than 10 km. (Reference is made to minutes of Space Science Board Committee on High Altitude Rocket and Balloon Research, meeting of May 17-18, 1962, for a further discussion of this subject.) This program will also involve a new and imaginative approach to the sensors to be used, since no package now exists that could make all these measurements from a reasonably small rocket -- though the individual techniques are probably at hand.

RECOMMENDATION: That a group of experts be convened by the Joint Meteorological Rocket Network Steering Committee (JMRNSC) to make a careful study of the best methods for publishing and storing meteorological rocket data and of techniques for their international exchange through WMO. Publication and storing procedures have been worked out for the current North American Rocket Network; and as other countries begin to use meteorological rockets, international exchange agreements will become important.

II. Satellites

A. Preliminary Results

In the two years since the first TIROS meteorological satellite was launched there have been obtained a very large number of pictures (over 100,000 are now in the Weather Bureau files) and a vast amount of infrared data (some of which have been published by NASA-USWB). The question is sometimes asked: Have any new scientific results come from this effort? Now that we have all these observations, do we know any more about the atmosphere than we did before? Here is a partial answer:

1. TIROS I had only been up one day when it sent back its first picture of a well developed storm system over the North Atlantic, and meteorologists clearly saw for the first time that such a system had a spiral-band pattern similar to a hurricane. Our conception of such storms will now have to be revised.

2. Mountain-wave clouds have been known and partially understood for many years, but it came as a great surprise to see such a series of regularly spaced clouds extending from the Andes Mountains across the width of the South American Continent. Moreover, as seen from a satellite there is a short-wave and long-wave structure to such major mountain-wave cloud systems, a complication that still needs to be explained.

3. Over the oceans clouds form by convection when the surface is warmer than the air, and these clouds as seen from a satellite align themselves in rows or "cloud streets" under certain conditions of wind and wind shear. Under conditions of strong inversion of temperature and little vertical wind shear the clouds tend to form "cellular" patterns. These cloud arrays are so striking when seen from a satellite

that a number of able theoreticians have been inspired to work on the laws governing these convective processes in the atmosphere. For the first time there is now a clear indication of the cloud patterns under various conditions against which to test new theories.

4. On several occasions isolated cloud masses containing one of nature's most devastating storms, the hail-producing tornado, have been spotted in TIROS pictures. The practical implications of this discovery to tornado warning are evident, but it has also spurred a new look at how such a phenomenon can occur.

5. The birthplaces of hurricanes have always been one of the mysteries of meteorology. They are known to form in easterly currents of tropical air, and it was generally assumed that they would have to begin over the ocean, though the complete life story of a hurricane has never been told. Last year Hurricane Anna was first spotted by satellite pictures over the Atlantic, though it was not identified as a hurricane until several days later. It developed from a disturbance which had originated in Africa a few days earlier. Is Africa a source of Caribbean hurricanes? Now we can follow them from birth to death, and the answer will lie in the sequences of cloud pictures. It now also seems possible that the typhoons of the West Pacific may originate over or near Central America, a region of the world that has hitherto been a virtually blank space on the weather map due to lack of weather observations.

These are only five of the more striking discoveries that have come from TIROS, and there are many more on the way. Among the results to be expected are better estimates of the Earth's albedo, a description of the varying sources and sinks of the heat energy that drives the atmosphere, and even the way in which the ice packs spread in the fall and break up in the summer. Cloud pictures have already demonstrated their value in defining the wind-flow patterns over the oceans of the world where weather observations are scanty or entirely lacking, and these flow patterns must be properly drawn as a first step in any hemispheric numerical weather prediction.

B. Conclusions and Recommendations

The answer to the question of whether weather satellites have given us new insight into the atmosphere is a very definite, "Yes!" But this raises a new set of questions: If these new observations are so valuable, are they being used to best advantage? Are scientists aware of how to use them, and are they being encouraged to use them? Are the observations in an easily accessible form? Are we measuring the most desirable parameters; and, if not, has the success of the cloud pictures obscured the need to develop new techniques? Would it be a good idea to create a center where the data could be stored and where research with weather satellites could be pursued in a more concentrated way? Or are there other ways to encourage research in this area?

NASA and the U.S. Weather Bureau have made a considerable effort to acquaint the scientific community with the meteorological satellite data. Announcements and technical papers have appeared in such journals as the Bulletin of the American Meteorological Society, Monthly Weather Review, Transactions of the American Geophysical Union, Quarterly Journal of the Royal Meteorological Society, Journal of Geophysical Research, etc. (The U.S. Weather Bureau alone has, as of 15 June, 1962, published 82 papers, many of them in 19 separate periodicals.) At the meeting of COSPAR in Washington in April 1962 there were six papers presented by European meteorologists using TIROS observations. Catalogues of satellite pictures and the pictures themselves have been distributed by the U.S. Weather Bureau to every U.S. university with a meteorology department and to many other organizations. NASA and the USWB havedistributed the "Big Red Book" of infrared data to a similar list of meteorologists and institutions. It is clear that any reputable meteorologist can get the information he needs about these satellite observations. The concern of both the USWB and NASA is that, in spite of their efforts, too few competent meteorologists are in fact working with these observations.

The next generation of meteorological satellites, the NIMBUS, will produce even more useful data, since the coverage is world-wide and the problem in location and rectification of pictures will be diminished. Moreover, some of the infrared data will be easier to compare to the cloud pictures and may therefore find more use. As these improved data become available on a current basis to a wider audience of forecasting meteorologists, new operational uses will be found for them, new empirical relationships will be discovered, and their application to meteorological research will become as universal as radiosonde data.

Some of the properties of the atmosphere that meteorologists are accustomed to using, such as measurements of winds, pressures, temperatures, etc., may be impractical to measure from satellites. On the other hand, satellites provide a platform for viewing the atmosphere from a new and unique point of view.

> RECOMMENDATION: A study of new methods for observing the atmosphere from satellites should be pursued, since the full potential of the satellite observing stationmay not be realized until new ways of observing the atmosphere have been developed. Such a study should involve consideration of passive and active methods for use in all parts of the electromagnetic spectrum, and of ways to relate these novel measurements to the behavior of the atmosphere itself.

The quantity of information that a satellite can acquire could be far greater than the information that it can send to the ground through even the most elaborate and powerful communication link.

> RECOMMENDATION: Studies of the usable information content of various kinds of meteorological satellite observations, methods for reducing redundancy, and methods for automatically assessing the information content and transmitting it to the ground should be actively encouraged. It is noted that such techniques would be of general use in transmission of meteorological data from station to station.

Most important of all, it is expected that, in the best tradition of scientific endeavor, certain scientists will become interested in particular kinds of meteorological satellite observations and will develop a special competence in some area of meteorological satellite research. This has in fact already occurred in some instances.

RECOMMENDATION: Such potential individuals or groups should be identified at an early date and encouraged to assume a responsibility for exchanging ideas and encouraging research in their area of competence. Thus, nuclei of scientists will evolve that will be willing and able to serve as foci for research activities in limited areas by making certain kinds of data more easily available, holding conferences on special subjects, and in general assuming the role of leaders in the exploitation of this newest school of meteorology. This will happen through a natural, evolutionary process; but they can only do this with the wholehearted support of the government program offices, following a coordinated plan of action.

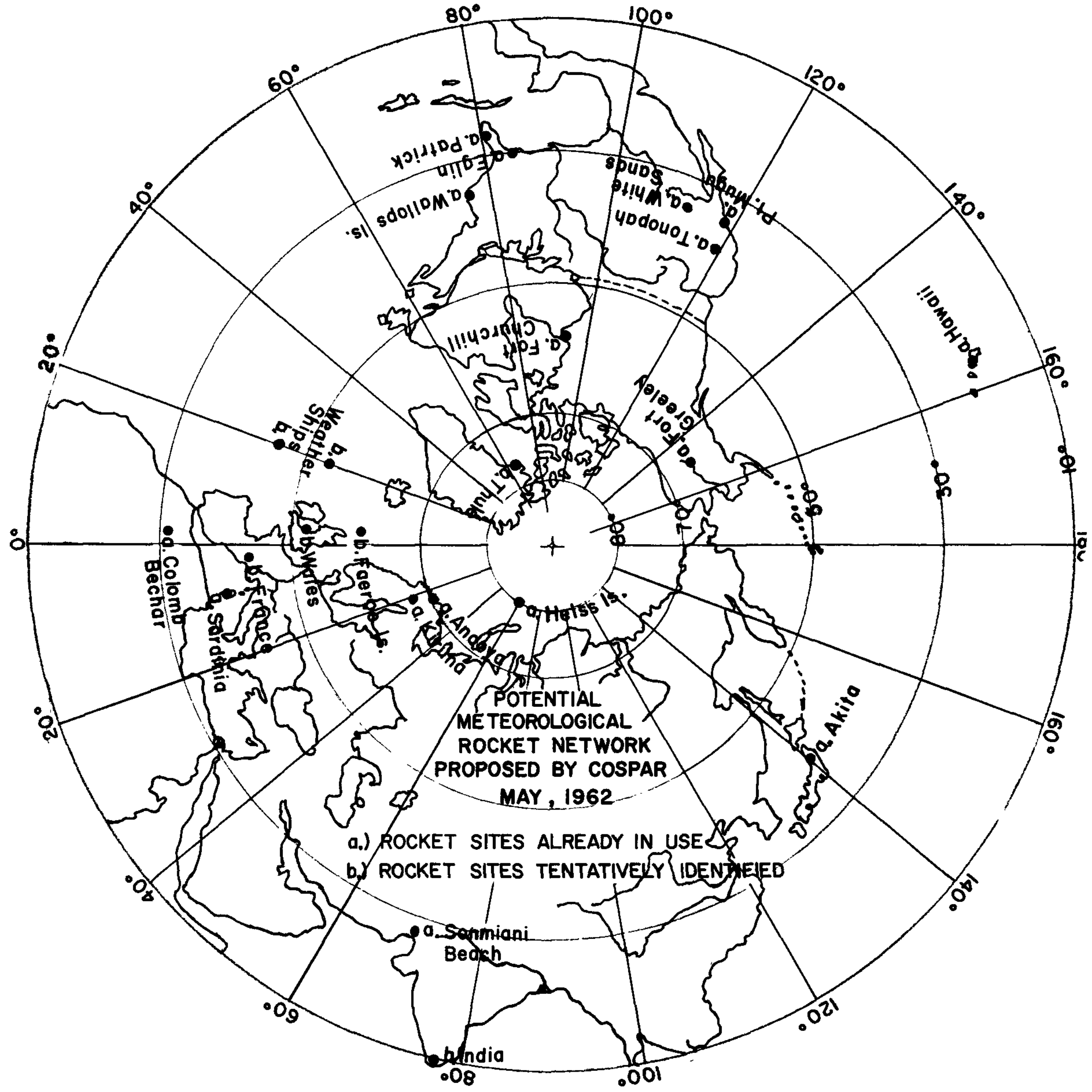

FIGURE 1

Potential Meteorological Rocket Network, proposed by COSPAR, May 1962.

Appendix I

Principal participants in the Working Group on Meteorological Rockets and Satellites included the following:

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Chapter Nine

BIOLOGY*

I. Introduction

The biologist's task in space science may be divided arbitrarily into three categories. First and -- for reasons developed later -- foremost, is the search for extraterrestrial life. Second is the immense task for the biological engineer of putting man into space adequately protected from the peculiar hazards of space and adequately sustained by a good simulacrum of his terrestrial environment. Third is an exploitation of special features of the space environment as unique situations for the general analysis of the organism-environment relationships including, especially, the role environmental inputs play in the establishment and maintenance of normal organization in the living system.

Superficially these three aspects of space biology are very different in character, in importance, and in justification. They are, furthermore, so disparate in their emotional and intellectual appeal that we have a heavy responsibility to appraise their relative importance as carefully as possible without at the same time -- by the very exercise of sober care -- stifling that element of challenge and high adventure that has characterized the scientific enterprise since it was put on its modern course by the brave minds of the Renaissance.

Later sections of this report address themselves to details pertinent to each of the three aspects of space biology. The purpose of this general preamble is to explicate, as we see it, the general philosophy that should guide the over-all enterprise of space biology.

If one looks at the third category -- the utilization of space environments as tools for analyzing the organism-environment-relationship -- he is faced with a prospect of certain dividends but at huge cost, and dividends, moreover, that insofar as they are certain are neither large nor revolutionary. If we limit our outlook to such dividends (those that are certain) and center our attention on the nonsense that a program like this must inevitably invite in its early stages, it is hard to escape cynicism about the vast sums that will be spent and a revulsion from the mixture of mediocrity, nonsense, and huge cost in which the NASA program could become involved if extreme care is not taken. A similar cynicism about putting man in space derives on the one hand from the staggering cost involved, and on the other from the project's early association with cereal boxes and comic strips. Most of us at one time or another have been beset by such cynical doubts. However, we have undertaken a careful scrutiny of the over-all potential for space biology in the NASA program. It is our considered judgment that the exploration of space will prove to be one of man's truly great adventures.

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*See Appendix I for list of participants in Working Group on Biology.

To delimit the scope of opportunities for environmental biology in the NASA program seems deceptively simple. In the space environment we can identify several factors of obvious biological importance which differ quantitatively and even qualitatively from those which obtain on Earth. Space can provide us with unique environmental variables (or with otherwise unattainable ranges of a familiar variable) to accomplish critical tests of existing biological theory. We cite briefly as examples the attainment of near weightlessness and the imposition of an environment unequivocally disconnected from the Earth's rotation. In both instances there exist theoretical bases for predicting more than one pattern of growth and behavior to be expected of organisms exposed to these features of the space environment. Patterns of orientation, growth, and other responses to the Earth's gravitational field are identifiable in all phyla of both animal and plant kingdoms. Circadian rhythms (those having a period of about one day) in divers biological parameters also have been observed in a large variety of plants and animals. The mechanism of the biological sensing apparatus, whether for gravity or for time, is unknown or incompletely known. In the case of gravitational responses there are a number of sensing mechanisms which differ in principle. For the apparently ubiquitous circadian rhythms displayed by different species in many different ways we have no sound basis for deciding whether in principle nature employs one or more than one fundamental mechanism to achieve the responses we can observe. Regretfully we recognize that theoretical analysis fails to provide neat predictions of the behavior of biological systems in the space environment on which most biologists are agreed. There is still so much ignorance of biological organization and so little theory in the proper sense of that term that we are much more likely to encounter unexpected results, to detect relationships we do not know how to anticipate, and to be surprised by the behavior of biological systems in respect to the unique aspects of the space environment, than is the case for non-biological systems. Therefore, it seems much more than idle speculation to suggest that, insofar as space provides opportunity for truly new types of environmental exposure of known biological systems, it not only offers means of testing -- sometimes critically -- those relationships we now think are understood, but it also invites the discovery of unforeseen relationships. Space experiments are too expensive and too difficult to be designed in cavalier fashion by trying out anything and everything in hope of a new discovery of fundamental importance; nevertheless, programmed serendipity is not really a contradiction and it should play a role in our long-range thinking about environmental biology in space.

We are, however, in general agreement that this category of effort in the space program, environmental biology, is subsidiary in importance to the search for extraterrestrial life. The peculiar point is that in this search we can be even less certain of the dividends which the cost in money and effort will return. It may be that Mars, the only serious candidate for another home of life in our solar system, will prove barren when we get there. But we have not the slightest hesitation, while facing fully our responsibility as members of society, in urging strongly that the venture be undertaken. More than that -- unless we wholly mistake the nature of man and science -- we are not really free to eschew the challenge and the opportunity to pursue this goal that rocketry has placed within our grasp.

We are fully aware that what follows -- the justification for setting the search for extraterrestrial life as the prime goal of space biology -- has a far greater philosophic appeal than has been fashionable in scientific discussion for some time. But it is not since Darwin -- and before him Copernicus -- that science has had the opportunity for so great an impact on man's understanding of man. The scientific question at stake in exobiology is, in the opinion of many, the most exciting, challenging, and profound issue, not only of this century but of the whole naturalistic movement that has characterized the history of western thought for three hundred years. What is at stake is the chance to gain a new perspective on man's place in nature, a new level of discussion on the meaning and nature of life.

The Darwinian revolution in biology had as one of its implicit propositions the notion that the development of life itself was only one chapter in the natural history of the planet as a whole. Oparin later made this notion explicit in his view that the origin of life was a fully natural -- and in some sense inevitable -- step in the ontogeny of the Earth. Those chemical systems capable of replication and controlled energy transfer which we call living systems had their origin in the sequence of chemical changes that were part of the planet's early history. This view has inevitably led to questioning the uniqueness and centrality of man in the universe in an even profounder way than the Copernican and Darwinian insights forced upon us. If planetary systems like our own are at all common in the vast reaches of the universe, life may also be almost as common in nature as a whole. It is surely unnecessary to belabor the immensity of this prospect for any man's philosophic position; or to belabor the pusillanimous and provincial viewpoint that would shrink from pursuing it.

It is useful to recall here a few of the more specific points that the biologist himself finds so exciting. The exploration of Mars, which the new technology makes an almost certain prospect within a decade or two, will allow us to pursue the question of how common life is. Given the estimate that there are something like 10^{20} stars in the universe, we need to know first, what fraction of these have planetary systems comparable to ours;* and second how "inevitable" -- or how probable -- is the origin of life in the natural history of a planet. It is the second question which in principle we can now begin to answer by an empirical exobiological research. If there is life on Mars, and if we can demonstrate its independent origin (from its chemical basis, etc.), then we shall have an enlightening answer to our question of improbability and uniqueness in the origin of life. Arising twice in a single planetary system it must surely occur abundantly elsewhere in the staggering number of comparable planetary systems.

If there is life on Mars, the biologists' interest will not stop with its discovery. In one sense the quest will have only begun. The analysis of Martian life -- or even Martian paleontology, if the planet has passed through that phase of its development during which it could sustain life -- will be a grand extension of the comparative method that characterizes most of biological analysis. Shall we find on Mars some of the unexplained peculiarities of terrestrial biochemistry? If amino acids are involved, are they D or L forms, or both? Does adenine, for instance, again play so many roles? Is the energy transfer system again based on phosphorus? What is the genetic mechanism like? What information storage and replication systems are involved? Has sex again been evolved or have some other devices arisen to engender genetic variability? At higher levels of organization, how have these systems adapted to peculiar features of the Martian environment including its ranges of temperatures and moisture, its thin atmosphere, and its low gravitational field? What has been the course and mechanism of Martian evolution?

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*The estimate of Huang puts the number at 10^{18}. Huang, Su-Shu, "Occurrence of Life in the Universe," American Scientist, 47, 3, pp. 397-402 (Autumn, 1959). If the upper limit of star systems that can support life is 5% and if there are 10^{20} stars in the observable universe, then 10^{18} systems capable of supporting life is a reasonable guess.

This search for life elsewhere will inevitably demand that man must get into space himself. The search for life on Mars, for example, ultimately will require that Martian samples be studied in manned -- preferably terrestrial -- laboratories. Many of us believe that the retrieval of Martian samples should be recognized explicitly as the ultimate objective of exobiological missions. It is doubtful if such retrieval can be accomplished satisfactorily by an unmanned expedition. Of course we shall get on with the job using remotely controlled life-detection systems even before this venture is fully possible. But we shall never be satisfied with negative results from our instrumental life-detectors because they are intrinsically hampered in their scope by our current ignorance of the nature of whatever extraterrestrial life there may be. Should these preliminary sallies give positive results, the urgency for man to get there will only increase. Therefore, while the intellectual appeal and status of the extraterrestrial life question stands out above all else in space biology, we are also confronted with the important task of providing the physiological basis for prolonged manned space flight.

Finally, it becomes clear that what the physiologist has to do to put man in space for any reasonable length of time is to learn far more fundamental environmental physiology. The three aspects of space biology enumerated at the onset are not, after all, so very different in aim nor disparate in importance. In a real sense they are three aspects of one and the same undertaking -- a general extension of the scope of nature that the biologist can bring within his grasp.

In addition to cosmobiology, man into space, and environmental biology, we feel that a fourth point warrants emphasis, not because it is logically coordinate in importance but because of its practical, economic, and scientific impact on experimental biology of all kinds. We predict as confidently for space biology as for other space sciences that the economic costs will be amply repaid in the long run by applications of space-oriented biotechnology to other fields of biology and medicine. We are not unmindful of these inevitably substantial though indirect contributions of NASA's continuing efforts in space biology.

II. Current NASA Programs in Space Biology

Various NASA efforts outside of the biological area were outgrowths of the IGY program. Chiefly for that reason, space biology, which was not a significant part of the IGY effort, was relatively late in taking shape as a NASA program. We find that it is not yet a full-fledged, on-going effort. Partly this is attributable to a slow start, partly to difficulties in NASA becoming fully staffed, partly to lack of coordination of activities of several offices each of which is involved in some way with space biology, and partly to the fact that bioscientists have been slow in seeking involvement in space research.

We concerned ourselves primarily with scientific matters with the intent that we would thereby provide a guide for the subsequent development of technical and management policies within NASA. In certain instances recommendations on administrative policies devolve rather directly from our attempts to establish scientific bases and guidelines for the space biology program.

Viewed as a whole, the NASA effort in space biology is as yet sporadic. Support has been directed principally to projects whose goal is to get man into space. The scientific justification for emphasis on manned space flight has been that it will be a step toward the scientific exploration of space. We realize that there are additional, practical, and sometimes overriding reasons for what seems to be a disproportionate emphasis on the manned space flight program. We should hope that NASA long-range planning will continue to distinguish between means and ends, for we believe there may be some danger in encouraging accelerated efforts to get man into space without NASA having clearly in view what he is to do there.

III. Terrestrial and Extraterrestrial Studies of the Origin of Life

The search for extraterrestrial life readily captures the imagination of laymen and scientists alike. The biologist is deeply concerned with the origins of life, and he now seeks empirical evidence of life existing elsewhere in the universe because of the commanding importance of such information with respect to the fundamental problem. If life exists on another planet, its study becomes an important facet of the scientifically broader investigation of mechanisms of the origins of life. The term, cosmobiology, has been suggested as useful to encompass a wide range of both terrestrial and extraterrestrial studies directed toward more complete understanding of the origin and distribution of living forms anywhere in the universe.

The two principal approaches to cosmobiology are Earth-based studies on the origins of terrestrial life, and the examination of other planets to determine whether life has also occurred there and how it began. Since chemical principles presumably hold in all locales, laboratory experiments on Earth can reveal which materials and conditions available in the universe might give rise to chemical components and structural or behavioral attributes of life as we know it.

The NASA program already supports laboratory investigations of this kind. Included are studies of the production of small organic molecules such as amino acids, pyrimidines, sugars, vitamins, and porphyrins from inorganic precursors, the generation of macromolecules -- large molecules such as proteins and nucleic acids -- from smaller units, and the conditions which lead to the production of still larger structural units which may serve as models of precellular organization. The program also supports investigations of terrestrial organisms living under extreme conditions of temperature, pressure, water availability, etc., in either natural or artificial environments. Studies on the survival of terrestrial organisms trapped in hard deposits also are relevant.

FINDING: We endorse the support of high-quality, ground-based studies of abiogenesis.

Upon direct examination of extraterrestrial bodies which are likely abodes for living matter, we may indeed encounter extraterrestrial life. The problem would be sharpened immediately as we press for information about the kind of life it is from biochemical, biophysical, and ecological standpoints. If there are found, say, Martian organisms which are fundamentally similar to our own, this enhances the likelihood of their common origin with terrestrial organisms. Such evidence will shed new light on the question of interplanetary transfer of living matter and on the question of parallel evolution.

The particular clusters of organic compounds which exist on other planets will make unique contribution to our understanding of molecular evolution leading to life. Detailed inventories on organic chemical systems of lifeless planets will thus have important biological significance.

> FINDING AND RECOMMENDATION: We endorse the energetic pursuance of the search for extraterrestrial life and organic compounds as an important step toward a unified view of cosmobiology. A high priority should be attached to this enterprise.

IV. Expectations for Detecting Life in Specific Regions

A. Moon

The exposed lunar surface is considered not to be habitable by terrestrial organisms. The possibility remains that life survives at some depth beneath the surface where conditions may be more hospitable. Without better information about the nature of the lunar surface and especially the subsurface we have no means of estimating the probability of living matter residing within the Moon. The undoubtedly hostile surface environment encourages very low estimates of the probability of any lunar life, but, even though the estimates are low, they are not zero and cannot be made so by any new information we are likely to obtain short of actual on-site exploration.

It seems more likely that the Moon harbors fossil evidence of extinct life, but the probability of this also cannot be estimated with enough confidence to make such estimates quantitatively meaningful. Paleontological evidence of past life on the Moon would be only slightly less exciting scientifically than evidence of present life there. Presumably we shall have to await manned exploration or the return of lunar samples before this topic can be profitably investigated.

It should be noted that lunar samples should contain dead organisms accumulated from space if the panspermia hypothesis is valid.

B. Venus

This planet apparently is quite inhospitable to life as we know it because of its high surface temperature. The existence of life in the cooler cloud layer cannot be ruled out. Thus, Venus should not be abandoned as a possible exobiological objective. Nevertheless the likelihood of Venus as an abode for extraterrestrial life may be considered perhaps comparable with that of the Earth's Moon.

C. Mars

At present Mars seems to be by far the most important area for exobiological investigations. Evidence already at hand suggests that the Martian environment is capable of supporting life. Mars has an atmosphere in which clouds can be seen. Carbon dioxide has been identified in its atmosphere and water is assumed to be present partly since polar caps presumed to be ice are seen on the planet and these wax and wane with the seasons. Temperatures as high as 25°C have been measured at the equator, even though the nighttime temperatures (not directly observable) must be very low, owing to the extreme dryness of the atmosphere (lack of substantial "greenhouse effect"). Phenomena associated with the dark regions (maria) on the Martian surface are suggestive of life. The maria show seasonal color changes, possibly related to cycles of biological activity dependent on the seasonal fluctuations of water vapor in the atmosphere. Furthermore, spectral features in the infrared corresponding to C-H resonance frequencies have been found in light reflected from the maria. These features are not seen in light from the Martian deserts. Still other arguments for (and against) life on Mars have been adduced, but will not be given here. Suffice it to say that available evidence is such as to add zest to the coming exploration of the planet.

Since Mars is the most likely candidate as a habitat for extraterrestrial life within our solar system, we have a unique scientific obligation to maximize the exobiologically significant information we can obtain from this planet. Life-detection studies on Mars may be considered in two categories.

1. From a Distance. Short of a landing on the planetary surface, gathering evidence of life on a planet other than the Earth is confined by present technology to methods employing sensors for some portion of the electromagnetic spectrum.

(a) Ground-based astronomy has provided most but not all the data of interest in this connection which it is capable of obtaining.

(b) Spectroscopic observations from balloons and orbiting observatories or from a lunar observatory could provide still more useful information.

(c) Spectroscopic detection systems on fly-bys can furnish information urgently needed for furthering exobiological research.

(d) Good photographs (visible region) of the planetary surface taken from a fly-by might furnish definitive evidence of exobiological import. Unfortunately, the early vicinal approaches to Mars probably will not come close enough to the planet for photographs to provide information of maximal biological interest.

(e) So far, optical studies from astronomical observatories have employed light reflected from the planets. In the microwave region power has been beamed to planetary targets and returning signals analyzed. Such radar methods in principle could be extended to the visible region through the use of lasers. Probably this will require new laser techniques which in any case are being developed for other purposes. The special point here is that ground-based observatories or fly-bys might employ radar-type laser devices for high resolution detection systems. Laser technology, it is likely, in a few years will have progressed to the point where Earth-based observations with essentially monochromatic light can provide information on the Martian surface with resolution of a small fraction of the disk. It is not yet known how many and which wavelengths will become accessible, but if a large number of visible wavelengths can be used, this technique would have great exobiological interest.

RECOMMENDATION: NASA should be alert to the potential of laser techniques to provide planetary data of biological interest; it should support research and development which will advance this technology.

Whether the observations are from ground-based observatories, balloons, or fly-bys, spectroscopic studies should carry high priority from the viewpoint of biology. With regard to Mars specifically, we badly need to identify the components of the Martian atmosphere and to determine the distribution of organic matter on the surface.

RECOMMENDATION: High priority should be accorded the acquisition of chemical information relating to the atmosphere and surface composition of Mars.

Of special biological interest are: O_2, CO_2, CO, CH_4, H_2S, NH_3, H_2O (including water of hydration), and organic compounds in general. Ecological parameters including temperature, pressure, ultraviolet and ionizing radiation flux also are biologically important and their measurement should be attempted.

2. From Landing Spacecraft. So far we have considered only the kinds of evidence which would have an indirect though important bearing on exobiological research. There are in addition life-detection experiments capable of providing more or less direct evidence of the presence of living matter. For biological as well as for other interests, a television survey of macroscopic structures will have great appeal. In addition to this obviously desirable life-detector device, four other kinds of experiments were considered.

(a) Microscopic examination of samples. In principle the inspection of enriched samples at various levels of magnification could give decisive evidence of structural peculiarities characteristic of living units. Such an experiment is now under development. Major operational problems appear to be sample collection and transmission of the vidicon images.

RECOMMENDATION: The collection (and enrichment) of samples of the Martian surface for microscopic examination and the transmission of vidicon images (with or without data reduction) are areas which warrant accelerated effort.

(b) Detection of physiological activity. Since one does not know what to look for, only the most ubiquitous metabolic capabilities of terrestrial organisms seem worth searching for. The bulk of the biomass on Earth consists of photoautotrophs (organisms which convert light energy into organic chemical bond energy as a basis for growth and energy storage). In the case of terrestrial photoautotrophs, this fundamentally important energy conversion process is called photosynthesis. Autotrophs may reasonably be expected to be abundant if not predominant in any other ecologically stable life system. Constraints of organic chemistry make it highly likely that the analogous process of energy conversion by extraterrestrial photoautotrophs would, like photosynthesis, use CO_2 as a raw material. Collected samples might be presented with labelled CO_2 and a measure taken of fixed CO_2. On several counts, a life-detection experiment based on light-dependent CO_2 assimilation seems worthwhile: (a) photoautotrophs are likely to be an important component of the Martian "flora," (b) substrate choice is simplified in that a recognizably specific substrate, CO_2, can be employed, and (c) a light vs. dark comparison of CO_2 uptake processes would reduce the likelihood of misinterpreting results. We are not aware that an experiment of this kind is under development.

RECOMMENDATION: NASA should support the development of a life-detection experiment based on CO_2 assimilation.

One also may search for evidences of heterotrophic conversion of appropriate chemical substrates into metabolically generated CO_2. Labelled substrates would lead to the production of labelled CO_2 the detection of which would be highly sensitive and specific. Conditioned by terrestrial experience, we would expect this test to succeed for a very wide variety of microorganisms which are capable of metabolizing the same substrate. The danger that an appropriate (metabolizable) substrate may not be included has been recognized by those who are responsible for this experiment which is at an advanced stage of development.

FINDING: We endorse the current development of a life-detection experiment based on metabolically generated CO_2.

(c) Detection of enzymatic activity. A judicious choice of enzyme systems could lead to a highly specific test of enzymatic activities in a Martian sample. Naturally one should select enzymes of fundamental biological importance and wide distribution in our own biosphere. However, the very specificity of such test systems may defeat the purpose of the experiment. Choice of the right substrate may be fortuitously easy or it may be as difficult as finding in some remote place a key which just happens to fit the lock on the front door. These difficulties have been considered by the scientists responsible for this experiment which now is under development.

FINDING: We endorse the current development of a life-detection experiment based on enzyme activity.

Consideration was given to the current status of the program for developing optical microscopic, metabolic, and enzymatic life-detection experiments slated for exploration of the Martian surface. While the experiments are imaginative in design they do not by any means exhaust the range of possible kinds of life-detection devices which might be developed. Relatively few biologists or bio-engineers appear to be devoting their principal energies to this enterprise. It seems possible that not enough thought has been given to back-up experiments in this area. Moreover, it is not clear that the development of already-programmed experiments is proceeding as rapidly as would be appropriate. In particular, it does not seem that enough effort is being devoted to the problem of sample collection and enrichment. Therefore, it remains an open question whether this program is moving at a pace which will insure the inclusion of life-detection experiments in the first vehicles we shall send toward Mars.

FINDING and RECOMMENDATION: We are not convinced that enough effort is being devoted to life-detection experiments and to developing means of collecting planetary samples under aseptic conditions. NASA should examine this matter carefully, and if necessary, the program should be increased in vigor so that maximal information can be gained before contamination of terrestrial origin destroys a unique opportunity.

If there indeed is a highly evolved Martian biota, reconstruction of Martian ecology by the tedious sorting out of information gained after contamination may prove impossible.

(d) Detection of motion. Assuming that forms larger than microorganisms may occur on the Martian surface, evidence of their existence should be obtainable using devices of varying degrees of sophistication. If there are large plant-like forms with aerial parts moving as a result of atmospheric convection or animal-like forms capable of locomotion, motion detectors could be employed successfully. One of the simplest devices to instrument would be a microphone. It is not apparent that the program concept includes such an obvious experimental procedure.

> RECOMMENDATION: We recommend that a microphone be included as a relatively simple life-detection device for an instrumented landing on Mars.

D. Interplanetary Space

According to the panspermia hypothesis, living entities have become dispersed throughout the universe by interplanetary migration. The probability of such units surviving space hazards for long enough times to effect interplanetary transfer seems low. This in itself does not preclude the dispersal of non-viable, organic, formed elements in space and gives us no clue as to the frequency with which such dead structures are apt to be encountered. Also, if one chooses to refute arguments in support of panspermia on the grounds that potential disseminules would be destroyed in space, that would involve the tacit admission that some not-yet-killed units may be present in deep space.

> RECOMMENDATION: The biological exploration of interplanetary space should be seriously attempted.

It may not be feasible to combine in one experiment the search for interplanetary life with the collection of interplanetary matter in general. From the biological viewpoint the possibly organic chemical composition of micrometeorites could be of considerable importance here as well as in the collection of lunar and planetary samples.

> RECOMMENDATION: In the collection of lunar and planetary samples, care should be taken to conserve carbonaceous compounds for their biochemical interest.

This specification will restrict the design of collecting devices to avoid sample contamination by organic (albeit sterile) materials.

It was suggested that special techniques already developed for the processing of samples to be examined electron-microscopically could be adapted to the examination of extraterrestrial matter. The use of thin graphite or single-crystal stable coherent films, replication, and shadow-casting are mentioned in this connection. For instance, by coating a portion of a sampling device with these thin films, and of course, protecting the collecting surface against destruction on re-entry, then stripping off pieces of film for examination by electron microscopy, one might gain direct access to a particle-size region which extends well below the resolving power of a light microscope.

Electron-optical techniques are ideally suited to the examination of particles in the size range, 10 to 1,000A. Requisite conditions for electron microscopy: high vacuum, thin specimens, and low temperature (for the recently introduced low-temperature electron diffraction and electron microscopy) are similar to conditions encountered in outer space. With present advances in the generation of stable, superconducting, electromagnetic fields, it is reasonable to contemplate the design of light-weight, high-resolution, cryo-electron microscopes operating at liquid helium temperatures and permitting the examination of interplanetary matter to be performed by electron-optical techniques in the space environment itself. Such experiments could partially replace or profitably supplement the use of light microscopy for similar purposes.

> RECOMMENDATION: Research and development on electron optical techniques adaptable to the space environment should be initiated.

Fallen meteorites offer the opportunity of sampling interplanetary matter. Photographic tracking, subsequent recovery, and appropriate analysis of recently fallen carbonaceous chondrites would be of great interest to the biologist even though the chief purpose of the presently conceived tracking and recovery program is nonbiological.

> RECOMMENDATION: The collection and analysis of fallen meteorites should be pursued.

Unmanned recoverable satellites and the Gemini and Apollo flight programs offer opportunities for making collections of interplanetary matter. We are not aware that planning has progressed as rapidly as the importance of the subject warrants; however, the urgency of making such interplanetary collections is far less than that of sampling the planets themselves.

Related to this general topic is the collection of very high-altitude samples from our own atmosphere. There is presently a paucity of trustworthy information on the palynology of the upper reaches of the Earth's atmosphere.

> FINDING: The beginning of space is an arbitrary concept and the exploration of the vertical extent of our biosphere is a responsibility of NASA.

Moreover atmospheric sampling by space-probe techniques if developed appropriately would yield data which could be compared with results from sampling by devices carried on high-altitude balloon flights and thus could "prove out" the probe techniques. It would appear of particular merit to utilize the X-15 flights which go to altitudes of 50 miles and over for sample collections as an adjunct to the primary mission of these flights.

E. Planets Other Than Those in Solar System

Listening for evidence of intelligence in radio signals received from outer space (Project Ozma) represents a courageous search for much more sophisticated forms of life than we hope to encounter in any extraterrestrial habitat within our solar system. While chances of success would appear to be marginal, the effort should be continued. The project is not one which can be given high priority simply because of the long times involved in two-way communication, even though we hope the basis for such communication will be established and the technology developed to beam return signals of sufficient strength to be received. This project may prove to have an *importance* whereby it overrides all other exobiological studies in the long run, but it can never acquire the *urgency* which we now attach to immediate goals in our search for extraterrestrial life.

V. Protection of Extraterrestrial Regions from Contamination by Terrestrial Organisms

Evidence was considered that the Moon, Venus, and Mars might offer terrestrial organisms the opportunity for survival and growth. Available evidence is not definitive, but in each case we feel that there is no basis for ruling out in an absolute sense the possibility that terrestrial organisms could replicate. The likelihood of this happening on the Moon or on Venus seemed very slight, but Martian conditions certainly bring this possibility well within the realm of reasonable probability.

> *FINDING and RECOMMENDATION*: Present NASA policy of requiring sterilization of spacecraft which might intentionally or accidentally land on Mars is heartily endorsed. The JPL effort to develop adequate sterilization procedures should be intensified.

Our unparalleled opportunity to explore virgin sites of possible extraterrestrial life must not be compromised by shortsighted impatience that could result in the irreversible destruction of that opportunity. The reasons for this view have been adequately developed in previous reports by other groups. As biologists, we must reiterate the stand that has been taken. Reversal of it would in our opinion be scientifically irresponsible. Many wishfully persuasive arguments have been advanced that a relaxation of decontamination standards to a low level would not really entail serious risk of contamination of possible sites of extraterrestiral life or that such contamination, if it did take place, would not be serious. These arguments may be correct, but they are not based on certain enough knowledge. Adequate knowledge, however, can be forthcoming relatively soon if the first experiments are carefully planned with this question in mind. As new information is gained, sober reappraisals of the risks should be made to reach a careful balance between proper caution and acceptable risk. A vigorous effort must be made to gain the necessary information, and an equally vigorous effort must be maintained to find improved methods of decontamination of space vehicles, that will offer a minimum interference with engineering operations. We feel that there is a need for rigorously derived sterility specifications which could become the basis for a realistic engineering approach to the over-all problem. We also feel that an estimate of the cost of the total sterilization program should be made available to facilitate realistic budgeting. (See Chapter Ten, Space Probe Sterilization.)

Since a NASA sterilization program will not achieve its objective of protecting Mars from biological contamination unless all Martian missions are carried out with adequate sterility precautions, this problem is international in scope.

FINDING: We endorse the suggestion that international cooperation be attempted in the area of space probe sterilization.

If feasible, an exchange of information about sterilization technologies must be effected. It is further suggested that even unilateral transmission of information would be efficacious.

VI. Back-Contamination

The introduction into the Earth's biosphere of destructive alien organisms could be a disaster of enormous significance to mankind. We can conceive of no more tragically ironic consequence of our search for extraterrestrial life.

Several members of the Summer Study, as well as many of our colleagues in the scientific community, feel great concern over the possible consequences of back-contamination. On the other hand, some members consider the danger negligible.

Nearly all known pathogens have evolved in association with their hosts so extraterrestrial organisms are not likely to be pathogenic. Nevertheless, there are some exceptions which give rise to diseases of man -- viz., psittacosis, aspergillosis, botulism, and tetanus. Also, recent work demonstrating that ribonucleic acid of tobacco mosaic virus can direct specific protein synthesis using the protein synthesizing system of E. coli should caution us that the host specificity of viruses may not necessarily be more pronounced than that of the pathogenic protista. Moreover, in a different environment adaptation may take a different turn -- illustrated by the observation that the virulence of many microorganisms for a particular host can be greatly increased by successive passages through this host. Therefore, it seems conceivable that originally harmless forms of extraterrestrial life may on the Earth acquire pathogenic characteristics. Such potentiality will be hard to judge when the organisms are first encountered.

In any case mankind is far from helpless against introduced pathogens. By anticipating danger, we can minimize it.

RECOMMENDATION: To reduce the danger of back-contamination, appropriate quarantine and other procedures should be employed when handling returned samples, spacecraft, and astronauts. NASA should do all in its power to make the risk as small as possible.

Surely many unmanned one-way missions to Mars will be carried out before a round trip becomes feasible. Much will be learned from these early explorations and we are confident that this will include information on the possible hazard of back-contamination. Thus we shall acquire solid knowledge to replace guesswork about whatever danger may be involved.

VII. Man into Space

Several specific scientific aspects of the NASA manned space flight program were considered. We recognized at once that the present effort has been thoroughly application-oriented. As such, only short-term goals have been given priority. We believe that this policy, which appears to have been carried to an extreme, will in the long run delay the manned space flight effort in contrast to the rate of progress if there were more emphasis on long-term goals.

Man's role in space is to implement his scientific quest. The more rewarding aspects of this quest will relate to missions involving more than a few hours of orbital experience. We have yet to develop the bioengineering technology which will be required to provide the astronaut with a compatible environment needed for longer missions.

Man, however, is an animal as well. Furthermore he is the only animal whose purely psychological state is accessible to human observation. The weightless state offers an unparalleled opportunity for definitive studies of the gravity sensors and of the angular acceleration sensors; many valuable experiments on the psychophysics and on the perception of acceleration can be performed when a manned laboratory vehicle becomes available. There also will be an unprecedented opportunity to explore some basic relationships in the area of visual space perception and in the interrelationships between acceleration and visual systems.

With regard to man as a scientist and crew member, it is not enough to be concerned purely with mechanical effects of acceleration or lack of acceleration. It is important for an assessment to be made of man's capacity to be the scientist whom we wish to send into space, to engage in many studies early in the manned space flight program, relating to maintenance of function in space. The roster of relevant and important topics is long and we cannot do much more than list some of the perhaps more important ones. Studies are required: on the cardiovascular systems — the dynamics of circulation, adaptive mechanisms, cardiac musculature; on metabolism – basal metabolic functions, changes in ionic balance, changes in skeletal structure, renal functions; on the functioning of the central nervous system – central control of respiration, sleep-wake studies, intellectual functions. Of great importance will be the complete psychophysical and perceptual assay after sustained weightlessness. Other areas of partial or complete ignorance include: the kind of radiation protection needed, the consequences of psychological isolation, and, for the missions longer than a few hours, the kind of regenerative life-support systems which will serve best.

> FINDING and RECOMMENDATION: The Gemini and Apollo programs are being used to best advantage in obtaining needed physiological and psychological information on man. This situation should be corrected by more coordination within NASA at the divisional level.

From the long-range point of view, the extended manned missions of the future almost certainly will have to employ regenerative types of life-support systems. Purely storage-type systems would surely place exorbitant demands on the space and weight capacity which can reasonably be anticipated in future generations of spacecraft. Recycling of water must be accomplished and at least partially regenerative O_2 and CO_2 exchangers need to be perfected. Work in this general area of regenerative systems including bioregenerative methods based on photosynthesis should be encouraged. Efforts in this area are not well coordinated and appear not to be progressing as rapidly as desired. Although the operational need for a so-called closed (or nearly closed) ecological system lies well into the future, this development is needed and must be done far in advance. Long lead times necessarily are related to the development and testing for reliability of equipment which will have to operate continuously for many months or years. It appears essential to mention here that life-support systems will be required not only for travel in space vehicles, but for the support of man in his occupation of other planets. Both types of systems should be developed simultaneously.

> RECOMMENDATION: NASA should support work on regenerative life-support systems including bioregenerative systems based on photosynthesis.

VIII. Space Research in Environmental Biology

A. Gravity

For experiments lasting more than a fraction of a minute only the space environment can make available to the biologist low gravitational fields. Our knowledge of the sensing mechanisms by which animals and plants detect and respond to gravitational stimuli is poorly understood in many cases. The availability of a low-g facility should provide impetus to current work on gravitational responses.

> RECOMMENDATION: NASA should support studies in ground bases and in orbiting laboratories on the biological effects of gravity fields both above and below normal. This should be considered a major responsibility of NASA in the area of environmental biology for which the space environment offers unique experimental opportunities.

The number of species and kinds of problems in this general area which can be approached experimentally seems to us very large. Initial studies in this area of space biology necessarily will be exploratory in nature. Quantitative information must be sought in some cases to test hypotheses and in other cases to collect performance data on which new theory will depend. Studies may be divided arbitrarily into those which deal with sensing aspects and those which are concerned with morphogenic aspects of low-g stimuli. Under the former category we noted the following:

(a) Sensory deprivation in animals of the following inputs needs to be examined: vestibular and statocyst, kinesthetic, cardiovascular, and microcirculatory.

(b) For circulatory categories, fluid distribution, particularly in venous return in mammals, will be altered in the low-g environment; quantitative descriptions of the alterations are needed.

(c) Extensive experimentation has been carried out on fish, mammals, and crustaceans which have been deprived of specific equilibrium receptors. However, it has not been possible to remove surgically all gravitational input. Comparable studies in the weightless state are needed for comparison.

(d) Long-term exposures to low-g to zero-g conditions probably will induce behavioral changes which are qualitatively different from those resulting from short-term exposures during which significant adaptations do not occur. Such changes will prove to have both theoretical and practical interest.

(e) In view of the reported balance disturbances in astronauts, experiments should be designed to examine the effects of low-g specifically on the central nervous system of selected experimental animals, including man.

(f) In general, the study of statocyst function and development as affected by g-field appear well suited to satellite experiments. Ranging from the "labyrinth" inner ear of the mammal or the statolith of invertebrates to the unknown sensor of the plant, a variety of potentially informative experiments could be designed to explore statocyst responses in very different plants and animals.

Some of the most significant studies on environmental biology could be carried out in the area of morphogenesis. It should be emphasized that terrestrial organisms have evolved in a normal g-field. Gravity does influence morphogenesis in a determining way. We do not know the mechanism of this influence and cannot predict with confidence what ontogenetic patterns should be expected to result in a zero-g environment.

A case can be made for the thesis that in some organisms the physiological and morphological polarization, which is a prerequisite for cyto- and histogenesis, is modifiable and hence definable by gravity. One inferrable consequence of development under the condition of weightlessness would be morphologic alterations ranging from minor departures from normal anatomical structure to the extreme condition of acellular accretion. Therefore, experiments should be carried out on undifferentiated, totipotent, embryonic tissues developing in supportive media in the weightless condition. Upon recovery of such material from orbit, a search should be made for alterations in form and structure at gross, histological, cytological, and macromolecular levels of complexity. In addition, physiological and biochemical aberrations would warrant investigation. We are confident that some important changes will occur. We have not the slightest doubt that these will be scientifically interesting and profitable to investigate, but we simply have not the theoretical basis for making quantitative predictions about what we expect to happen.

We take special note of the probability that important questions in morphogenesis might be suggested or delimited by the observed characteristics of biological material developed in the weightless state but brought into the normal g-field on earth to observe subsequent development. We believe that initial experiments should be of a design which would permit multiplicity of observations at various levels of organization to identify quantitative or qualitative differences from the norm. It was suggested that a study of leaf mesophyll development would be highly appropriate here by virtue of: an appreciable background knowledge of its structure and gravity-sensitive behavior, its ready culture, and its adaptability to in-flight experiments. The egg cell, fertilized in the weightless state, would be another example of material which also should be considered in the context of recoverable packages.

In addition to and closely concerned with the design of experiments in low-g environments there are necessary studies in Earth laboratories which can contribute immeasurably to the meaning of data from satellite experiments. Viewing the g-field as a continuous variable which the centrifuge can extend in the one direction as the satellite can in the other, we recognize the need for information from centrifuge experiments which will apply to the same kind of experimental material which we shall send or shall have sent into orbit. Probably the most interesting changes to be anticipated will relate to the lowest regions of the g-variable but there is no reason to predict that any gravity-sensitive biological property will be a discontinuous function of g. Particularly in studies of g-adaptation the need is obvious for an extended range of the variable both above and below normal.

Some of the most valuable studies on gravity-dependent responses of higher plants have been carried out using clinostats. Further ground-based work on these and similar gravity-compensating devices ought to be encouraged. Results from this kind of work will be helpful for more easily designing scientifically useful flight experiments. In general plant material offers many advantages for study of the gravity sensor, the physiological sequelae of sensor activation, the determination of quantitative thresholds, and the exploration of the stimulus-intensity x presentation-time relationship.

B. Altered Periodism and Phenomena of Biological Time

One limit to Earth-bound organisms, including man in space, may be the capacity of the biological clocks to adapt to drastically altered periodism. On Earth the clocks are normally set by photic stimuli but, once set, they maintain their periodicity which can be little altered. There is some question as to whether in the absence of photic triggers, exotic environmental factors such as daily variation in magnetic and electrostatic fields might be used to set the biological clocks. This is an important question which should be investigated by observation of rhythms in organisms in space in (a) polar and equatorial low orbits, (b) orbits less than, equal to, and greater than 22,000 miles. These critical tests in space need not involve recovery. They should be prefaced by observations in controlled environments on Earth.

> RECOMMENDATION: NASA should support studies of biological rhythms in plants and animals including man as part of its effort in environmental biology.

C. Radiation

1. Measurements. The principal need in this area has been the thorough characterization of space radiation by appropriate physical measurements. This characterization must include not only the energy distribution and flux of the various particles and other radiations, but also the temporal and spatial changes during solar flares. Such knowledge is basic to all of the biological problems involving radiation in space. Recent measurements and data analysis have advanced knowledge in this area very rapidly.

2. Tissue-Equivalent Dosimeters. The use of tissue-equivalent dosimeters is advocated only as a stop-gap measure to secure a preliminary guide to problems of shielding. The technique does not provide the information ultimately needed.

3. Heavy Particle Experiments. We are unable to resolve completely a difference of opinion over the practicality of studying biological effects of heavy primaries (Z greater than 2) in the space environment. Available information on the flux of heavy particles suggests that the flux is generally too small to permit interpretable, reliable, biological experiments to be performed within feasible

flight times of recoverable capsules. If very long exposures are contemplated in order to increase the heavy primary dose, other kinds of radiation may impose severe experimental complications. However, we are dealing with an exploratory type of study wherein it may be naive to predict the uselessness of particular kinds of experiments which examine poorly understood phenomena.

On the other hand, there are certain kinds of effects which may be anticipated from short-term exposures of particular biological systems to space radiation. In such cases radiation experiments at ground level have furnished the basis for quantitative predictions of space radiation effects. Such predictions, of course, are worth checking. Where thorough control studies have been carried out a confirmatory space experiment could easily be defended, although it ordinarily would not be considered a top-priority experiment.

In this connection we note that, from time to time, biological materials of diverse kinds have been proposed for balloon or space-flight experiments without due regard to the radiosensitivity of such materials. The most familiar experimental organisms are not necessarily the ones best suited for confirming quantitative expectations of the effects of space radiations. Well-studied plant systems are available which have high sensitivity (down to ca. one rad) and for which somatic mutations can be scored relatively easily. The advantages of such systems seem not to have been widely appreciated.

We are unable to resolve a basic disagreement on the priority which should be attached to exploratory studies of biological effects of space radiation. A substantial majority felt that balloon-based studies of heavy primaries with available technology are not likely to be useful and that laboratory simulation of the radiation, while imperfect, will nevertheless be capable of yielding whatever information is needed.

As a practical matter we face the urgent problem of astronaut safety. Simulation of heavy-particle radiation by facilities such as HILAC (high-energy linear accelerator) and other particle beam generators can be very useful even though not all particle energies can be produced artificially. To investigate the biological effects of particles one cannot generate in accelerators, we suggest that studies with microbeams of high-energy protons, deuterons, and alpha particles will be at least as deleterious to the test organisms as the damage resulting from penetration of heavy primaries in the same location. These studies should be done both with single-cell systems and with multi-cellular organisms, where possible, to compare these effects with those of heavy primaries which currently available machines will produce. Thus it should be possible to set an upper limit for galactic cosmic-ray damage to, say, the brain of an experimental animal by knowledge of the flux to be simulated and by the topologically controlled production of extreme damage along high-energy microbeam tracks.

At least for small organisms, simulation by means of available facilities such as HILAC and other particle beams was regarded as a useful approach, especially when combined with theoretical studies on the role of linear energy transfer (LET) in biological radiation effects.

4. Chemical Protection. We considered chemical protection against radiation an area which apparently does not now warrant research effort by NASA.

Known protective agents are much less effective against high LET (linear energy transfer) radiations such as occur in space than against low LET radiations. As a practical matter we conclude that what the field needs is more ideas and some new approaches; otherwise this topic does not deserve high priority.

FINDING: Chemical protection against radiation is an area which does not now warrant research effort by NASA.

D. Exotic Environmental Stimuli

Various kinds of fields will be encountered in the space environment. Effects of electric and magnetic fields, both constant and varying, should be considered and, where necessary, experimental programs initiated to measure the effects and to assess their possible importance to a wide variety of forms of life including man.

E. Combined Effects of Stimuli

Synergistic effects are familiar in biology. There is ample precedent for the intuitive expectation that combinations of stimuli will affect biological systems in space in ways which could not be predicted from separate studies of individual variables. Thus, properly designed experiments should be conducted to explore the effects of different environmental factors when they impinge simultaneously on test organisms. Each such experiment must be planned with great care to insure that a proper set of controls is included. A "shotgun" study in which a host of possibly important factors vary simultaneously usually will yield no definitive information. Close attention must be paid to the statistical validity of experimental design. Neglect of these elementary precautions will seriously discredit the NASA effort in environmental biology in space.

RECOMMENDATION: Properly designed experiments should be conducted to explore the effects of different environmental factors when these impinge simultaneously on test organisms. Each such experiment should be planned with care to insure that a proper set of controls is included.

IX. Communication and Education

There are a number of conventional devices for improving communication in space biology.

(a) International participation of biologists in the planning for and use of biosatellites and probes.

(b) Symposia, preferably of international scope, on topics of space biology. A subject on which international discussion would be timely is that of extraterrestrial life and the sterilization of space probes.

(c) Fellowships for research and training in space biology. The modest program now underway is a good start.

(d) A space biology journal. If such a journal is to be published its sponsorship could well be international. Some members of the Summer Study did not favor the creation of a new publication in this area, especially not at this early date, because a space-biology journal may suffer from a tendency to report only applied studies and, begun too early, such a journal could acquire a non-scientific reputation which would reflect on the field as a whole. Preferably, articles relating to space biology will appear in existing professional journals and, when the volume of such publication warrants it, a space-biology journal would receive widespread support. However the disagreement on the need for a new journal specifically for space biology remains and no recommendation is made.

> FINDING: Communications between nations, between scientists, between NASA and other federal agencies, and within NASA itself all present problems which are becoming increasingly complex. This is not one problem, but several, warranting continuing study at appropriate levels.

X. Biotechnology and Instrumentation

Biology in the space age has at hand a gamut of special instruments and techniques which are orders of magnitude more complex and have enormously greater potential than what was available before. Sometimes it is claimed (perhaps facetiously) that experimental biology has disappeared and has been replaced by biophysics, biochemistry, molecular biology, etc. Such claims may be scientifically misleading because experimental biology is a science based on problems, not on gadgets or methods. It is the problems which change with an evolving science of biology. Nevertheless operationally the exploitation of so many powerful techniques, which is responsible for advancing our science so rapidly, clearly has made us devote much time and effort acquiring competence in what we may broadly refer to as biotechnology or bioinstrumentation -- the techniques of biological experimentation.

This increasing emphasis on biotechnology to which the experimentalist contributed and for which he must prepare his students and trainees has become a part of all biology, not just space biology. It is appropriate for NASA to recognize training in biotechnology as an area in which it has a direct responsibility for support and from which the space program may expect to reap indirect but nevertheless certain rewards. Devices to support biotechnology training as well as advances in biotechnology itself are many and varied.

> RECOMMENDATION: Projects of high quality in the area of biotechnology should be supported by NASA even though their immediate relevance to space experiments is only indirect.

XI. Theoretical Biology

Biology is and always has been a largely empirical science. One hundred and fifty years ago the same could have been said of chemistry. Today chemistry remains chiefly experimental, but there has grown up a substantial core of theoretical chemistry which guides the conduct of experiments and which has established the advance of theory as the chief goal for new fact collecting. Biology of course has followed the same pattern, but this science is so very much more complex -- one may say more difficult -- than chemistry that it has not come as far down the same road. Even today theoretical biology is scarcely identified as a discipline in its own right.

We are confident that major advances in biological theory are in store. It is the interplay of theory, observation, and experiment which makes a science progress. However, theory takes time to evolve. It requires the efforts of individuals some of whom are not, by temperament or for other reasons, experimentalists. Those who are most likely to contribute theoretical achievements are not so readily identified as are experimentalists with common objectives or special interests. Biological theoreticians, if they can be called such, often are separated from each other, which delays progress. Furthermore a great body of physical theory already exists, much of which surely applies to biology; this provides opportunities to hasten the development of biological theory. We have seen numerous examples of the application of physical theory to biology in specific areas (genetics, radiation biology, communication theory, kinetics, and others).

There was a division of opinion among Summer Study members. Some argued that theory always accompanies experiment and therefore needs no special emphasis. Others insisted that the very lack of emphasis has inhibited the development of a substantial theoretical biology. These differences in viewpoint are not necessarily unhealthy, nor are they likely to disappear as long as we have biologists who are by preference theoreticians and others who live chiefly as experimentalists sitting on the same committees. Accordingly our recommendation is not unanimous on this matter. Nevertheless, it was the feeling of the majority that NASA should make special attempts to exploit the opportunity for further theoretical advances in biology.

Three approaches were recommended:

(a) NASA should consciously encourage theoretical approaches to biological problems. This would amount to increased emphasis on theoretical work as a criterion in evaluating proposals.

(b) Conferences, symposia, and workshops should be set up to redefine problems and in other ways fulfill the requirements for scientific communication. Such meetings would be different only in that they would be almost entirely theory oriented. NASA might share sponsorship with other agencies.

(c) In some instances NASA should consider establishing a continuing theoretical group at a university or possibly at a NASA center like Ames. Such a group could address itself to the problem of identifying opportunities for theoretical developments in biology and for carrying out such work. Under proper leadership the activities of such a group -- in fact, its very existence -- would do much to stimulate interest among biologists and theoreticians of many kinds in the theoretical opportunities which exist in modern biology.

The relative cost of increasing the level of financial support for theoretical work would not be great and valuable developments could be shortened by several years. Such returns would be important to bioscience in general and could have crucial importance for the space program. Since the space environment is one in which experiments will always be very costly, the development of a better theoretical guide to suggest areas for wise experiment and areas to avoid would be easily justified.

> RECOMMENDATION: NASA should encourage theoretical approaches to biological problems and should consider establishing a continuing theoretical group in space biology at a university or possibly at a center like Ames.

Appendix I

Principal participants in the Working Group on Biology included the following:

Allan H. Brown, Chairman
C. S. Pittendrigh, Vice Chairman
K. C. Atwood
V. Dethier
R. O. Erickson
H. Fernandez-Moran
S. W. Fox
R. Galambos
S. Gordon
H. K. Hartline
N. Horowitz
R. B. Livingston
N. Nelson
E. Oakberg
E. C. Pollard
C. L. Prosser
E. Six
A. H. Sparrow
G. Derbyshire, SSB Secretariat
R. Barry - Rapporteur

NASA Contributors

W. Haymaker, Ames
E. Konecci, HQ
H. Newell (with J. Clark, alternate), HQ
O. Reynolds, HQ
J. Soffen, JPL

Representatives of Other Government Agencies

J. Liverman, AEC

Chapter Ten

SPACE PROBE STERILIZATION*

Earlier deliberations have revealed that compelling scientific reasons exist for protecting certain celestial bodies from contamination by terrestrial organisms. In order to reach valid conclusions concerning a sound program of space probe sterilization, we have informed ourselves as far as possible about: the status of the NASA space probe sterilization program, the direct and indirect costs of that program, and the operational difficulties connected with applying sterilization treatments and with maintaining sterility. Since responsibility to put into effect the present sterilization program is assigned to the Jet Propulsion Laboratory (JPL), we have obtained much of our information on current practices, costs, difficulties, etc. from that source (see Appendix III).

A substantial background of activity by diverse individuals and responsible groups who have already considered the necessity for and problems of sterilization is summarized for reference in Appendix I.

I. Definitions of Sterility

To avoid misunderstanding the objectives of a sterilization program it must be realized that "complete" or "absolute" sterility is not an operational concept when applied to spacecraft. The same is true for the many industrial products which must be guaranteed or certified as sterile, yet the individual items cannot be tested for sterility in a nondestructive manner. In each instance certification is based on the manufacturer's adherence to a set of proven procedures which are expected to produce sterile products, with close control of manufacturing procedures and routine sterility tests on random samples. The probability that a particular product item, certified as sterile, actually is contaminated can be made very small but, generally speaking, it cannot be made zero. Thus, <u>certified sterility</u> is a meaningful operational concept. In order to apply the concept to a spacecraft we need to prescribe an appropriate set of procedures for achieving sterility. We need to test these procedures in terms of the effectiveness with which they reduce the burden of viable microorganisms. This information allows us to establish a standard and a procedure to attain it; thus a particular spacecraft can be certified as sterile in the sense that there is a certain low probability of viable contamination. That probability can be made as low as desired within the capabilities of whatever procedures for sterilization have been prescribed.

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*See Appendix VI for a list of participants in the Working Subgroup on Space Probe Sterilization.

II. Techniques for Sterilizing Spacecraft

Four treatments are available for the evaluation of their applicability to the problem: dry heat, ionizing radiation, bactericidal liquids or vapors, and filtration of liquids and gases.

A. Dry Heat

A wealth of scientific literature and industrial experience provides the framework for establishing and testing sterilization by means of heat. Much of this experience, however, pertains to moist thermal death. Very limited effort has been devoted to the study of dry thermal death. Even that limited effort, in most cases, has been applied to the elimination of pathogens or other bacteria which usually are not as difficult to destroy as are the most resistant spores which may contaminate a spacecraft. Also, there has been no practical reason to study the efficacy of dry heat sterilization at relatively low temperatures (below 160°C) for extended times. Thus, we have very scant information on the kinetics of dry thermal destruction of microorganisms at temperatures below 150° or 160° C.

Current technology for moist heat sterilization is based chiefly on the rather well established linear relationship between the logarithm of the number of surviving organisms and heating time for a homogeneous population of a particular strain of microorganism. The industrial microbiologist is accustomed to quantifying such a thermal destruction rate in terms of the time required for a reduction by a factor of 10^{-1} in a bacterial or spore population maintained at a particular temperature. Such decimal reduction times, referred to as "D" values, are sometimes derived from end-point survival determinations in cases where insufficient data are attainable to establish directly the time relationship of population survival under heat stress, known as the "survival" or "thermal death rate" curve. Such derived "D" values were obtained by Dr. C. W. Bruch in the Wilmot Castle Company Laboratories for a native garden-soil mixed-spore population, exposed to dry heat (Appendix II). Of all the common sporogenous bacteria, these are likely to be the most resistant to heat while contained in soil. Data were expressed in terms of thermal death times (TDT) for samples held at particular temperatures. From the same data, together with certain simplifying assumptions, "D" values and "TDT" curves (log "D" or "TDT" vs. heating temperature) were deduced. This has been the basis for the dry heat treatment regimen currently recommended by JPL for spacecraft sterilization, viz., 135°C for 24 hours in the case of planetary spacecraft.*

It is our opinion that, while the presently available data have served the purpose of setting a conservative estimate of the heat dose necessary for adequate sterility, further work is definitely needed to establish dry thermal death kinetics, which are more exacting to our sterilization objectives, and hence provide a more exacting reference to our future sterilization standard requirements. Further work is necessary to examine the various indices which would significantly influence the rate of death, and hence establish the final and most reliable reference kinetics; e.g., the index spore type(s), the index heating menstrum, the index contamination level, etc.

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*For lunar spacecraft the current requirement is 125°C for 24 hours.

Parameters which would undoubtedly considerably influence the final dry heat sterilization requirement are the mode and level of initial contamination load. Since dry heat sterilization is primarily aimed against embedded contamination, and since much can be done to minimize the level of such contamination, both by the supplier as well as in assembly, and since among such a minimum population only a fraction would be the most resistant spore type(s), it is felt that the actual thermal dose necessary for sterility may be less stringent than the treatment previously prescribed.

At present, the matter is important primarily because only in this way can we set an appropriate temperature tolerance for component testing. To be sure that a particular component will not be degraded by, say, 135°C, it is necessary to test it at a slightly higher temperature to ensure that the survivability of the component is not marginal at the sterilization temperature. Therefore the test procedure would subject all heat-sterilizable components to a temperature which is higher by several degrees than the minimum prescribed for certification of sterility. With some components there is at present very little latitude between the certified, safe, sterilizing temperature and a temperature at which degradation occurs.

B. Ionizing Radiation

Since some spacecraft components are degraded or destroyed by heating, the use of highly penetrating ionizing radiation has been considered as an alternative means of sterilization. Unfortunately those critical components which have been tested were degraded by sterilizing doses of radiation (10^6 - 10^7 rad) to approximately the same degree as by dry heat treatment. One possibly important exception was solid propellant, for which radiation sterilization may prove to be the method of choice. Even here the advantage is not certain and, aside from this possibility, current information suggests that radiation has no advantage over dry heat as a means of sterilizing spacecraft.

C. Chemical Sterilization

Surfaces accessible to a highly bactericidal vapor, such as ethylene oxide, can be decontaminated effectively in this way. Other chemical vapors have been investigated, but so far none has been demonstrated to be superior to ethylene oxide in all respects. Where only surface sterilization is required, the time needed for achieving sterility often is less than for an equivalent heat treatment. Further modifications of the currently practiced procedure (pressure, temperature, humidity, concentration) could reduce the sterilization time still further.

The spore populations used for evaluating the effectiveness of the present ethylene oxide treatment were pure cultures (Bacillus subtilis, var. niger) and were not as resistant to dry heat as were the native soil spore flora used for the study of thermal death kinetics. However, Bacillus subtilis is relatively resistant to ethylene oxide. Therefore the choice of test organisms was conservative in both treatments and was appropriate to the treatment being evaluated.

FINDING: Ethylene oxide sterilization of the spacecraft and its various components needs further investigation.

Where tightly fitting mating surfaces are involved, ethylene oxide may not penetrate, and appropriate liquid chemical sterilants may be applied at the time of assembly if heat treatment is not to be carried out thereafter.

A number of problems in the use of chemical sterilization remain to be solved. However, none of these seems to represent an insurmountable difficulty.

The use of a sterilant like ethylene oxide vapor has made possible a new class of procedures which has direct application to spacecraft sterilization technology. These methods may be referred to collectively as "glove box" procedures, which are a combination of asepsis-antisepsis techniques. They can be used for making repairs and calibration adjustments on an already sterilized capsule. In contrast to the apparently complicated and difficult task of employing such procedures on a large spacecraft, advances in this sort of technology over the past two decades have produced a series of relatively simple but effective techniques of great versatility. Especially in the handling of sterile animals these methods are now routine. Many of them are directly applicable to manipulations on sterile spacecraft. There can scarcely be any procedures which one must perform on a sterilized spacecraft which are more challenging than complex surgical operations on experimental germ-free mammals, which are now routinely performed behind the protection of a bacteriological barrier in a number of biological laboratories.

> RECOMMENDATION: Glove box procedures have been employed for certain subassembly items on Ranger spacecraft. We recommend continued study of these techniques to ensure that they will be exploited wherever they prove applicable.

D. Filtration

Bacteriological filters may be required to sterilize certain liquids or gases which must be loaded on the spacecraft, but which cannot be heated or sterilized by chemical means. Examples of such fluids which may require filtration are: compressed helium or nitrogen, liquid propellants, and enzymes or other liquid constituents of biological experiments. Existing filter media can probably be used for handling all of the possible fluids; it will be necessary, however, to design suitable filter assemblies for each kind of application which incorporate sufficient depth of the filtering media to assure sterilization.

III. Current JPL Sterilization Program

A summary of present technology and the viewpoints of cognizant personnel at JPL with regard to spacecraft sterilization is appended to this report (Appendix III). From a review of the JPL program certain findings emerged:

> FINDING: A good working relationship seems to have developed between cognizant biologists and spacecraft engineers. This is commendable, for the continued improvement of the program depends very much on maintenance of such rapport.

> FINDING: The principal objective accomplished by the lunar spacecraft sterilization program has been to develop capability and establish experience in the sterilization of spacecraft.

Commendable progress with sterilization techniques has been made, and we are optimistic about still further improvements. Work with the Ranger spacecraft, one of which impacted the far side of the Moon, illustrates what has already been accomplished in an operational mission. While the Ranger cannot be "certified sterile" in the technical sense defined above, nevertheless the sterilization procedures that were followed ensure that the spacecraft could not have carried many viable organisms.

Scientists and engineers are now in a better position to meet future sterilization requirements as a result of experience with the Ranger series. Problems are now better understood. Areas for future research and development effort have been identified. Planning operations are now much better able to take into account sterilization procedures.

FINDING: Because of the program schedule imposed on the lunar program by nonscientific considerations, time probably will not permit development and incorporation of certain advanced sterilization concepts. Waivers have been provided for critical components which could not be made sterile internally without serious degradation of their performance. Such waivers should continue to be issued where necessary. In general the lunar sterilization program should continue in force in order to keep contamination of the Moon low and thus to interfere as little as possible with future biological study and only secondarily in order to advance the state of the art of sterilization.

FINDING: At the present state of the art it is not possible to assign a quantitative measure of the degree of degradation in reliability of a spacecraft that results from sterilization procedures. Opinions differ on this point, but they are only opinions; to a large extent the matter probably will remain a topic for speculation.

FINDING: The JPL spacecraft sterilization program has resulted in substantial technological advances. Problem areas have been identified and in many ways the success of future efforts will depend on what has been learned already. Critical components have been identified, and in some cases intensified efforts toward redesign have produced components which can withstand specified sterilization treatments, thus removing such components from the critical list. A tabulation of some of the components known to present problems when heat-sterilized is given herewith:

Batteries
Solid propellants
Parachute
Recording tape
Bolometer star tracker (on Mariner bus)
Data encoder (on Mariner bus)
Vidicon tubes
Photomultiplier tubes
Biological experiments
Radiation experiments
Pyrotechnics
Gyroscope (on Mariner bus)

FINDING and RECOMMENDATION: As detailed plans materialize for missions to Mars it is likely that one serious impediment to the maintenance of sterility can be avoided, namely the last-minute adjustments before launch, which are often required but, if performed, require that the spacecraft be resterilized. With long-duration missions the spacecraft will be expected to operate for many months without maintenance. Current space capsules are so designed that so-called last-minute adjustments will have been made days or weeks before launch, and it should then be neither necessary nor desirable to open the sterile capsule for further tinkering. Distinct engineering advantages will accrue from avoiding servicing operations on the capsule just prior to launch. It was recommended that the concept be incorporated into NASA thinking and planning for missions to Mars.

FINDING: Aside from the presence of viable organisms which might contaminate an extraterrestrial body, the introduction of nonliving organic compounds might constitute an unwelcome complication for cosmobiological research, even on the Moon or a planet that is devoid of indigenous life. This complication can be materially lessened if a complete inventory of organic constituents of each spacecraft is maintained. In keeping with the directive of 13 October 1959 (Appendix IV) such inventories should be recorded in a form for convenient reference.

FINDING: It is difficult to estimate the direct cost of the lunar sterilization effort to date. It has been estimated at some 5 or 10% of the cost per lunar mission. Regardless of the figure which is selected, when one takes into account the transfer of technological advance to missions to Mars and later missions to provide a reasonable basis for expressing direct costs, the expense of the sterilization program must amount to a very small fraction of the eventual overall cost per mission.

FINDING: In general an intensification of effort seems called for in order to achieve the goal of certified sterility of space-craft for missions to Mars and to more distant space objectives.

IV. Relation of Sterilization Program to Manned Landings

For any particular space objective of biological interest the need for spacecraft sterilization will be reappraised as early missions provide pertinent information. In some cases, if not in all, the need for maintaining sterility or for keeping contamination localized and as low as possible will continue, even after this is made more difficult by manned exploration. There is scant evidence that serious attention is being paid to this requirement in plans now being formulated for accomplishing manned landings on the Moon.

FINDING: The mere suggestion that fecal material might be jettisoned under conditions which could contaminate the lunar surface, perhaps even before the first collection of lunar material, is symptomatic of the attitudes which fail to give appropriate consideration to exobiological objectives. Such irresponsible procedures are clearly contra-indicated.

RECOMMENDATION: Planning for manned landings on the Moon and planets must be based on the assumption that sterility precautions will still be required during the phase of manned exploration.

RECOMMENDATION: Work should be intensified on the development of space suits which will function as more effective biological barriers than those presently available.

V. Urgency of Biological Experiments

The ultimate goal of the NASA spacecraft sterilization program is that the biologically significant information about the Moon and the planets shall not be compromised or destroyed. The exobiological organisms and the biologically pertinent organic matter on those bodies may be subject to irrevocable change through contamination introduced by nonsterile space probes. The physics and the geology, on the other hand, are not subject to change.

RECOMMENDATION: Therefore biological and chemical experiments should be carried out with first probes, while pollution from the Earth is still minimal.

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Since the nature of our subsequent studies of the Moon and the planets will depend largely on the results of early observations, it is impossible to decide in advance of the first landing all the things we may wish to do later on. It is even likely that we shall think of things to do with uncontaminated lunar samples years after the first landing which will have provided the opportunity to collect the samples.

RECOMMENDATION: Therefore we recommend that, among the samples of Moon stuff which are first collected, some be sealed off under sterile conditions and retained as minimally disturbed and uncontaminated samples of lunar material for future use, as new information may dictate.

Unless this is done at the earliest opportunity, the gradually increasing contamination of the Moon by the arrival of incompletely sterilized spacecraft might make it impossible to certify the sterility of a sample collected later.

RECOMMENDATION: It is recommended that the collection of samples be introduced into the programs for unmanned missions in view of the relatively greater risk of contamination during manned flights to the Moon and planets. The uncontaminated sample need not necessarily be returned promptly to Earth, but may be left at the collection site to be picked up and returned even many years later.

VI. International Cooperation

FINDING: The problem of maintaining extraterrestrial bodies uncontaminated by terrestrial organisms is international in scope.

It is essential that there be international agreement on the need for spacecraft sterilization or for keeping contamination to a feasible minimum for each specific space objective. Further than this, it seems important that information on sterilization methods be exchanged internationally. From a scientific standpoint even unilateral exchange would be permissible and desirable, whether or not it could be considered politically feasible. Maximum effectiveness would be achieved if a truly cooperative operational effort in exobiology were established on an international basis.

VII. Policy Considerations

FINDINGS AND RECOMMENDATIONS concerning what we believe should be adopted as national policy with respect to individual space objectives (celestial bodies) are described below.

Moon. It is not certain whether the Moon harbors a viable indigenous biota. Whether it does or not, the organic compounds which may be present in localized regions are of great significance for cosmobiological theory. Lunar investigations, at least initially, ought to be directed toward a search for organic constituents as well as toward the discovery of extraterrestrial life.

Insofar as possible, lunar missions should avoid contaminating the lunar surface with terrestrial life forms. The lunar sterilization program has resulted in operational spacecraft which contain very few viable organisms. It remains highly desirable in future missions to the Moon to keep the contamination of spacecraft as low as possible with the use of present sterilization methods. These methods should continue to be employed except where it can be shown that the probability of mission failure would be significantly increased by the treatment of a particular component, in which case a waiver will be in order.

The lunar probe sterilization program, which formerly had as its principal objective the development of a sterilization capability, will continue to contribute experience and improved technology. However, its future goal will be chiefly that of protecting the lunar environment for cosmobiological objectives.

Attention should be focused on certain biological aspects of the lunar environment which are sometimes misinterpreted. The environment is often referred to as excessively hostile. While this is true for an organism like man, it is not true for most microorganisms. At some not very great distance below the lunar surface, low temperature, high vacuum, and protection from destructive radiation are provided. These are exactly the conditions which have been found maximally effective for preserving microorganisms. Whether or not conditions favoring proliferation of microorganisms obtain in specific regions cannot be determined without further knowledge of the Moon.

The acquisition of a representative series of samples of lunar material for biological and organic-chemical examination remains as the principal biological objective of lunar exploration. It is emphasized that on early missions to the Moon the highest priority should be given to the task of collecting biologically significant samples, simply because the inevitable introduction of contamination by successive landings may destroy the unique opportunity for carrying out such tasks at a later time.

Venus. Various kinds of information yield for Venus an internally consistent model for the atmospheric temperature profile with height above the surface. The consensus is that the surface of the planet is too hot for terrestrial organisms to survive, but that cooler temperatures prevail at high altitude. From the standpoint of temperature alone terrestrial life could survive only in the high cloud layer or at very high elevations if the surface is very mountainous (Appendix V). From a biological point of view it will be desirable to avoid contamination specifically of these high-altitude sites where terrestrial organisms, if introduced, might survive. Technically this does not appear difficult since an impacting spacecraft, if its surface is sterilized, would not be likely to introduce contaminants on passage through the cooler layers of the atmosphere of Venus. Therefore surface sterilization procedures are recommended.

The use of fly-bys which are unsterilized should be limited to trajectories which insure only a 10^{-2} chance of impacting. This relaxation of tolerance (from 10^{-3}) will be justified by the increased reliability of a mission which will not require a final retromaneuver. Such a maneuver would be necessary if the probability of impacting were to be made as low as 10^{-3}

Mars. This planet is at present our most important exobiological objective. As a matter of national policy it should be declared that the first several Mars missions will be principally, and if need be exclusively, for biologically significant research. In effect, Mars should become a biological preserve. That policy should remain in force until information returned from Mars shall permit a change in policy on biological grounds. If necessary, a sterilization program many times more ambitious than the present efforts may be required to provide the technological background and experience needed to certify the sterility of the first spacecraft we send to Mars. It was recommended that the Mariner entry capsule for Mars be sterilized by an officially certified treatment and be handled asceptically thereafter. The goal of these activities should be that there is less than a 10^{-4} probability that a single living organism will be released on the planet.

For Mars, either fly-by trajectories should be controlled to insure not over 10^{-4} probability of hitting the planet, or else sterilization should be carried out on the spacecraft.

Control of Sterilization. A Sterilization Control Group should be established as a unit within the organization responsible for planetary missions. This Group should be invested with the necessary authority to prescribe sterilization procedures and charged with the responsibility for the necessary supervision and inspection which must be considered an integral part of the sterilization program. The Sterilization Control Group should report directly to the Project Manager.

The existence of a Sterilization Control Group should in no way lessen the responsibility of cognizant engineers to attain sterility.

Specific responsibilities of the Sterilization Control Group should include the following: (i) Certifying of procedures necessary for fabrication and launching of sterile spacecraft; (ii) instruction and training of cognizant engineers in officially certified procedures to be used for sterilization and maintenance of sterility; (iii) continual monitoring and recording of sterilization procedures and maintenance of sterility; (iv) recommending of research and development work needed for insuring a successful sterilization program; (v) reporting progress and results of the sterilization program to Project Management and to NASA Headquarters.

Appendix I

August 1, 1962

Resume of Some Earlier Extraterrestrial Contamination Activities

G. A. Derbyshire, Space Science Board Secretariat

Origins

On February 8, 1958, the Council of the National Academy of Sciences, recognizing that,

> "The launching of IGY satellites has opened space to exploration; accordingly, attempts to reach the Moon and planets can be anticipated, with reasonable confidence, within the foreseeable future"

expressed its deep concern that initial operations might "compromise and make impossible forever after critical scientific experiments." Resolutions adopted by the Council urged: (i) "scientists to plan lunar and planetary studies with great care"; (ii) the International Council of Scientific Unions "to encourage and assist the evaluation of such contamination and the development of means for its prevention." Additionally, (since at that time U. S. IGY lunar probes were already scheduled), the Council (iii) agreed to "plan lunar or planetary experiments in which the Academy participates so as to prevent contamination of celestial objects in a way that would impair the unique... scientific opportunities."

Satellite Life-Sciences Symposium

The Academy early recognized the possibilities for biological research inherent in the space environment and the necessity for educating the life scientists to the possibilities and limitations of Earth satellites and space probes as new tools for its exploitation. Accordingly, in late 1957, the President of the Academy suggested the organization of a "Satellite-Life Sciences Symposium," and requested the Chairman of the Earth Satellite Panel of the USNC/IGY to act as the Chairman of the planning committee. The NAS was joined by the American Institute of Biological Sciences and the National Science Foundation in the sponsorship of the Symposium, held in Washington, May 14-17, 1958. Some 400 life scientists attended the sessions for which there were 60-odd speakers and discussants.

They first covered engineering indoctrination; and then discussed biology and the space environment from the cellular through the behavioral level. During the session on Basic Biology, Professor Joshua Lederberg presented a paper on lunar contamination dangers which subsequently appeared as "Moondust," Science, 127: 1473, 1958. The paper represented an extension of thoughts expressed by Lederberg in a memorandum widely circulated in the scientific community during January, 1958.

CETEX

The resolutions of the Council, NAS, were communicated to the ICSU Bureau at its March 3-5, 1958, meeting by Dr. Lloyd V. Berkner (then ICSU President). As a result, the Bureau formed the ad hoc Committee on Contamination by Extraterrestrial Exploration (CETEX).

This committee met on May 12-13, 1958, and presented its report recommending the adoption of a code of conduct aimed at achieving a compromise between an all-out program of lunar and planetary exploration on the one hand and the desire to provide absolute protection of these objects for future research on the other. One of the recommendations in that report proposed that ICSU request its national members to prepare detailed papers dealing with the topics raised; and that, before the end of 1958, these papers be available for a second meeting of CETEX when detailed recommendations would be prepared with the aid of advisory experts (see Science, 128:887, 1958).

The first CETEX Report was circulated with a request for comments in July, 1958. The Space Science Board strongly endorsed the intent of the report and its recommendations, having secured the support and co-operation of those federal agencies concerned with launching U. S. space probes. The recommendations of the Board and the assurances of the cooperation of U. S. launching authorities were provided to the Academy, and these formed the basis for U. S. (NAS) response to ICSU.

CETEX II

Following ICSU acceptance of the CETEX recommendation for the formulation of a code of conduct for space research (October, 1958) and concurrence with the request to hold a second meeting, CETEX II convened March 9-10, 1959. This meeting confined itself to the general formulation of the problem, and the review and slight modification of its initial report. CETEX held that the contamination problem was an integral part of the duties of COSPAR (see below) and so reported. For report of CETEX II, see Nature, pp. 925-928, April 4, 1959.

COSPAR

The ICSU Committee on Space Research (COSPAR) was established in October, 1958, to provide for continued and expanded cooperation in space science, which had been so successfully begun during IGY by the Comité Spécial de l'Année Géophysique Internationale (CSAGI). Previously, ICSU had requested the views of national members on the need for COSPAR, and had been advised by NAS of enthusiastic SSB support. Dr. W. Albert Noyes, Jr., Chairman of the SSB Committee on International Relations, was appointed U. S. delegate to COSPAR and attended the first meeting, November 14-15, 1958. Because of pressing commitments, Dr. Noyes requested that he be replaced as COSPAR delegate, and Dr. Richard W. Porter was, therefore, named. Dr. Porter currently serves as Vice-President of COSPAR.

In accordance with ICSU instructions, COSPAR assumed responsibility for the area of concern to CETEX and acknowledged the CETEX II report at its March 12-14, 1959, meeting. COSPAR asked (at the same time) the U. S. and U. S. S. R. to consider methods for avoiding contamination. The U. S. delegate to COSPAR communicated this request to the Space Science Board for action.

Space Science Board

The Space Science Board was formed by the President, NAS, in late June, 1958, to serve as the focal point for the interests and responsibilities of the Academy-Research Council in space science. At the formation of the Board, the Academy, in an enumeration of the Board's responsibilities, stated that:

> "Still another task before the Board is a program to gain the cooperation of the International Council of Scientific Unions and other international organizations in the prevention of undesirable and unnecessary contamination of Moon and planet surfaces and atmospheres with alien particles of energy and matter introduced from Earth by space vehicles."

In December, 1958, a group met under Board sponsorship to discuss the problems connected with the detection of extraterrestrial life and the prevention of contamination of celestial bodies with terrestrial organisms. Present at this seminar were representatives of the biological, astronomical, physical, and engineering sciences. This group (subsequently named EASTEX) circulated its proceedings to form a basis for further deliberations by interested groups. As a logical extension of this seminar Dr. Joshua Lederberg (also under Board sponsorship) formed a comparable West Coast group (WESTEX) which held meetings in February, March, May, and September 1959. This group selected as its immediate and most pressing problem the establishment of logical requirements for space probe sterilization from the biologists' viewpoint. Recognizing the absence of effective procedures for space probe sterilization, the Space Science Board, at its meeting of May 7-9, 1959, requested the establishment of a small ad hoc committee to consider methods and techniques for achieving this objective. Dr. Joshua Lederberg agreed to serve as chairman. This ad hoc committee found, in discussions held on July 8, 1959, that sterilization was feasible and it appeared that effective procedures could be evolved provided that necessary emphasis was given to the problem. The report of the ad hoc committee and its recommendations were adopted by the Board and were provided to the responsible federal agencies on September 15, 1959.

The first steps taken by NASA to put the recommended policies into effect were reported in a letter on October 13, 1959 from T. Keith Glennan, then Administrator of NASA. (See Appendix IV).

Appendix II

Dry Heat Sterilization: Its Development and Application to Components of Exobiological Space Probes

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The past and current emphasis in microbiological studies associated with space exploration has been with the aspects of manned space flight, i. e., the use and control of microorganisms in closed ecological systems. In a previous symposium of this society on space-age microbiology, Bejuki (1960) listed the prevention of biological contamination as one of the challenges of astrobiological or exobiological exploration. Although the subject of extraterrestrial contamination invites much discussion among astrobiologists, the research programs concerned with this problem are small and have not attracted much attention.

Since the first space satellite was launched four years ago, the question of the origin of life and the possibility of extraterrestrial life have been revived. As Lederberg (1960) has pointed out, the best approach to the study of extraterrestrial life involves microorganisms. Such studies, however, would be seriously jeopardized by contamination of celestial bodies with earth microorganisms deposited as a result of hard landings of space probes. The hazards of such biological contamination have been assessed by CETEX (Committee on Contamination by Extraterrestrial Exploration of the International Council of Scientific Unions), and certain covenants regarding space exploration have been formulated by this group. Thus, the sterilization of space probes is required by international agreements designed to prevent contamination of extraterrestrial sites by terrestrial microbes.

Several methods for obtaining sterile space probe components have been suggested including sterile assembly, built-in or self-sterilization, and terminal sterilization. Our research has investigated the use of dry heat for the terminal sterilization of electronic components before assembly of the space probe. At the time of launch the entire space probe is presently surface-sterilized with gaseous ethylene oxide.

A brief review of the literature on dry heat sterilization has revealed that there is a dearth of information on this subject, especially at temperatures below 150°C. While dry heat is used for the sterilization of hospital supplies, the sterilizing cycles thus employed have been based on the destruction of spores from pathogenic microorganisms. Perkins (1956) in an analysis of dry heat sterilization for hospital supplies has shown the serious inconsistencies in results obtained by past investigators for the dry heat resistance of a given bacterial species. Recently, Darmady, Hughes, and Jones (1958) recommended 45 min. at 160°C and 18 min. at 170°C for the dry heat sterilization of small hospital instruments. These values were based on the destruction of tetanus spores, which were more resistant to dry heat than spores of *Bacillus stearothermophilus*. Small samples of soil were more resistant to sterilization than the bacterial spore preparations, but they concluded that the instruments would have been damaged by sterilizing cycles based on the sterilization time for soil.

Nearly all of the fundamental work on mechanisms and kinetics of thermal death of microorganisms has been provided or supported by the food industry. Until 15 years ago moist heat was the chief sterilizing agent of this industry, but the advent of aseptic canning procedures in which cans and covers are sterilized by superheated steam or other hot gases have renewed interest in the study of microbial resistance to dry heat. Collier and Townsend (1956) reported that spores of B. stearothermophilus strain 1518 and Bacillus polymyxa have approximately the same resistance to superheated steam but in gases of low water content the spores of B. polymyxa were far more resistant than those of B. stearothermophilus. Their results were expressed in F_{350} values (min at 350 F to destroy the organism) and z values (slope of the thermal death time curve in degrees F): B. stearothermophilus strain 1518, $F_{350} = 0.708$, $z = 26$, and B. polymyxa, $F_{350} = 0.667$, and $z = 28$. Pflug (1960) has determined the resistance of spores of Bacillus subtilis strain 5230 to superheated steam and has reported a D_{350} (time in min for a reduction of one logarithm or 90% of the number of organisms at a temperature of 350 F) of 0.57 and a z value of 42. Although dry heat is usually considered as one entity, microorganisms are more resistant to destruction by hot gases of low water content than to superheated steam (Collier and Townsend, 1956). Until more data are available, hot air, hot inert gases, dry heat in a vacuum, and superheated steam should not be accepted as equivalent sources of dry heat sterilization.

The data from any research on sterilizing processes must be expressed in meaningful terms. It is not within the scope of this presentation to review either the derivation of the many terms associated with microbial resistance to sterilizing agents or to consider the mathematical concepts of process time calculations. The terminology and concepts that have been developed by the food industry for moist heat and radiation sterilization are applicable to data from studies on dry heat sterilization. In this discussion only D values (time to reduce a given microbial population 90% or one log in count), F values (time to sterilize a given microbial population at a given temperature), and z values (slope of the thermal death time curve expressed in degrees F to traverse one log cycle) will be used to report our results.

Dry heat D values may be calculated from partial survival data by the equation that Stumbo (1948) has applied to such data from moist heat studies:

$$D = \frac{t}{\log A - \log B}$$

where:

A is the total number of samples heated multiplied by the number of spores per sample;

B is calculated by assuming one surviving spore per sample when less than the total number of samples show survival, and

t is the exposure time at a given temperature that gives sterilization of some, but not of all, of the samples.

It is also possible to calculate D values from survivor curves that show a logarithmic order of death. The D value from a logarithmic survivor plot is the reciprocal of the k value obtained from the slope of the curve. The mathematics of these calculations have been discussed thoroughly by Schmidt (1957).

F values can be determined experimentally or can be derived from D values by the following equations of Schmidt (1957):

$$F = D\ (\text{Log } A \pm 2)$$

where A is the number of spores per replicate, or

$$F = D\ (\text{Log } M + 1)$$

where M is equal to the number of spores per replicate times the number of replicates.

The latter formula has been employed in our calculations.

In our first studies we investigated the relative resistance of dry spores of B. subtilis var. niger and B. stearothermophilus strain 1518 to moist and dry heat sterilization. Small samples (0.01 milliliter) of washed spore suspensions of known count were inoculated onto filter paper strips and air-dried. The strips were placed in 150 x 16 mm screw-cap tubes, which were heated in test-tube ports in a cylindrical aluminum block with a covering lid. After the heating period the spore strips were placed in trypticase soy broth and incubated at 32°C for B. subtilis var. niger, and at 55°C for B. stearothermophilus strain 1518. Thermal death times (F values) were determined at intervals of 10°C in the temperature range of 120-180°C. The thermal death time curves that were plotted from these data are shown in Figure 1.

A direct comparison of the F values from these curves is not possible because the concentration of spores for B. subtilis var. niger was 1×10^6 per strip, while the spore concentration for B. stearothermophilus strain 1518 was 5×10^5 per strip. D values, which are independent of initial spore concentration, were calculated and showed that B. subtilis var. niger was 3.5 to 5 times more resistant to dry heat than B. stearothermophilus strain 1518. In a correlated study with moist heat the spore strips for B. stearothermophilus strain 1518 required 25 min at 121°C for sterilization, whereas the spore strips of B. subtilis var. niger were sterilized in less than 5 min at 110°C. The spores of B. stearothermophilus strain 1518 are far more resistant than the spores of B. subtilis var. niger to moist heat sterilization, but the spores of B. subtilis var. niger are 3 to 5 times more resistant than the spores of B. stearothermophilus strain 1518 to dry heat sterilization.

Our next studies with dry heat commenced with the receipt of a contract from the National Aeronautic and Space Administration (NASA) for the sterilization of electronic components of exobiological space probes. When this contract was received, NASA was recommending a dry heat cycle of 125°C for 24 hrs. for the sterilization of electronic components. The time of 24 hrs. had been derived from an extrapolation of published data at higher temperatures. The temperature of 125°C had been selected because of the known sensitivity of certain electronic components to higher temperatures. Our first efforts were designed to confirm the sterilization in our cylindrical aluminum blocks of small soil samples and spores of known bacterial species held on several carriers at this time-temperature regimen.

Six soils that had been dried at 50-55°C for one week were screened for their resistance to dry heat sterilization at 120°C. Times of sterilization from 35 to 60 hrs. were obtained. The two most resistant soils, FG and CO, were selected for sterilization studies in the temperature range from 120-180°C.

Table 1 lists the times for sterilization at several temperatures for 0.1 g samples of these soils. If one plots the D values (log scale) against temperature

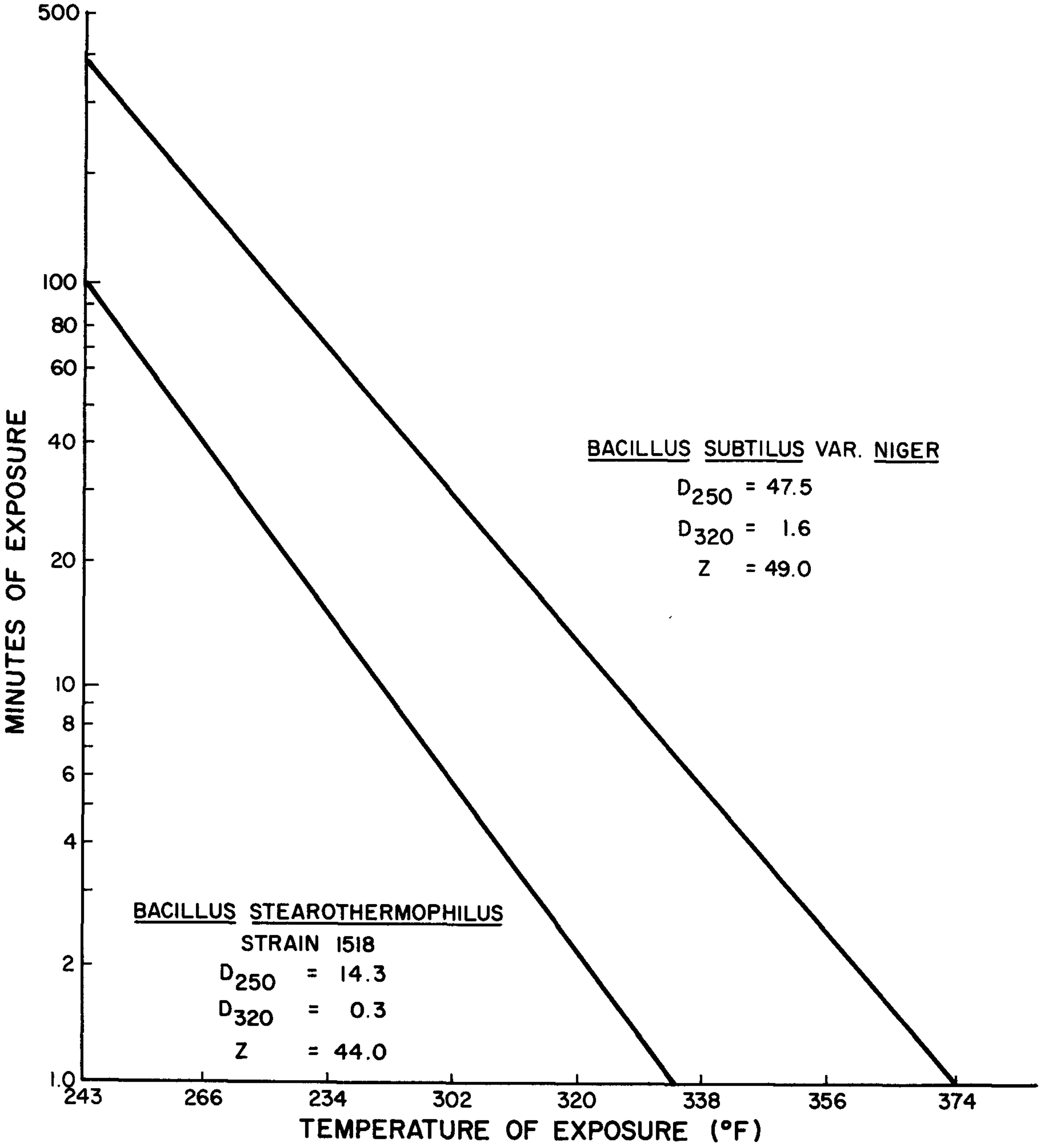

FIGURE 1

Resistance of Dry Spores from Two Bacterial Species to Destruction by Dry Heat (linear scale) for both of these soils on the same sheet of semi-log paper, a fairly linear plot can be obtained with a z value of approximately 46. Only the results at 120°C are askew. These data show that a cycle of 125°C for 24 hrs. could sterilize soil, a natural material considered difficult to sterilize.

In our research with various bacterial sporeformers, we attempted to simulate the conditions of microorganisms in the soil samples. Since there were no data on the effect of physical carriers on microbial resistance to dry heat sterilization, four carriers were included in our studies. Equivalent samples of washed spore suspensions of known count were inoculated onto filter paper strips and air-dried, or were inoculated into glass tubes or onto 2 g quantities of sterile sand or 0. 1 g quantities of vermiculite and dried in a desiccator at 50-55°C for at least 2 days.

The washed spore suspensions of all aerobic sporeformers except B. subtilis var. niger, B. stearothermophilus strain 1518, and Bacillus coagulans were obtained from growth on nutrient agar with one ppm Mn. B. subtilis var. niger was sporulated in a casein acid digest broth (Roth, Lively, and Hodge, 1956), B. stearothermophilus strain 1518 on nutrient agar with 10 ppm Mn, and B. coagulans on the agar medium of Rice and Pederson (1954). Spores of the clostridial species were obtained from 10% trypticase broth (Brown, Ordal, and Halvorson, 1957) or modifications of that medium.

The data for the dry heat resistance of several mesophilic sporeformers on the various carriers are given in Table 2. An analysis of these D values by organisms shows that B. subtilis var. niger, B. coagulans, and soil isolate-69C are the most resistant members of this test series. It should be mentioned that the spore harvest of B. subtilis 5230 that had high heat resistance was obtained from growth on nutrient agar with 10 ppm Mn and 0. 05% glucose as compared with the growth on nutrient agar with one ppm Mn. The unidentified sporeformers isolated from heat treated samples of soil that had high resistance to dry heat sterilization possessed heat resistance comparable to that found for known sporeformers.

An analysis of Table 2 by carriers shows that the spore samples on sand were always more resistant than the spore samples on paper or glass. Although the vermiculite samples are not listed in Table 2, they tended to be more resistant to sterilization than the sand samples, but the variation in sterilization time for the vermiculite samples made D value calculations difficult. The inclusion of thermocouples in the samples showed an increased lag of only a few min for the sand and vermiculite samples. Some unknown physical factor or factors are responsible for the difference in heat resistance of the spores on the various carriers.

The dry heat resistance of several clostridia and B. stearothermophilus strain 1518 on the various carriers is given in Table 3. What is immediately striking is that this group of sporeformers has 1/3 to 1/4 of the dry heat resistance of the mesophilic aerobic sporeformers described in Table 2. The increased resistance of the spore samples on sand as compared to the samples on paper and glass is similar to the results noted in Table 2.

Comparisons were made of the calculated F values derived from the D values with the experimentally found sterilization endpoints. The agreement for spore samples on paper strips was good, but the soil samples and spore samples on glass or sand occasionally had sterilization endpoints smaller than the calculated F value.

An examination of the chemical and physical conditions that might be present during the dry heat sterilization of electronic components showed that the effect of vacuum, inert gases, and entrapment of spores within solids on sterilization by dry heat should be investigated. Spores of the various organisms on the four carriers were subjected to dry heat sterilization in a vacuum oven and in evacuated tubes held in the heated aluminum blocks. When enough data had accumulated so that a comparison with the sterilization times in air could be made, a definite trend of decreased sterilization times was noted for all samples heated in a vacuum. The data in Table 4 are indicative of this effect.

The vacuum that has been employed in our work is low (10^{-1} to 10^{-2} mm Hg). Even with this limitation the increased resistance of the spore samples on sand noted in Tables 2 and 3 is decreased to levels of resistance found for spores on paper strips sterilized in air. Although a shorter time for sterilization in the presence of a low vacuum than in air has been found for all of the species of bacterial spores tested on paper strips, this effect is not exhibited by all spore crops for a given organism.

Davis, Silverman, Goldblith, and Keller (1962) have studied the destruction of microbial spores at high vacuums (10^{-8} to 10^{-9} mm Hg) and temperatures from -110°C to 88°C. At the higher temperatures they found a definite decrease in the thermal resistance of microbial spores in the presence of a high vacuum as compared to their thermal resistance at atmospheric pressure. Clearly, the presence or lack of a gaseous environment surrounding microbial spores during dry heat sterilization influences the rate of spore destruction. The composition and stability of the gaseous atmosphere may also be important and may be the factor responsible for the high dry heat resistance of spore samples on sand and the soil samples.

The entrapment of spores within solids provides an analogous situation in which air is excluded from the spore environment during dry heat sterilization. Spores of B. subtilis var. niger have been entrapped in plaster of Paris, asbestos patching cement, solid rocket propellant, and various dental plastics. The times for sterilization and approximate D values are listed in Table 5 for cylindrical pieces of the solids that were exposed in test tubes in the heated aluminum blocks. After the heat treatment the samples were ground as finely as possible and added to tubes of trypticase soy broth. Adequate control tests were made to insure the absence of extraneous contamination during assay procedures and lack of toxicity from the solids added to the sterility test medium.

Some of the D values in Table 5 are the highest yet found for any known organism in or on any known carrier. The variation in D values indicates that the ingredients of solids giving low D values must react with the spores during the heat cycle. Since the lag in heat penetration is approximately the same for all of these solid materials, the increased D values found for the spores in most of the solids could result from the presence of a stable physical-chemical environment around the spore. This condition would be in marked contrast to the condition of a spore in a vacuum and may explain in part why a heat treatment in vacuum is more deleterious than a heat treatment in air. Studies with hot inert gases should provide additional information to interpret the effect of gaseous composition and the pressure differential of an atmosphere on dry heat sterilization. All of these factors must be understood so that effective process cycles can be devised for the dry heat sterilization of space probe components.

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TABLE 1. Thermal death times and D values (in hours) for the mesophilic bacterial spore population of two soils

Temperature °C	Soil FG		Soil CO	
	Time to sterilize	D value*	Time to sterilize	D value*
120	60	8.9	60	8.7
125	28	4.0	24	4.1
130	18	2.6	15	2.4
140	8	1.1	8	1.1
150	3	0.41	3	0.39
160	1	0.13	1.5	0.19

* The D values for each soil were calculated from a mesophilic spore count obtained after a heat shock of 10 min at 80°C. The count for soil FG was 3.1×10^6 spores per g; the count for soil CO was 1.0×10^6 per g.

TABLE 2. D values (in hours) for dry heat sterilization at 120°C of spores of mesophilic aerobic bacteria dried on three carriers.

Organisms	Carriers		
	Paper strip	Glass tube	Sand
B. subtilis var. *niger*			
Spore crop A	0.91	0.94	1.6
Spore crop B	0.63	0.72	1.2
B. subtilis 5230			
Spore crop A	0.72		1.3
Spore crop B	0.47	0.50	1.0
B. subtilis 120	0.38	0.28	1.3
B. coagulans WH-9	0.96	1.1	1.4
Soil Isolate-44X	0.50	1.0	1.3
Soil Isolate-69C	0.98	1.2	1.7
ETO Isolate-TG*	0.77	1.0	1.6

*Organism with high spore resistance to gaseous ethylene oxide sterilization.

TABLE 3. D values (in hours) for dry heat sterilization at 120°C of spores of several anaerobic or thermophilic bacteria dried on three carriers.

Organisms	Carriers		
	Paper strip	Glass tube	Sand
B. stearothermophilus strain 1518			
Spore crop A	0.31	0.28	0.47
Spore crop B	0.32	0.22	
C. sporogenes (PA 3679)	0.27	0.27	0.7
C. sporogenes strain Vera	0.15	0.27	0.6
*C. tetani**	0.28	0.22	
C. perfringens (type A)	0.25	0.25	

*Nontoxigenic strain.

TABLE 4. Effect of vacuum during dry heat sterilization on the destruction of bacterial spores at 120°C.

Organisms	D values (in hours)	
	Air and atmospheric pressure	Vacuum* (10^{-1}-10^{-2}mm Hg)
Paper strips		
B. subtilis var. *niger*		
Spore crop A	0.91	0.30
Spore crop B	0.63	0.61
B. subtilis 5230		
Spore crop B	0.48	0.24
B. coagulans WH-9	0.96	0.60
Soil isolate-44X	0.50	0.26
ETO isolate-TG	0.77	0.47
Sand		
B. subtilis var. *niger*		
Spore crop B	1.2	0.60
B. coagulans WH-9	1.4	0.56

* The samples were exposed in a preheated vacuum oven and the times of exposure were based on total time in oven. No correction has been made for lag time to reach exact temperature and vacuum.

TABLE 5. Thermal death times and D values (in hours) at 120° C for spores of *B. subtilis* var. *niger* entrapped in several solids

Compounds	Time to sterilize	D value*
Solid rocket propellant	24	2.5
Asbestos patching cement	20	2.1
Plaster of Paris	12	1.1
Glue-base marble patching plaster	30	3.8
Dental materials:		
Inlay investment A	< 4.5	< 0.6
Inlay investment B	30	3.2
Inlay die material	36	3.9
Bridge model material	30	3.3

* The D values were calculated from levels of spore contamination found by assay of the solid materials. The weight of samples for a given solid was held constant and was in the range of 0.5-1.5 g for all materials.

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Appendix III

Review of the NASA-JPL Spacecraft Sterilization Program

G. L. Hobby, Jet Propulsion Laboratory

I. Introduction

The recommendations which have been made by various scientific councils on spacecraft sterilization suggest considerable difference in emphasis between the lunar and planetary programs. For Mars missions we are committed to extremely rigorous requirements, in which we either meet the necessary standards of caution or suspend the mission. For Venus, the recent radio emission data and their interpretations indicate that reconsideration of earlier stringent sterilization requirements can be made. For sterilization of lunar spacecraft the main effort has been to minimize biological contamination and to gain experience in the difficult art of spacecraft sterilization. Because of differences in emphasis between lunar and planetary missions, and the fact that no direct experience has so far been obtained with planetary spacecraft, this discussion will first review experiences with the sterilization of recent lunar spacecraft, and finally describe our approach for planned planetary flights.

II. Lunar Spacecraft Sterilization

A. Sterilization Policy

In September, 1959, the Space Science Board made official recommendations to the Administrator of the NASA on spacecraft sterilization. It was recommended that an "immediate study program be initiated to determine sterilization requirements and to develop recommendations compatible with present design and assembly processes." These recommendations recognized that sterilization efforts might be costly, and suggested that the initial study program should include consideration of engineering and operational aspects as guidance to further steps. The conclusions of the Westex Committee were recommended as guidelines for the study and development of methods.

In October, 1959, the NASA instructed the Directors of its field centers to initiate the studies recommended by the SSB and to carry out sterilization procedures to the extent technically possible.

In December, 1960, a memorandum from the Administrator's office of NASA was sent to the field centers. This memorandum stated that NASA policy consisted of the continuance of spacecraft sterilization studies, and that responsibility for approval of the planned sterilization procedures, for specific space missions, would be assigned to the Associate Administrator, NASA. This responsibility was subsequently delegated to the Director of the Office of Space Science Programs.

The major responsibility for technical implementation of the spacecraft sterilization program was delegated to the Jet Propulsion Laboratory, California Institute of Technology, since this agency is the one chiefly concerned with the lunar and planetary programs in which extraterrestrial contamination would be a factor.

The sterilization policy for lunar programs was interpreted and implemented by the JPL to conform, as far as possible, with recommendations made by the SSB and the NASA Administrator. The sterilization guidelines for the lunar program were established on the basis of the expected reliability of the Ranger spacecraft system, and the desired biological requirement. The philosophy of the JPL lunar program office has been to sterilize all lunar spacecraft to the extent technically possible, without jeopardizing the reliability and performance of the spacecraft. In a list of 16 priority items, reliability has been given first position and the sterilization requirement was placed number 14. However, it was intended to increase the emphasis on sterilization as reliability of the Ranger spacecraft system improved. Within these constraints, the sterilization program for lunar spacecraft has been developed.

B. Methods and Procedures

1. Internal Sterilization

On the basis of bacteriological assays conducted by Dr. Charles R. Phillips and his group at the Army Biological Laboratories to determine the internal biological contamination of electronic components, sterilizing agents which would penetrate into electronic components or plastic materials appeared necessary. It was shown that microorganisms apparently can become imbedded and remain viable in these materials for prolonged periods of time.

Because of the many unknowns involved in spacecraft sterilization, including the total numbers and types of organisms normally present, techniques which were effective against the most resistant types of known microorganisms were desirable to ensure suitable sterilization. Sporicidal agents were considered essential, since agents which will destroy spores would be expected to destroy virus and vegetative microorganisms. It was also necessary to seek procedures which would efficiently sterilize mating surfaces during the spacecraft assembly, as well as the surfaces exposed after final assembly.

Dry Heat. Two methods have been considered to accomplish internal sterilization: dry heat and radiation. Both are detrimental to spacecraft materials.

Very few references can be found in sterilization literature on the use of dry heat as a sterilizing agent at temperatures lower than 150°C. Obviously, since it is desired to minimize degradative effects on spacecraft materials, the lowest possible sterilization temperatures are desirable for sterilizing spacecraft components and structures.

For the lunar program dry heat sterilization cycles were selected on the basis of the best available data in the sterilization literature. A survey of the manufacturer's specifications for electronic components indicate that many are rated for operating temperatures of 125°C. Therefore, the effect of a 125°C heat cycle was expected to have minimal effects on such components. From limited data on low-temperature dry-heat sterilization, a thermal cycle of 125°C for 24 hours was adapted as the best compromise between component degradation, and a minimum heat sterilization cycle.

The JPL specifications for the use of heat sterilization of lunar spacecraft require that temperature equilibration be attained at 125°C, in the largest, centrally located, thermal mass, after which the system will be exposed for 24 hours. These specifications for thermal sterilization cycles have largely been applied to Ranger spacecrafts 3, 4, and 5 and are written into the specifications for the Surveyor spacecraft.

Although the specifications were originally adopted before any significant laboratory studies were done to verify the time-temperature cycle, subsequent studies by Dr. Carl Bruch, formerly of the Wilmot Castle Co., indicate that the technique appears to be generally adequate for lunar spacecraft sterilization. All thermally resistant bacterial spore types so far tested are readily destroyed by this cycle for populations between 10^5 - 10^7 organisms. Exceptions were mesophilic types when sterilized in garden soil.

Radiation Sterilization. Initial considerations for using radiation on a large scale for internal sterilization did not appear promising. Tests on typical spacecraft components indicated that, in general, those components which were sensitive to thermal sterilization were also degraded by sterilizing doses of gamma radiation. Transistors, diodes, batteries, certain capacitor types and teflon were degraded below acceptable specification. Also the potential hazard to personnel and the additional operational complexity involved in handling radioactive materials was considered highly undesirable. For these reasons as well as others, radiation sterilization was not further pursued. However, in special cases, where applicable, specification of a minimum dose level of 10^7 roentgens was established.

Sterile Assembly Procedures. During the main assembly and subsystem assembly process, it is necessary to ensure the sterility of mated surfaces between which microorganisms may become entrapped, and which will not later be accessible to ethylene oxide terminal surface sterilization. Both dry-box procedures, using ethylene oxide, and antiseptics have been used for this purpose.

In subsystems which cannot be heated entirely, due to the presence of one or more heat-sensitive components, heatable sections are subjected to the thermal cycle, and the sensitive components are assembled to the heated portion of the subsystem in a dry-box after all surfaces have been sterilized with ethylene oxide.

These procedures were applied as part of Ranger 3, 4, and 5 sterilization process to sterilize the Earth sensor, which contains a heat-sensitive photomultiplier tube; the gamma-ray spectrometer, which contains a thermally sensitive scintillator, a plastic case, and an optical coupling fluid; and the seismometer, which contains batteries and heat-sensitive transistors.

The main assembly procedure, in which subsystems are assembled to the main spacecraft chassis, was carried out using isopropanol to clean surfaces before mating. Although alcohol is not considered an adequate sterilant, suitable liquid sterilants do not exist at this time which are sporicidal and whose compatibilities are completely tested for all applications.

Studies have been performed by the Dynamic Science Corporation to test a number of sporicidal materials for this application. Formaldehyde in methanol appears to have promise for this application. However, difficulties are encountered in sterilizing electrical connectors with this agent, since polymerization of formaldehyde to paraformaldehyde leaves a residue with high electrical resistance on the contact surfaces. A satisfactory method of sterilizing electrical connectors has not yet been found.

2. Terminal Surface Sterilization

Terminal sterilization has been achieved by eleven-hour exposures of the entire assembled spacecraft to gas mixtures consisting of 12% ethylene oxide and 88% freon-12 by weight, at room temperature and 30-40% relative humidity, using the modified Ranger nose cone or shroud as a gas sterilization chamber. The gas sterilization cycle consists of a 1-hour fill time, an 11-hour soak time, and a final 1-hour purge with sterile nitrogen.

The nose cone assembly consists essentially of the Agena B adapter which fits atop the Agena second stage and the nose cone which is mounted on the adapter. The spacecraft is also mounted on the adapter and is separated from the booster stage by a plastic diaphragm. The conical single-piece shroud fits over the spacecraft and is sealed at the base by a large neoprene "O" ring on the upper face of the adapter assembly.

With this kind of structure, the Agena adapter, spacecraft, and nose cone can be handled as a single unit when assembled. This permits carrying out the terminal gas sterilization procedure under controlled conditions on the ground rather than atop the gantry, and permits protection from recontamination during subsequent ground handling and launch phases. The nose cone contains two entry ports, one near the base and another near the top. Each port contains a filler valve for filling and purging the shroud. The plastic diaphragm which isolates the spacecraft from the lower stage contains a cylindrical 3" x 1" bacteriological filter assembly consisting of a pair of millipore filter discs mounted in series with glass wool packed in the space between the filters. Filters are standard millipore types with a porosity of 0.45 micron. This arrangement permits air to pass into and out of the shroud after sterilization, as a result of ambient temperature variations, without introducing contamination. Another filler valve is mounted in this diaphragm to permit rapid purge of ethylene oxide from the bottom of the assembly at the end of the soak period. The diaphragm also contains a 5" Bobrick valve which is present to open under a predetermined acceleration loading and pressure differential. This valve permits rapid exhaustion of shroud air during the launch phase. Both the filter parts and Bobrick valve are covered during the gas sterilization procedures.

After the terminal sterilization procedure the shroud seals are maintained, although nitrogen is bled slowly into the cavity through a bacterial filter during transport of the sterilized spacecraft from the explosive safe facility to the gantry. The payload is thereby protected from recontamination during the mounting procedure and launch. Ejection of the shroud from the payload occurs above 90 km.

C. Technical Problems

1. Effects of Heat Sterilization on Reliability

One of the most serious technical problems concerns the effect of sterilizing heat cycles on the reliability of the spacecraft system. Although most of the spacecraft components now in use will survive heat cycles of 125°C for 24 hours, a number of critical items of hardware are seriously affected. In addition, almost nothing is known about the effect of these cycles on component lifetime. Reliability testing of sufficiently large scale has never been done to obtain a statistical analysis of failure rates over long operating periods after exposure to thermal sterilization. It is therefore impossible at this time to make an intelligent analysis of the effect of heat sterilization on overall mission reliability. Most engineers are becoming increasingly concerned over these problems, and strong pressures are being exerted to waive the heated procedures for obtaining internal sterility of spacecraft in the lunar program. To date, no component failure can be traced directly to thermal sterilization. However, on Ranger 3 and 4 series spacecraft, three component failures occurred on the prototype model, which did not undergo heat sterilization, whereas on the flight models which were heat sterilized, nine component failures occurred during tests. In the Central Computer and Sequencer system at least one failure occurred on each unit that was heated. Since the number of instruments tested was very small, it is not possible to establish the significance of these failures in relation to a thermal sterilization procedure.

On the other hand, a special vidicon camera system was developed for Ranger 3, 4, and 5 which could be subjected to heat sterilization. Seven of these cameras have so far been heat sterilized and no component failures were observed, nor was there any degradation in performance.

2. Sterilization During Main Assembly

The problems involved in sterilization during the assembly of subsystems to the main spacecraft frame involve not only the difficulty of finding suitable liquid sterilants, but also the time and care involved in the proper application to a large number of diverse parts.

The Ranger spacecraft bus has over 100 electrical connectors, nearly 1000 screws, over 500 mating flanges and more than 43 miscellaneous items to which liquid sterilants must be applied. Because the Surveyor spacecraft is a more sophisticated instrument the number of applications will be even greater. If subsystems are removed for repairs and replaced during test procedures, a considerable increase in the number of steps is involved. The reliability of this kind of sterilization procedure is considered inherently low because of the high chance of errors in applying liquids to structures having complex geometric configurations.

3. Problems of Terminal Ethylene Oxide Sterilization

Although gaseous ethylene oxide as a surface sterilant has proven the least troublesome from the standpoint of compatibility, ease of application, and sterilizing effectiveness, the time required for its application is a potential problem area. In case a malfunction is detected in the spacecraft, it may have to be removed from the booster and returned to the assembly area for repairs. If this occurs near the end of the firing window it is possible that the window will be missed if resterilization is required.

4. Quality Control

The enforcement of adequate sterilization quality controls over all the sterilization steps is a problem which has not as yet been adequately solved. The efficient monitoring of the hundreds of sterilization steps involved in the sterile assembly of subsystems, the main spacecraft assembly, and the heat sterilization cycles is a major operation.

The current method requires the cognizant engineer to apply sterilization methods to his own equipment properly. Where special techniques are required, such as sterile dry-box assemblies, the aseptic assembly procedure is worked out in consultation with a cognizant sterilization engineer or scientist. On Ranger 3 and 4, sterile assembly procedures were designed for the seismometer, the gamma-ray spectrometer, the Earth sensor, and the midcourse propulsion system. Where simple heating is the only procedure required, the cognizant engineer is solely responsible for proper application of the heat cycle in conformance with prescribed specifications. Since the construction of a considerable number of subsystems is done by subcontractors, they must be relied upon to meet the specified sterilization requirements, and sterilization monitoring must be done in a general way, along with engineering monitoring.

D. Inventory of Sources of Possible Biological and Chemical Contamination of Ranger 4

1. Waivers Issued

Although almost all of the structures of the Ranger 4 spacecraft were internally decontaminated using the heat sterilization cycle, it was impossible to apply this procedure to all components. Waivers from NASA were requested and approved on Ranger 4 for the following items:

1. Six wafer type batteries in seismometer
2. Five germanium transistors in capsule
3. Twelve pyrotechnic switches
4. Five primer chambers on midcourse propulsion system
5. Three acceleration switches
6. Main retromotor propellant and igniter
7. Spin motor propellant
8. Spare altimeter battery pack.

2. Possible Residual Contamination Due to Other Causes

The sterility of a number of items was jeopardized because repairs were made after heat sterilization procedures. These breaches occurred mostly because the removal of a small electronic component and its replacement involved removing and replacing potting compounds or resins. Resterilization of the potting compounds is not possible unless the structures are reheated or self-sterilizing resins are used. Reheating is considered undesirable from a reliability standpoint. These breaches included replacement of components in five data-encoder modules, the control computer and sequencer module, seven power modules, and one commutator module. Also a diode, a small terminal board, and a single resistor which had not been subjected to thermal sterilization before replacement, were added to the system.

Although attempts are being made to develop self-sterilizing plastics, the sterility of the plastic resin of the capsule and spacecraft battery casings is in question because the self-sterilizing properties of these materials has not been definitely established. The capsule power and sequencer was heated only 17-1/2 hours instead of 24 hours because of schedule problems. Many mating surfaces and electronic connectors could neither be sterilized with heat or subjected to proven sporicidal agents. Instead, isopropanol was used to clean surfaces of this kind.

3. Inventory of Organic Materials

The Ranger 4 spacecraft contained a variety of organic materials consisting mostly of polymers. These included epoxy resins, mylar, neoprene rubber, teflon, and plastic binding materials in the solid propellant. The capsule system included a 30-pound balsa wood impact limiter. Balsa wood was chosen for this purpose only after intensive efforts were made to find a non-bioorganic substitute material.

The estimated weight of organic material for bus and capsule systems is 120 pounds, excluding the capsule retromotor, and 310 pounds including the retromotor. The retromotor would be expected to ignite on impact. This weight represents 40% of the total spacecraft.

E. Technological Advances in Spacecraft Sterilization from Lunar Program Efforts

Imposing the sterilization requirement on lunar spacecraft has resulted in hardware development and better understanding of the problems:

1. Both engineering and biological problem areas have become better defined;
2. The inadequacy of the use of liquid sterilants for highly effective spacecraft sterilization has been demonstrated;
3. The need for simplification and careful control has become very apparent;
4. It has resulted in the development of a heat sterilizable vidicon camera;
5. Experience has been obtained in designing hardware for gas sterilization, e. g. , a multi-purpose nose cone which serves as a gas

sterilization chamber and as a protection from recontamination after sterilization, as well as the usual aerodynamic functions. A gas delivery system was also developed to deliver ethylene oxide mixtures under carefully controlled conditions.

F. Research and Development Programs

Studies conducted so far have included work on liquid sterilants, thermal sterilization, sporicidal resins, an ethylene oxide gas delivery system, and analysis of electronic components for bacterial contamination.

1. Liquid Sterilants

The objective of liquid sterilant studies was to test the sporicidal activity and the compatibility of formaldehyde, ethylene imine, betapropriolactone, epichlorohydrin, and epibromohydrin in water, methanol, and trichloroethylene. Five per cent formaldehyde in methanol, 5% beta-propriolactone in water and ethylene imine in all solvents, were found to be effective sporicidal agents. However, evidence in the literature on the carcinogenic activity of betapropriolactone negated this solution for use in spacecraft sterilization. Ethylene imine solutions were too corrosive for general use. However, solutions of formaldehyde in methanol show promise but require further study.

2. Thermal Sterilization

The objectives of the thermal sterilization studies were to define suitable time-temperature cycles. The work included the screening of heat-resistant bacterial species for maximum thermal resistance in order to use the more resistant types as standards for obtaining decimal reduction times and thermal death time curves. Except for several highly resistant soil samples tested, the cycle of 125°C for 24 hours appeared to be adequate for sterilizing populations of heat-resistant organisms greater than 10^6. These studies are being continued.

3. Sporicidal Activity of Potting Compounds

Various tests to determine the sporicidal activity of potting compounds and resins were made at the Jet Propulsion Laboratory. Resins are profusely used to hold electronic components firmly on circuit boards as adhesives, and to afford protection against shock and vibration, such as the polyfoam plastics. Five resin systems out of 32 tested thus far appear to have sporicidal properties. These may be used to repot electronic components after repairs have been made on sterile modules in order to ensure resterilization, although these resins may not meet the physical specifications. Further studies are required in this area.

4. Development of Ethylene Oxide Gas Delivery System and Nose Cone

The ethylene oxide-Freon gas delivery system was developed for the Jet Propulsion Laboratory by the Lockheed Missile and Space Corporation. The machine permits the safe delivery of sterilizing gas into the spacecraft nose cone or other enclosure under controlled conditions. Facility is provided to regulate gas flow, temperature, and humidity, and to dispose safely of excess gas which is purged from the shroud at the end of sterilization period.

The previously described Agena nose cone modification was also developed by Lockheed according to specifications of the Jet Propulsion Laboratory.

5. Bacteriological Assays of Electronic Components

Work done by the Army Biological Laboratories under Dr. Charles Phillips established the need for internal sterilization of spacecraft. Bacteriological techniques were devised for assaying the contamination in electronic components or other solid materials.

III. Sterilization for the Planetary Program

The planetary program sterilization requirement has been concerned mostly with the Mariner B Mars mission, originally scheduled for 1964. Considerable thought is also being given to more advanced Mars missions. The general approach to sterilization of these spacecraft is set forth by Dr. L. D. Jaffe in his report to the NASA Office of Lunar and Planetary Programs.

A. Policy

The planetary programs office at the Jet Propulsion Laboratory has given sterilization of Mars spacecraft top priority. Every feasible effort will be made to maintain a probability tolerance for landing a single viable organism on Mars to less than one chance in ten thousand. It is planned that this tolerance will be applied to the miss distance of Mars fly-by spacecraft, as well as to the sterilization of Mars landers. In order to do this, significant changes in the sterilization procedures used for lunar spacecraft are contemplated.

B. Philosophy

Our experience with sterilization of the Ranger spacecraft has emphasized the need for radical simplification of spacecraft sterilization procedures in order to ensure a high degree of control. Major responsibility for sterilization of hardware cannot be delegated to engineers, subcontractors, and technicians without careful monitoring. The procedures must be reduced to a small number of carefully executed steps monitored by sterilization scientists and engineers. Adequate testing and practice of each step in the sterilization process must be performed before the methods are applied to flight spacecraft.

C. Recommended Methods and Procedures

As recommended by Dr. Jaffe, the most feasible method for obtaining sterility to a high level of confidence involves the concept of thermal sterilization after enclosure of the assembled spacecraft in a bacteriologically sealed capsule, or nose cone. This would reduce the decontamination process to a minimum of operational complexity. Access to the spacecraft after sterilization cannot be permitted. The probability of not achieving sterility would greatly increase with the number of exceptions to thermal treatment. These exceptions, for example, the use of dry-box techniques, must be minimized.

In order to achieve this, engineering concessions will be made on Mariner B spacecraft design and mission performance. The system would consist of a gas sterilized bus carrying a heat sterilized landing capsule. The bus and capsule would be launched into a Mars trajectory so that the probability of impact of the Mariner bus would be less than 10^{-4}. At an appropriate place in the trajectory the bus and capsule would be separated, and the sterile capsule provided with sufficient impulse to place it on an impact trajectory with Mars. The bus would then fly by the planet making observations of the surface from several thousand kilometers, while the sterile capsule would enter the atmosphere and be parachuted to the surface.

One of the major reasons for the split-capsule concept is the concern for the undesirable effect on reliability if the entire spacecraft system were heated. Reliability is a major engineering concern for any space mission, and it is particularly critical for very long missions, such as the 6- to 8-month Mars flight. The philosophy in this case is to place maximum emphasis on sterilization of the Mars capsule and to release the severe constraint of heat sterilization on the bus.

D. Derivation of Recommended Heat Sterilization Cycle

The heat cycle which has been recommended for the Mariner B sterilization specifications is 135°C for 24 hours. The cycle was derived from thermal death-time curves determined for resistant soil samples. The microbial populations of these soils displayed the highest thermal resistance of all the samples screened during the Wilmot Castle study. This unusual thermal resistance, however, was identified with the soil substrate rather than the species present.

Decimal reduction times, or D values, were determined using multiple end-point techniques according to the method of Stumbo:

$$D = t/(\log A - \log B)$$

Counts of the soil microbial population were made to determine the initial number of organisms in the test, A. The constant B was determined by the number of tubes showing recoverable organisms after exposure time, t, when some fraction of the tubes in a set contained recoverable organisms. D values were obtained for six points over the temperature range 120° - 160°C. Thermal death times or F values were computed from respective D values, and estimated spacecraft contamination levels, for a survival probability of 10^{-4}, using a modified equation of Schmidt:

$$F_p = D_T (\log N_o + 2 + \log 1/p)$$

where p is the probability of a single surviving organism in a population N_o, and D_T the decimal reduction time, at a temperature T. Estimation of the probable spacecraft contamination level, or N_o, is 10^9 organisms. It is felt that these values are conservative and will ensure sterilization of the spacecraft to a tolerance of at least 10^{-4} if properly applied. More concrete evaluation of the numbers and types of organisms found in the spacecraft system, and the thermal resistance of microorganisms in the spacecraft environment may make it possible to reduce the severity of the heat sterilization requirement to some extent. However, until we are more certain of our sterilization standards the 135°C, 24-hour cycle will continue to be our operational objective.

E. Funding Requirements

Estimation of the cost of the present sterilization program is difficult to arrive at because extra costs are often hidden in special development projects, increased labor costs, or test programs which are related to normal development procedures and are difficult to analyze. However, fairly reasonable estimates are possible.

The Hughes engineers give a cost estimate of about $1.5 million for the sterilization of seven Surveyor spacecraft or less than 2% of the total program cost.

Estimates of cost increase contributed by sterilization of Ranger spacecraft range from 5 to 10%, or about 1 to 2 million dollars.

If these values are compared with the cost of the total vehicle, including the second stage and first-stage booster systems, the percentage cost is very low.

The efforts required for developing heatable hardware and for reliability studies to determine the effects of thermal exposure would cost several million dollars.

However, an extensive component reliability problem exists, apart from the special problem of heat sterilization. Reliability engineers estimate that an adequate reliability study for planetary missions would cost from 10 to 25 million dollars for a 2- to 4-year program. The extra effort to determine the effect of heat sterilization would probably be from 2 to 5% of this or one-half to 1.25 million dollars. Therefore, if the reliability studies related to sterilization were included in the general reliability study program the relative cost would not be great.

The Jet Propulsion Laboratory has already begun an effort in reliability testing on several critical components for the Mariner B capsule. The effort will involve applying sterilization cycles up to 145°C for 36 hours, and subsequent 10,000-hour life tests on these components.

IV. Summary

Considerable progress has been made on the lunar program in establishing sterilization procedures, defining problems and developing hardware. It is clear that important changes in methodology are required for Mars landing spacecraft. The problem of achieving sterility tolerances of 10^{-4} or lower is extremely difficult because of engineering as well as biological considerations.

The method which appears to permit the best approach to sterilization to high tolerances is the terminal heat sterilization method with the spacecraft in a protective shroud.

Intensive efforts and time are required to solve the many detailed problems that such a procedure implies.

Appendix IV

Letter From T. Keith Glennan (then NASA Administrator) Describing Directives on Sterilization

October 13, 1959

Dr. Hugh Odishaw
Executive Director, Space Science Board
National Research Council
2101 Constitution Avenue
Washington 25, D. C.

Dear Dr. Odishaw:

This is in answer to your letter of September 14, 1959, in which Space Science Board recommendations concerning space probe sterilization were made to the NASA.

This Administration agrees with the Space Science Board that a need exists for more factual information concerning the requirements for sterilization of space probes. In recognition of the possibility that earth life forms may be conveyed to the moon and the planets via space vehicles, the NASA has adopted the general policy of sterilizing, to the extent technically feasible, all space probes intended to pass in the near vicinity of or impact on the moon or planets. To implement this policy in regard to currently planned space probes, the following actions have been accomplished:

a. Instructions have been given to the Space Technology Laboratories to sterilize the Atlas-Able 4 Lunar Orbiter payload. In these instructions, NASA recommended soaking the payload for four to five hours in gaseous ethylene oxide contained in a polyethylene bag shrouding the payload.

b. The Goddard Space Flight Center is being instructed to sterilize the lunar miss payload Delta P-14. In this case, the procedure will include a complete inventory of the composition of payload components and materials.

c. Instructions are being forwarded to the Jet Propulsion Laboratory to develop technical procedures for the effective sterilization of all lunar Vega probes and for the inventory of components.

With regard to the first of the Space Science Board recommendations contained in the above referenced letter, the NASA will discuss with the Army Biological Warfare Laboratories and other competent groups the means of implementing and the funding needs for a study of space probe sterilization requirements. This matter has already been discussed with Dr. Charles Phillips of Fort Detrick preparatory to subsequent more detailed discussions.

With reference to the second Space Science Board recommendation, action already underway will be continued to establish an inventory of the composition of payload components and materials of all NASA space probes as indicated in paragraphs b and c, above.

This Administration will maintain close liaison with the Space Science Board in order that the Board will be kept up to date on the progress made toward the resolution of this problem.

Sincerely,

T. Keith Glennan
Administrator
(NASA)

Appendix V

The Surface Temperature of Venus

by F. D. Drake,
National Radio Astronomy Observatory

Repeated observations at wavelengths of about 3 cm at the Naval Research Laboratory, and at 10 cm at the National Radio Astronomy Observatory, have shown a mean Venus disk equivalent black-body temperature of about 600°K. From these data, the spectral index χ appearing in the equation for the flux density S

$$S = K \lambda \chi$$

is found to be

$$\chi = -1.97$$

Application of the quoted observational errors in order to produce the extremes as permitted by the data gives

$$-2.17 < \chi < -1.75$$

Thus the spectral index lies very close to the exact value for a true black body radiator, $\chi = -2$. It is very unlikely that any physical mechanism other than thermal emission from the surface could reproduce so closely the thermal spectrum over such a long baseline in wavelengths (almost two octaves).

Statistical studies of the radio data have shown the radio intensity to be non-variable on a short time scale, which, with the value of the spectral index, militates strongly against the ionospheric theory of the radio emission. Thus the radio data give strong evidence that the high observed temperatures are in fact nearly the kinetic temperatures at the surface of Venus. At least the low atmosphere will possess temperatures nearly equal to the observed value.

A confirmation of this has now been provided by Spinrad and Kaplan, who have measured rotational temperatures in Venus infrared CO_2 rotational bands of 440 and 700°K. Thus it appears quite certain that temperatures of this order occur in the Venus atmosphere, and at the Venus surface.

It should be noted that the Venus radar reflectivity is about 10%, so that the actual kinetic temperatures are about 10% higher than the radio equivalent black-body temperatures.

With regard to the detailed temperature distribution over the Venus surface, the radio observations show that the mean disk surface kinetic temperature T_K follows with high accuracy the relation

$$T_K = 707 + 44 \cos L\ (^{o}K)$$

where L is the longitude of central meridian, L = zero being at the subsolar point. This gives directly the mean surface temperature at a given longitude, T_{KL},

$$T_{KL} = 707 + 56 \cos L\ ^{o}K$$

These temperatures have recently been confirmed by 10-cm observations near superior conjunction. If Venus is not synchronously rotating, and the temperature distribution in latitude is roughly the same as with Earth and Mars, the maximum surface temperature on Venus is about

$$T_{max} = 775^{o}\,K$$

and the minimum surface temperature is

$$T_{min} = 540^{o}\,K$$

If, in fact, Venus is rotating in synchronism with its orbital period, the range between maximum and minimum temperature is actually less. Thus the surface temperatures on Venus range somewhere between 540 and 775° K, more than sufficient to destroy any organisms of terrestrial origin.

It should be noted that no high-resolution radio measurements have been possible, so no study of temperatures of small resolved regions of the Venus disk have been made. Thus, small cool regions resulting from very abnormal topography or atmospheric circulation have not been ruled out. Nevertheless, extremely implausible circumstances (mountains tens of kilometers high, regions where air masses never shift) would be necessary to produce temperatures where terrestrial organisms could survive.

Appendix VI

Principal participants in the Working Subgroup on Space Probe Sterilization included the following:

A. H. Brown, Chairman	University of Minnesota
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A. Cherkin	Don Baxter, Inc., Glendale, California
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H. D. Hedberg	Princeton University
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Chapter Eleven

THE SCIENTIFIC ROLE OF MAN IN SPACE EXPLORATION*

I. Foreword

We have examined the role of man from a number of aspects, chiefly as a scientist-explorer, but also in certain other more indirect ways, with a view to maximizing the scientific return in space exploration. The following were among the questions considered. To what kinds of scientific missions will a scientist-explorer make his chief contributions? Can any specific guidance be given in the choice of manned missions, as compared to instrumented missions? If scientists are to go on space missions, what are the requirements for their selection, training, and career development? These questions were posed particularly with respect to (i) the Apollo (manned lunar landing) mission and (ii) later missions, still in the conceptual stage, such as a lunar laboratory, Earth-orbiting laboratory, and manned planetary missions. Scientific tasks suitable for the Mercury and Gemini programs, as well as the carrying out of maintenance, repair, and modification of equipment by man were also considered, but less extensively.

Some months ago the Space Science Board solicited suggestions from a number of scientists as to the scientific program for the Apollo mission. A summary of responses to this inquiry has been prepared by W. W. Kellogg and is appended to this report (Appendix I).

In considering the scientific program discipline by discipline, we have looked for ways in which man might assist by being present, in addition to those situations where his presence appears indispensable. Briefly, we conclude that in research on particles and fields, or on the atmospheres of the solar system, manned space units would contribute little or nothing at this time. In astronomy, man in space can also contribute little as a scientist or observer; we note, however, that with further increase in the complexity of orbiting astronomical observatories (OAO), a point will be reached where manned servicing, maintenance and modification of equipment will be desirable, and we suggest that the next step beyond the present OAO will require these capabilities. It is further apparent that a trained meteorologist in an Earth-orbiting satellite can make substantial scientific contributions. Biological studies would benefit from experiments carried out in a manned space laboratory. Finally, the lunar and planetary research program (including its biological aspects) requires the inclusion of scientists on expeditions. These findings have formed part of the basis for the discussions and conclusions in this Chapter.

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*See Appendix III for the list of participants in the Working Group on Man as a Scientist in Space Exploration.

It became apparent in the discussions that a precise definition of the role of the scientist in space explorations was extremely difficult to develop. It was quite clear that, in many studies involving the routine collection of data along certain well-defined lines, man would generally contribute nothing and could, in fact, be an actual impediment. For other purposes, it was quite generally conceded that in many instances a man, not necessarily a scientist, could contribute substantially to the reliability and effectiveness of a mission through piloting, as well as through the servicing, maintenance, modification and operation of complex equipment (as noted above). Finally, there was a general concurrence that a person with an appropriate scientific background could make crucial contributions to the success of many missions. This last conclusion arose from a number of factors, many of them difficult to pinpoint; but critically important was the ability of an informed mind, rigorously trained in the use of the scientific method, to select from the vast field of sensory data presented those features that are particularly significant, and to make relevant and efficient selection of alternatives among a number of courses of action. An obvious corollary is that it would be self-defeating to overprogram the time of a scientist-explorer; he must be allowed flexibility for the full exercise of judgment made, for unscheduled observations, and for modification of plans. Discussions also make it apparent, on the other hand, that the spacecraft requirements will make it necessary for the scientist-explorer to be fully trained and qualified as a crew member.

Within this framework we outline in Section II below some of the scientific objectives to be sought in (i) lunar exploration, (ii) permanent lunar laboratories, (iii) Earth-orbiting laboratories, and (iv) manned missions to Mars.

There are several places in a space system where a scientist can be placed: (i) at the point of data collection; (ii) on the Earth connected by a communications and control loop to an instrumented data-collection unit; or (iii) in the loop at a number of intermediate points. Discussion of this issue, particularly among the specialists concerned with this topic, revealed that in general, it is not possible at this time to make firm objective decisions on these questions, and that they are in urgent need of further study. It is particularly important that remote sensing and data-collecting devices continue to be developed in parallel with the extension of man's capability in space. Such devices are, in any case, absolutely necessary for operations in those regions where man cannot go in person. We urge further research aimed at the development of remotely controlled robot devices. These questions are discussed in Section III of this chapter.

The objectives of the Apollo mission and its overall relationship to the national space effort were extensively discussed. There was concern that the Apollo mission was being mislabeled as primarily a scientific effort in its own right and that such misrepresentation could react unfavorably on the more basic scientific parts of the space program. It is apparent that the Apollo mission is a great challenge as an expedition of exploration and discovery. As has often occurred in the past in similar efforts, this enterprise has become involved in international competition which serves further to confuse the objectives of the mission in the minds of many. This competition is an undefined mixture of a desire for national prestige, a race for military advantage, and a drive for natural resources, as well as a pursuit of scientific aims.

Manned exploration of space promises great scientific return and Apollo can be a fruitful first step in this effort. It is in this overall framework that Apollo has become a national mission of high priority. Although the mission itself is first an engineering enterprise aimed at ensuring that man reach the Moon and return safely, it is also the first step in the manned scientific study of the Moon and the planets. Thus, we conclude that the scientific return from Apollo should be maximized. We further conclude that this end can best be served, without prejudice to reliability, by including in the crew a scientist fully trained as an astronaut.

This recommendation leads naturally and inevitably to a recommendation for the establishment of an Institute of Space Sciences adjacent to the major astronaut training center. This institute would be aimed at ensuring that the scientist-astronaut's scientific skills will be unimpaired during the intensive phase of his astronaut training and will be further stimulated and enhanced in the other portions of his training and preparatory periods. In addition, such an institute could and should become a significant center for space science research. This suggestion and recommendations for the selection, training, and career development of the scientists involved in manned space exploration are set forth in Section IV below.

Detailed findings and recommendations concerning all the foregoing subjects will be found in Sections II through IV of this Chapter. Findings and recommendations on the direct science or scientifically useful tasks that a man in space might perform in the fields of biology, astronomy, and meteorology will be found in the Chapters on those subjects.

Finally, we recognize that the success of the Apollo mission will be critically dependent on several factors, a fact which leads to the following general findings:

> FINDING: Further information is needed on the lunar surface and environment prior to the first lunar landings. (Chapter Four on Lunar and Planetary Research makes specific suggestions in this regard.)
>
> FINDING: Further work is required on the physiological and psychological aspects of the Apollo program, and a full use of ground studies and the Gemini and Apollo-orbiter programs will be needed to strengthen these aspects.

II. Scientific Undertakings for Man in Space*

A. Introduction

The scientific exploration of space has been proclaimed a national goal, and manned lunar landing is a major step in the implementation of this program. Man's opportunities for scientific exploration of the Moon are practically unlimited. The lack of an adequate scientific endeavor could invite serious criticism of the program, while the impact of a successful scientific mission by means of a lunar landing will enormously enhance the importance of the Apollo program in the eyes of the world.

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*See Appendix III for the list of participants and contributors in the Working Subgroup on Scientific Undertakings for Man in Space.

B. Scientific Tasks for Manned Lunar Explorations

Among the most important scientific tasks foreseeable for manned lunar explorations are: (i) observation of natural phenomena, including micro- and macro-structure and composition; (ii) collection of representative samples; and (iii) emplacement of monitoring equipment. For a more extensive statement of objectives which we endorse, we refer to ''Draft Report of the Ad Hoc Working Group on Apollo Experiments and Training on the Scientific Aspects of the Apollo Program'' (July 6, 1962), prepared under the chairmanship of Dr. C. P. Sonett, NASA. For recommendations concerning both the science to be done and ways in which a man would be useful to this end, see also Chapters Two, Four, Eight, and Nine.

> RECOMMENDATION: One crew member of each Apollo lunar mission should have scientific abilities and training maximized, consistent with his required contribution to spacecraft operations. His scientific activities should be continued throughout his period of training in spacecraft operations. It is recommended that a scientist-astronaut* participate in the first lunar missions.
>
> RECOMMENDATION: While there would be an important scientific return from a manned lunar mission on which the scientist remains on the orbiting spacecraft, in the Lunar Orbiter Rendezvous (LOR) mode a maximum return is anticipated only if the scientist himself lands on the Moon.
>
> RECOMMENDATION: A ground-based (terrestrial) program should be established to determine the scientific value of geological exploration and sample collection by an observer-astronaut* in an unfamiliar terrestrial environment, connected by radio and vidicon links (including time delay) to a remote professional geologist.

C. Permanent Lunar Laboratories

The unusual lunar environment affords the opportunity of pursuing a new class of scientific investigations. Some of these require the establishment of a permanent lunar laboratory. The returns from the Apollo program will constitute only a beginning in lunar exploration. A sustained effort will be required to unravel the history and structure of the Moon, which, as noted elsewhere, is by far the most important objective. Examples of other new investigations are: (i) a search for surface and subsurface indigenous life and organic matter; (ii) the precise observation of the physical librations of the Moon; (iii) the study of lunar natural resources, including water; (iv) observations of magnetic fields, solar protons, etc., which cannot be made from within the Earth's magnetosphere; (v) the observation of faint radio sources with large antenna arrays (such observations might possibly be made on the back of the Moon to reduce terrestrial interference); (vi) very low-frequency radio observations of astronomical sources; (vii) far ultraviolet observations of astronomical sources requiring long observing times.

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*These terms are defined in Section IV. B below.

FINDING: We conclude that there is a definite foreseeable scientific need for a manned lunar laboratory.

D. Earth-Orbiting Manned Laboratories

A manned Earth-orbiting space laboratory later than Gemini, larger and more complex, will have unique value for studies in the weightless state. These studies are chiefly biological. Geotropic effects, orientation, growth and development, behavioral studies (both physiological and psychological), altered periodism, and other phenomena connected with biological time are key areas for investigation. Physiological and psychological research should be initiated in Gemini. Future orbiting missions will be useful in extending the biology program and could provide some justification for a manned orbiting laboratory. Manned orbiting satellites would also be useful in meteorological studies similar to those suggested for Gemini in Appendix II of this Chapter; also for some geophysical and astronomical investigations.

> RECOMMENDATION: A manned orbiting laboratory beyond Gemini will be useful primarily for biological studies; the time phasing and form of such development needs further study.
>
> RECOMMENDATION: Means of manned modification, maintenance, and repair of orbiting satellites should be developed for scientific experimental purposes as well as for mission support.

E. Manned Missions to Mars

Manned exploration of Mars promises to yield results of exceptional scientific importance. The planet may be the only one outside the Earth which can be investigated in great detail. The possibility that life may be discovered on this planet offers unusual challenges.

Because manned missions to Mars will not be possible during the next decade, it is important that attention be given to those factors in future missions which involve long lead times. The missions foreseen include: (i) manned landing; (ii) manned orbiters; (iii) landings on the satellites Phobos and Deimos; (iv) the establishment of a semipermanent observing station on the side of Phobos facing the planet for continuous observation of Mars.

> RECOMMENDATION: Manned exploration of Mars represents an extraordinary scientific opportunity; preliminary planning, especially of long lead-time items, should begin at an early date.

While the biological investigations should have first priority, they will be closely followed by surface geology and solid body geophysics, with the study of the Martian atmosphere coming next. Since the visits to the planet are likely to be of long duration, consideration should be given to landing parties containing more than one scientist, each of great disciplinary breadth.

> RECOMMENDATION: Biologists should be included in the NASA program of scientist-astronaut training early enough to ensure that adequate personnel will be available for the Mars missions.

> RECOMMENDATION: Since the manned Mars landings will pose serious problems of biological contamination, these problems should be considered in the developments of spacesuits, airlocks, sterilization procedures, etc.

F. Institute for Scientific Training

The subject of an institute for advanced space studies was also treated thoroughly from the standpoint of training and career development (see below). The following recommendation, which is repeated in Section IV.C of this Chapter, is also included here because it relates as well to the research aspects of manned space exploration.

> RECOMMENDATION: An institute of space sciences is required; it should be located immediately adjacent to the major NASA training facility for astronauts. The institute must be of the very highest scientific calibre to support the continued scientific activities of the scientist-astronaut, and to facilitate major efforts in the space-related sciences. It should maintain effective liaison with major centers of research in the space sciences in the United States and abroad. The institute, through affiliation with major universities, should function as a graduate school offering advanced degrees in various areas of science. If the institute is not established under contract with a major university, then it should be administered by that division of NASA responsible for scientific research and planning, for example, the Office of Space Sciences.

III. Man's Place in the Sensing and Control Loop*

A conventional automatic machine is designed to do only one task, but to do it quickly, repeatedly, and well. A slave robot, such as those developed by the Atomic Energy Commission, is more versatile in that its articulated limbs can be made to perform a multiplicity of tasks under the direct supervision and control of man. The supervision may be by direct viewing or by television monitoring, and the control channel may consist of direct mechanical connections or an intermediate

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*See Appendix III for a list of participants and contributors to the Working Subgroup on Man's Place in the Sensing and Control Loop.

electrical or radio link. Adaptive machines are being developed which, in a primitive way, can change their mode of operation, "learn" new tasks, and adapt themselves to new or changing conditions without man's intervention.

An adaptive slave robot, herein called a "telepuppet," (a term coined by F. L. Whipple, private communication) would combine the best sensing devices available, the monitoring and control aspects of a versatile slave robot, and such adaptive features as may be possible and desirable. Such a machine would appear to be an indispensible aid to man in the exploration of space.

It is recognized that man in a sensing and control loop can provide judgment, adaptability, improvisation, and selectivity to a degree which cannot be matched now or in the foreseeable future by adaptive machines which only report the results of their observations, experiments, and actions to man. On the other hand, telepuppets which combine self-adaptive operation and communication with and control by man, can make use of man's abilities in these areas and can also extend his senses and mechanical abilities.

The communication and control loop between man and a distant telepuppet suffers from the fundamental time delay set by the finite velocity of radio signals, and the relative distortion and imperfection of the sensing, communication, and articulated components. For great distances between man and machine, the time delay becomes significant, and it is here that the proper selection of self-adaptive vs. controlled functions is of greatest importance.

A comfortable, safe, and secure scientist-astronaut at the scene of action using his own sensors and motor skills and also using near-by telepuppets to extend his senses and actions, can make observations, do experiments, and perform tasks which would be superior to those he could do if he were remote from the scene but connected to telepuppets by wide-band communication and control channels. However, when his life-support system does not or cannot provide the environment required to keep him at his highest operating efficiency on the scene, he might well do a better job by remaining on the Earth as a part of a more extended sensing and control loop. Furthermore, many regions of great scientific interest are too hostile to allow manned visits for the foreseeable future.

There is a requirement for research to determine the characteristics of sensing and control loops made up of various sensors, mechanical articulated limbs and locomotion devices, self-adapting and learning computers, self-repairing and redundant systems, control and communication channels, and man. No valid prediction of man's position and role in this loop for maximum contribution to space exploration can be made in the absence of this research.

The study should include an evaluation of relative cost, hazard, degradation of sensing or performance, distortion, delay, etc., of having man at various positions in the loop. It should include not only man on the scene and man on the Earth, but also various other possibilities such as a scientist-astronaut connected to a group of senior scientists on the Earth by two-way communication links, and telepuppets on a planet under the control of a scientist-astronaut on, or in orbit around, the same planet. The study should include training and tests of various arrangements in simulated space environments, including an attempt to evaluate the scientific results that might be obtained under each arrangement.

We believe that present research toward the development of a family of versatile telepuppets is inadequate, in view of their potential importance for expanding man's abilities in space and for exploring regions where man himself will not venture in the foreseeable future. It would be anticipated that telepuppets would accompany man in orbit and on the Moon, would precede him in going to Mars, and might go to Venus, Jupiter, Mercury, etc., in lieu of man.

> RECOMMENDATION: It is therefore recommended that NASA, possibly in conjunction with the AEC, fund at a substantial level theoretical and experimental investigations in the following areas:
>
> (1) Study of the characteristics of various scientific sensing and control loops which include man at several places in the loop.
>
> (2) Construction and testing of models of a family of telepuppets which are as versatile as can be done with present technology.
>
> (3) Research and study of the basic components required to build succeeding generations of superior telepuppets.

IV. Selection, Training, and Career Development*

A. Introduction

The importance of the role of science in space activities resides not merely in the science objectives themselves, or in the added degree of safety of spaceship operation in an entirely unfamiliar environment, but also in the overall increase in the stature of science throughout the nation, and indeed the world. The most careful consideration is therefore needed in the selection and training of the spaceship crews in science, so that they can assure both the success of the mission and the maximum contribution to science from the enterprise.

The burden of responsibility for the success of the scientific mission in space will be centered on appallingly few individuals, working with complex scientific and operational equipment. This will make teamwork imperative. The success of the spaceship missions will depend upon intensive collaboration among its occupants, and through the communication channels with the engineers and scientists on the earth. It is within this framework of the union of spaceship engineering and piloting skills with scientific knowledge and experience that the following outline of required personnel categories has been reached.

B. Types of Personnel for Scientific Space Missions and their Selection and Training

It seems to us that scientists involved in the space effort will probably make their specific contributions in four ways. By far the most important in the present

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*See Appendix III for a list of participants and contributors to the Working Subgroup on Selection, Training, and Career Development.

context and the one to which priority attention should be given is the position of SCIENTIST-ASTRONAUT, a fully qualified and recognized scientist with astronaut capabilities. The SCIENTIST-PASSENGER and the ASTRONAUT-OBSERVER represent two other in-flight categories. Finally, there is the role to be played by the GROUND SCIENTIST.

We feel that all the different categories just named will be involved in the development and operation of the scientific program in space, on the Moon, and on the planets. However, certain time schedules in the development of vehicles dictate what kind of scientist can go, on what kind of a mission, and at what time. Our estimates concerning availability of vehicles for manned space missions, together with our estimates of the needs for scientific personnel, are shown in Figure 1.

Astronaut

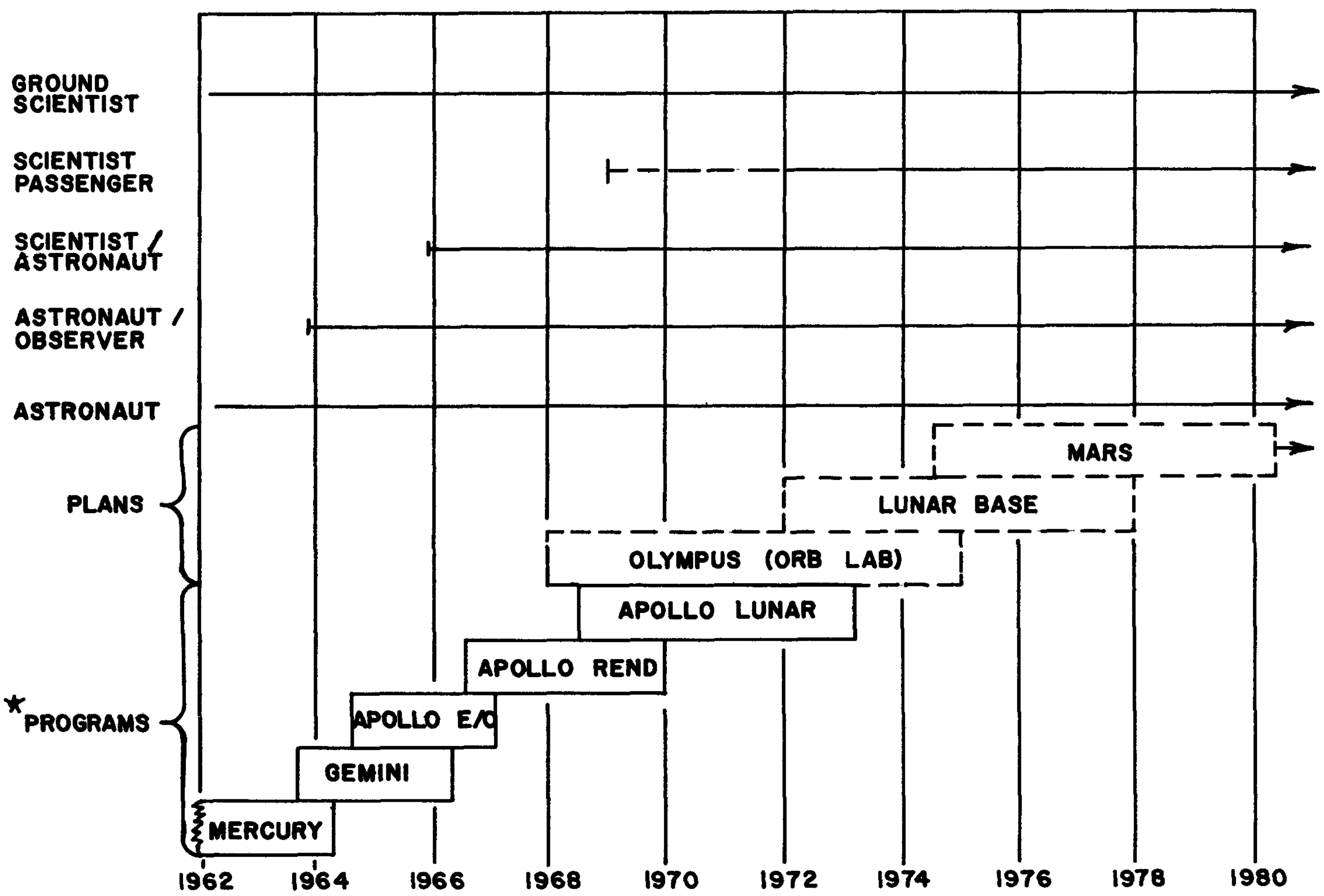
We expect that flight crews (pilots) will be required to control vehicles into the indefinite future, but aside from a general orientation into the purposes of the scientific mission, and possibly instruction in operating more or less automatic devices, the selection, training and career development of the flight crew is a matter for NASA flight crew operations.

Ground Scientist

The Ground Scientist uses an observer as a remote experimenter. He is primarily selected by the acceptance of a research proposal by NASA. The proposal may have originated from him, or from other scientists for whom he is the representative. It appears that the scientific community needs stimulation to offer valid, feasible experiments, and such proposals by the scientific community need evaluation. Thus, the problem may be one of getting an array of proposals to select from, rather than of discarding lesser proposals.

The Ground Scientist is being asked to devise and perform an experiment in an unusual way. He must direct a remote "assistant" through a communication link to manipulate, to observe, to report. The communications link will probably be time-limited, noisy, and distorted. There are, then, simple mechanical restraints on the selection of the Ground Scientist. He must be able to speak intelligibly and to understand speech through noise. He must also work effectively with the Astronaut-Observer who is acting as remote experimenter; and this suggests that personality variables may have some effect on selection.

The Ground Scientist must be trained to comprehend a number of areas: The limitations of a man in a spacesuit, both normal and inflated; the limitations of vehicle space and layout; the inherent limitations of vehicle control and maneuver. The use of remote assistants involves practice both with men in general, and with the Astronaut-Observer assigned to the project in particular. A limited language for direction and a vocabulary for description will be required. A simulator facility is certainly required in order that feasibility of experiments can be verified and practice obtained for the working team.



* NASA SP-4 27 APRIL 1962

FIGURE 1

Time Requirements for Personnel in Manned Space Missions.

Scientist-Passenger

Programs for a Scientist-Passenger aboard a space vehicle will originate with the individual scientist, a university space group or department, or with any other agency with interests in space research. In order to stimulate the submission of such proposals, NASA should make available to the scientific community certain information regarding the scope, limitations, and possibilities of space research.

NASA will have the responsibility of reviewing such proposals with the assistance of special disciplinary and/or interdisciplinary reviewing committees, in order to determine the value and feasibility of a specific program of space research. The research proposal selected may not be unrelated to the availability of a uniquely qualified investigator to carry out the mission.

The Scientist-Passenger will be an established scientist, a person recognized for his research competence in his discipline. His selection will depend upon his unique abilities to carry out a specific proposed scientific mission either of his own design or submitted by others in his disciplinary area. Additionally, his knowledge in scientific areas other than his own discipline may well be an important criterion for selection, since he may be required to participate in other planned scientific programs.

The Scientist-Passenger should satisfy all of the standard physical, medical, and psychological requirements applicable to crew members, except for certain deviations in age, height, vision, etc., which would not adversely affect his participation in the mission. The physical qualifications for the scientist will depend upon the best understanding of the stresses of space flight available at the time of the mission. It is acknowledged that the selection and training requirements for physiological and psychological status and conditioning will vary as more data become available; however, the Scientist-Passenger will be required to meet any or all of them that are essential to survival or success of the mission.

With respect to training it is acknowledged that the qualifications of the Scientist-Passenger as a competent and qualified scientist rule out the need for a training program in his discipline. However, he may be required to be briefed or to spend time in extended review of areas outside his discipline in which he will be required to make observations or measurements in order to be well versed on current techniques, methodologies, and results in these areas. In addition, it will be necessary for him to participate in flight simulation sufficiently to become fully adapted to the motion, heat, pressures, etc. encountered in flight and to be able to adjust to sudden environmental changes. He will need extended training in communication procedures in order to communicate with Earth or with other crew members by voice and, if necessary, by brief coded messages. He will need complete briefing and familiarization with mission goals, with critical instrumentation and controls involved in emergency procedures, and finally as a coordinated member of a flight crew, even through ordinary operations will not devolve upon the Scientist-Passenger.

Astronaut-Observer

The Astronaut-Observer is a qualified astronaut with sufficient additional training in scientific fields to permit him to make scientific observations or carry out experiments during space missions. He may serve as a remote perceiver, recorder, communicator, and technical aide for the Ground Scientist.

Until such time as Scientist-Astronauts become available, the Astronaut-Observer will be the principal person who can conduct research on space missions. Even after the advent of the Scientist-Astronaut it is anticipated that he will continue to be a useful observer for the Ground Scientist and a scientific assistant to the Scientist-Astronaut or the Scientist-Passenger.

He will be selected from the existing pool of astronauts on the basis of his scientific capabilities, and his interest in participating in scientific investigations.

Depending upon his scientific background and the specific requirements of the research program, he may need additional intensive training in a specific area such as astronomy, geology, meteorology, or the life sciences. His training should include both background information in the area of research involved and intensive briefing with the scientist concerned.

Scientist-Astronaut

The Scientist-Astronaut will combine the qualifications of an experienced scientist with that of a qualified astronaut. A Scientist-Astronaut is a professional scientist of considerable maturity and breadth in the physical or biological sciences, trained also for spacecraft operations. Since he will represent the most satisfactory combination by which both the safety of spaceship operation and the attainment of maximum scientific goals can be assured, considerable attention must be directed to his proper selection and ultimate training.

Candidates for the Scientist-Astronaut position will be drawn from three principal sources: (i) experienced scientists who would be required to qualify for astronaut status, (ii) experienced pilots who would be required to attain scientific and astronaut status, and (iii) a career development program for Scientist-Astronauts. Since approximately an eight-year period would be required between the time a bachelor's degree is obtained and the scientist has an advanced degree and the necessary stature and recognition to qualify as an experienced scientist, and in addition, a two- to three-year period would be required in specific training as an astronaut, it is clear that the first Scientist-Astronauts must come from the first source above, namely the already experienced scientist group. Meanwhile, activation of scientific training for the other categories through career development programs should begin at once in order to augment the lists of volunteer scientists with men specifically trained in space science from the time they acquire bachelor's degrees.

Selections will be made from these lists of volunteering scientists who have passed the same preliminary physical, psychological, and aptitude tests as are required for astronauts. It is expected that the ages of these men (or women) will be in their early thirties, and that their advanced degrees and research specialties will be in a field relevant to the space program under consideration. The National Academy of Sciences should be requested to appoint an appropriate committee to make the nomination of candidates, giving due regard to the scientific qualifications and to the scientific programs advanced by the candidate. It is recommended that at least six such scientists should be selected periodically, presumably annually, to begin the astronaut phase of the training.

A flow chart summarizing the training and selection of scientific participants in the space effort is shown in Figure 2. Three potential sources of candidates for

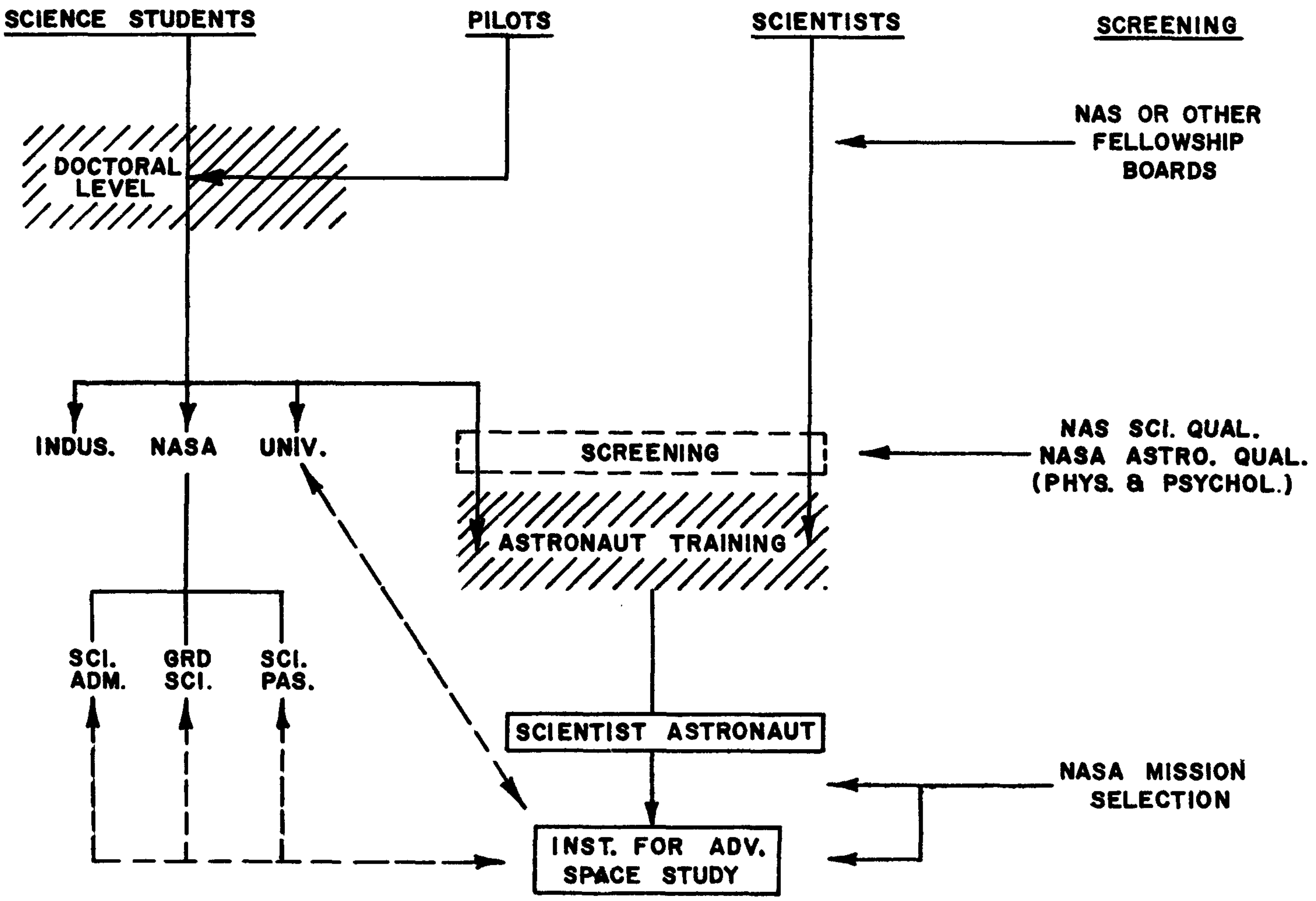


FIGURE 2

Space Science Career Program.

Scientist-Astronaut are indicated: (i) existing scientists who require only astronaut training, but who must be screened prior to such training by NAS and NASA for scientific and astronaut qualifications, respectively; (ii) existing pilots, who require further scientific training and experience before progressing to screening for astronaut training; (iii) science students who require advanced scientific training and experience before progressing to screening for astronaut training.

Those who qualify as Scientist-Astronaut will remain available for final mission selection by NASA and will require continuing astronaut and scientific training (and indoctrination) and opportunity to utilize their research potential. The Institute for Advanced Space Study (see below), located in the vicinity of the Manned Spacecraft Center, is envisaged as a vital mechanism for maintaining Scientist-Astronauts in a continuing state of mission readiness without degradation of their scientific or astronaut capabilities.

Science students shown at the left of Figure 2 develop after many years of conventional training to the point where, by choice or selection, they volunteer for Scientist-Astronaut training or other activities. Fostering the interest of this group in the space sciences will be required if their recruitment into the program is to be accomplished. Fellowships, summer programs, and similar mechanisms sponsored by NASA will be required to present the opportunities of a space science career to individuals at all levels of professional development, including beginning college students. Those who aspire to Scientist-Astronaut careers but fail to qualify will find their talents in great demand in NASA, in the universities, and in industry, as shown at lower left. NASA's requirements for Science Administrators, Ground Scientists and Scientist-Passengers are indicated in the lower left of the figure. The dotted arrows show various interactions between the Institute and outside organizations.

> RECOMMENDATION: In view of the foregoing considerations, we recommend that maximum possible participation of scientists be effected in all space programs: on board vehicles in flight, at extraterrestrial stations, and on the ground. In particular, one crew member of each Apollo lunar mission should have the maximum possible scientific abilities and training consistent with his required contribution to spacecraft operations. His scientific activities should be continued throughout his period of training in spacecraft operations. It is recommended that a Scientist-Astronaut participate in the first lunar mission of the Apollo program.
>
> RECOMMENDATION: We recommend that participation of scientists include the following categories:
>
> (i) Scientist-Astronauts—men who will combine experience and resourcefulness of both trained scientist and trained astronaut;
>
> (ii) Scientist-Passengers—experienced, mature scientists with adequate training in critical and emergency spacecraft operations.

(iii) Ground Scientists—leading scientists in pertinent fields, who will collaborate with spacecraft personnel in the accomplishment of the scientific mission.

(iv) Astronaut-Observers—astronauts with varying degrees of special scientific observational training.

RECOMMENDATION: We recommend that scientists for participation as Scientist-Astronauts in Apollo Earth-orbiting satellites and later missions be recruited, selected, and trained. Recruitment procedures should assure that all qualified individuals in the appropriate scientific disciplines have opportunity to apply, regardless of current employment status.

RECOMMENDATION: We recommend that present Astronauts be given scientific training appropriate to their early collaboration with Ground Scientists in coming missions.

RECOMMENDATION: We recommend that the National Academy of Sciences be requested to devise and operate a mechanism by which:

(i) The bases for selection of Scientist-Astronauts and Scientist-Passengers should be established.

(ii) The nomination of successful candidates can be accomplished.

RECOMMENDATION: We recommend that in order to ensure an adequate pool of qualified candidates for all the Scientist categories needed, graduate fellowship support in the sciences is required.

RECOMMENDATION: That during the remaining Mercury, Gemini, and the Earth-orbiting Apollo programs a vigorous research program be carried out to study human physiological and psychological reactions in the space environment with the object of determining the conditions necessary to insure that man remain an effective observer in space.

C. Institute for Advanced Space Study

The program discussed above defines the need for an institute to ensure maintenance and development of the scientific qualifications of Scientist-Astronauts and, in addition, to advance space science generally. This training will be carried out primarily at the proposed institute where continued instruction and information in current space knowledge will be made available, and where the pursuit of specialized research can be maintained, as well as necessary aptitude and fitness requirements. Therefore it should be located in close proximity to the major training center for astronauts. In addition, it would provide indoctrination and education for Astronaut-Observers, and familiarization and training of Scientist-Passengers in space flight requirements. Finally it would bring Ground Scientists together with crew members of space vehicles for the attainment of coordinated effort in carrying out scientific missions. Another function of such a center would be to maintain a complete library of historical and current space flight data as well as a comprehensive literature in the sciences relevant to space flight missions.

Although the staff for such a laboratory and training center cannot be specified at this time, it is anticipated that it might include a "permanent" staff of distinguished representatives of the principal sciences involved in space research, as well as a group of one- or two-year visiting scientists. Training responsibilities might be shared by the resident scientists, the visiting scientists, and fully trained astronaut scientists. Training will involve classroom instruction, laboratory experience, and realistic simulation of missions.

It is imperative that the institute maintain a scientific program of the highest caliber. In addition to the need for the best possible staff selection, it will be important to maintain close liaison with centers of research in universities and elsewhere, in the space sciences throughout this country and abroad.

Special consideration should be given to the administrative requirements of this institute. Since its primary functions are scientific and scholastic, not operational, it would be appropriate for NASA to enter into a contract with some university or group of universities under which the institute would be established and operated. It should have close ties with those offices in NASA where science is the primary concern.

> RECOMMENDATION: We therefore recommend that an institute of space sciences be established, and that it be located immediately adjacent to the major NASA training facility for astronauts. The institute must be of the very highest scientific caliber in order to support the continued scientific activities of the Scientist-Astronaut, and to facilitate major efforts in the space-related sciences. It should maintain effective liaison with major centers of research in the space sciences in the United States and abroad. The institute, through affiliation with major universities, should function as a graduate school offering advanced degrees in various fields of science. The institute, laboratory and library facilities should be of the highest quality. If the institute is not established under contract with a major university, then it should be administered by that office of NASA responsible for scientific research and planning.

Appendix I

Summary of Responses to Space Science Board Inquiry on Scientific Program for the Apollo Mission

Prepared by W. W. Kellogg

Some 14 replies to the SSB Questionnaire of April 20 are pertinent to the Summer Study discussion of Man as a Scientist in Space Exploration. In undertaking to summarize these responses, I have taken the liberty of reformulating the original questions so that they will apply more closely to the set of questions posed to the Summer Study, and then giving a composite answer drawn from these letters. This is a dangerous process, but it turns out that there was a good deal of unanimity in the attitudes expressed, making it possible to find a consistent story by piecing the various contributions together.

I have not covered the comments about where to land, whether the dark or light hemispheres would be better for scientific work, or the discussions of en route experiments. The summary relates entirely to the scientist who will some day set foot on the Moon with the terrible responsibility of reporting back to the world the characteristics of that strange world.

1. Should a scientist (at least one) be a member of the first Apollo crew?

Emphatically, "Yes!" (See under No. 5 for justification.)

2. What should his scientific training be?

Physics, chemistry, and field geology.

More specifically, he should have studied and be familiar with the following subjects:

Lunar observations, not only the traditional ones now done from the ground (telescopic, radiometric, radar, etc.), but also the latest results of unmanned landings.

Laboratory studies of materials in a vacuum, under various conditions of irradiation, proton bombardment, etc.

Field geology, to gain intuitive insight into various types of formations (volcanic, impact), and to develop the ability to perceive important features of his surroundings and to describe them clearly and objectively. However, he should be cautioned from depending too much on terrestrial analogies when studying the Moon.

3. Should he also be trained as an astronaut?

Of course. He should be familiar with all aspects of the spacecraft and be able to take over in an emergency. However, his qualification as crew member would not depend so much on his ability as a space-pilot as on his scientific aptitude.

4. What should this scientist do on the Moon?

The number one job would be the acquisition of representative samples of lunar rock and dust for return to Earth. If time on the Moon is short, he will be able to do little else.

His next most important job will be to note carefully his surroundings, spot unusual features, make verbal and photographic records of his observations. He should be permitted to travel as freely as possible.

If time and space permit, he would install apparatus to measure, record, and telemeter back to the Earth such things as seismic activity (3-axis seismometer), gravity and tilt, magnetic field, temperature of sub-surface, radiation environment, etc. This part of the job could also be done by other members of the crew who were not scientists, if they were available; in fact, this would probably be preferable, leaving the scientist member more time for his special investigations.

It is not necessary to spell out in detail what he will do at this stage. One of the chief advantages of a man over a machine is that he does not have to be programmed far ahead of time, and his design is fixed. We will learn much from our first unmanned landings that will suggest what he will need to do.

5. Why should a scientist be included in the crew?

Because the chief justification for going to the Moon (beyond the simple realization of a "National Goal in Space") is to acquire knowledge about the Moon.

Because a trained scientist has the ability and the motivation to gather the most significant information available during his short stay in that strange environment.

Because, in particular, the gathering of rock and dust samples for return to Earth must be done with the greatest discrimination, and with a scientist to screen quickly samples for significant characteristics.

Because there may be unexpected emergencies, or unanticipated phenomena on the Moon that may endanger the expedition, and an alert and knowledgeable scientist in the crew might make the difference between success and failure.

6. How do we develop our astro-scientists?

They should be selected from scientists at the graduate student or early post-doctoral level. Perhaps four would be selected immediately, more later.

(The maximum age that he could be at the time of the flight must be decided by the human factors experts.)

Their training scientifically should be in the areas listed in No. 2. They should demonstrate ability to do original research in at least some of these areas. Facility with scientific instruments and measurements will be essential. They should be able to relate quickly new ideas, and be good at order-of-magnitude calculations. Training should start now and last for at least four or five years.

They should go through astronaut training for part of each year to become familiar with problems of space flight. It is hoped that this would not involve too large a fraction of their time, since emphasis should be on their development as scientists.

Appendix II

Report on Meteorological Training for Gemini Pilots

Prepared by W. W. Kellogg

At the meeting on Man as a Scientist in Space Exploration the question of the use of a man in space for meteorological research was briefly discussed. The following appeared to be the main conclusions.

A. Opportunities for Meteorological Research from Manned Orbital Stations

While a great deal of valuable information has been obtained by TIROS satellites, and still more will be forthcoming from NIMBUS, there are certain kinds of observations which can be done better by a meteorologist in orbit. The main advantage that a man has, when he can look at the Earth directly instead of through a TV link, is in discrimination. He can direct his attention to those features of the clouds and atmosphere that seem unusual and interesting. He can use binoculars to study the details, and his eyes may perceive subtle effects not picked up by the camera.

A few specific phenomena that he might look for are, for example, the isolated clouds that appear to be associated with hail-producing tornadoes, the orientation of cumulus cloud tops as indicators of flow and advection of cool or warm air, and (on the night side) the pattern of lightning strokes in squall lines. He may be able to detect noctilucent clouds as he crosses the terminator, and the curious color effects on the horizon that astronauts have already noticed can be studied more carefully. These are some of the things we can think of now. There will undoubtedly be many other things spotted by an alert observer-meteorologist looking down from space.

B. Conclusions and Recommendations

The Meteorological Working Group concluded that meteorological research can be furthered by a trained meteorologist making observations from an orbiting station, and recommends that a meteorologist be made a part of the crew of a future manned orbiting space observatory.

The Gemini crew selection is rather far along, and it appears to be impractical to include a professional meteorologist among them at this time (unless, by chance, one of the test pilots selected happens to also be a meteorologist). However, it is recommended that some small number of Gemini pilots be given special training in meteorology and the use of meteorological satellite data to prepare them as competent observers, with the expectation that they will be able to make use of their time on orbit to make valuable meteorological observations. This training should be with the U. S. Weather Bureau's Meteorological Satellite Laboratory and certain designated universities and research organizations where meteorological satellite research is going on. In order to be of much use at least three months should be spent in such training, or as much time as can be spared from the other parts of the Gemini pilot training course.

Appendix III

Principal participants in the Working Group on Man as a Scientist in Space Exploration included the following:

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R. Galambos - Vice Chairman
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Allan H. Brown
Loren D. Carlson
Frank W. Casey
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Von R. Eshleman
John Firor
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Chapter Twelve

NASA/UNIVERSITY RELATIONSHIPS*

I. Introduction

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 (hereafter called universities) 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 institutions of higher learning in the country have within their educational custody some 3,000,000 young men and women. It is from these educational ranks that future staffing of the national effort in space science and technology will come. Accordingly, although the quality of education at all levels (but more particularly at the collegiate and post-graduate levels) is of fundamental importance to the national competence, we have directed our attention particularly to the level of graduate education as the one most crucial to the next decade in space science and technology.

From the standpoint of actual effort and expenditures, the NASA program is dominantly a technological one and is concentrated in industry and in the federal establishment. Yet the present and projected demand for persons of high scientific, technical, and scholarly competence is so great that the impact on higher education is potentially enormous. Moreover, the opportunities for developing new fundamental knowledge and technical applications may very well equal or exceed those which have existed in the atomic and nuclear physics fields during the past thirty years.

It is for the above reasons that a vigorous academic program in all appropriate aspects of the space endeavor must be developed. Such a program must enjoy a

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* This report is based to an important extent on a draft report prepared for the Space Science Board by an ad hoc committee composed of James A. Van Allen, Chairman, Lloyd V. Berkner, Edward B. Espenshade, James Gilluly, James G. Harlow, Joseph C. Morris, and Colin S. Pittendrigh. See Appendix VI for list of participants in Working Group on NASA/University Relationships.

**See Appendix I to this report.

viable relationship to that of the federal establishment itself; but it is of utmost importance that it preserve the essential virtues of universities, viz., a devotion to scholarly and scientific inquiry, a primary concern for the guidance and education of students, full freedom of discussion and publication, and essential autonomy in the formulation of research objectives and of programs of work directed toward such objectives.

II. Current NASA Universities Program

The existing relationship of NASA to the academic community consists of a diversity of research contracts, research grants, training grants for graduate students, and facilities grants to universities engaged in space science research. Also there are many persons from university staffs who act as consultants on special matters.

Much of the present program in universities is traceable directly to their pre-NASA activities, especially those supported during the International Geophysical Year by the National Science Foundation, and by the Office of Naval Research and other agencies of the Department of Defense.

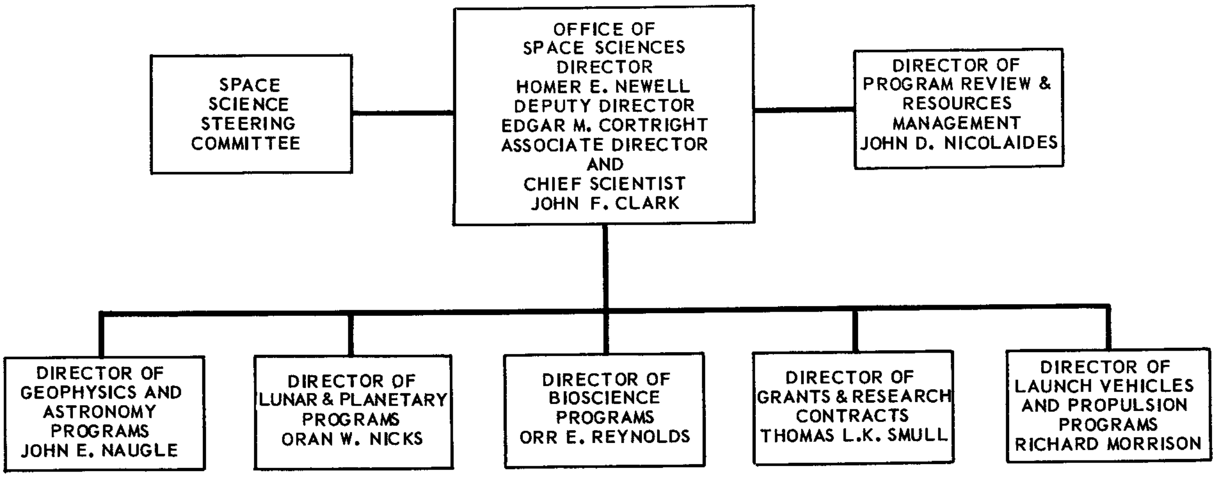
The primary initiative for the existing program for space science in universities has rested with the universities themselves. NASA has, however, reacted with sympathetic attention to the representations of university scientists and administrators. The August, 1961, conference of consultants on NASA/university relationships was a significant milestone in the direction of broader planning.

On September 24, 1961, NASA announced major organizational changes keyed to the nation's accelerated civilian space program, and to its three specific national objectives: (1) scientific study of the space environment and celestial bodies to gain new scientific knowledge; (2) early application of Earth satellites and of space research and technology to immediate use for human benefit; and (3) the exploration of space by man. Five major offices were established: the Office of Space Sciences, the Office of Applications, the Office of Manned Space Flight Programs, the Office of Advanced Research and Technology, and the Office of Tracking and Data Acquisition.

The Office of Grants and Research Contracts, which is the unit that serves as a focal point for NASA relationships with educational institutions,was placed in the Office of Space Sciences under this new organization. Although located in this particular office, it coordinates the NASA/university relationships of all the NASA Program Offices.

The Office of Space Sciences is organized around four major programs, in all of which there is strong university participation: Biosciences, Geophysics and Astronomy, Lunar and Planetary Programs, and Launch Vehicles and Propulsion. Other details of this Office are shown in the organization chart on the following page.

Responding in part to recommendations made by the working groups mentioned in the organization chart, and in part to views expressed by their individual members, NASA is endeavoring to enhance the university contribution to its program by support of research, training, and facilities. Forty million dollars of FY 62 funds and



something in the neighborhood of $100 million of FY 63 funds will be devoted to such activities. These figures are small in the context of NASA's total budget. Moreover, a fraction of the funds, though administered by universities, is not expended on university campuses but is committed by purchase and subcontract for the development of flight equipment, etc. It should be understood further that, for those university programs which involve flight experiments, NASA must provide, within its own budget, substantial sums of money for the purchase of vehicles, conduct of flight operations, operation of telemetry and tracking stations, etc. The payload development costs are typically about 10% of the over-all costs of satellite and space probe missions and about 25% of the over-all costs of sounding-rocket missions.

NASA has affirmed its intention to encourage and support broad interdisciplinary approaches to space problems. The initial program has been concentrated in major universities of demonstrated research achievement. A deliberate attempt will be made in the future to broaden the program to include other universities which are of lesser eminence in space research but which have good potential for growth.

The various categories of NASA's university program are described in somewhat more detail as follows.

A. Research

This category includes grants and research contracts for specific, mutually agreed objectives, often for the development of observational equipment on a specified schedule for scheduled flights, e.g., development and construction of a magnetometer for Explorer XII. It is planned to continue and expand this program.

Also included in this category are grants for the support or assistance of fundamental research in a scientific or technical area, often an interdisciplinary one, which is related to NASA's mission as interpreted broadly. The arrangements for such grants are adapted to a wide variety of needs in many institutional settings.

In this category, NASA is dealing with about 70 universities at present; it expects to increase this number to about 200 in coming years. All grants and contracts must be for work related to NASA's legal mission. But this mission is one of great breadth and even includes social, economic, political, legal, and international implications of peaceful space activities for human affairs. A listing of "Active Grants and Contracts" is issued periodically by the Office of Grants and Research Contracts of NASA and is available on request.

B. Training

NASA has selected ten universities (University of Chicago, Georgia Institute of Technology, University of Maryland, University of Michigan, University of Minnesota, Rensselaer Polytechnic Institute, Rice University, State University of Iowa, Agricultural and Mechanical College of Texas, and University of California in Los Angeles) at which to begin a traineeship program in the fall of 1962. The criteria employed in the selection of the initial list were: currently active space science programs, geographical distribution, and/or nearness to NASA research centers. The budgeted $2 million for FY 62 will support ten traineeships at each of the ten universities for three years. The basic stipend is $2400 for 12 months, given directly to the student, and $1000 given to the university to be administered by it as it deems necessary for dependency allowance, escalation, etc. In addition, a sum of money is to be given to the university for its out-of-pocket expenses; the amount of this sum has not been determined yet and will depend on experience in the early stages of the program. As a matter of NASA policy, the program is restricted in application to U.S. citizens except in unusual cases. There are no oath or affidavit requirements.

The traineeships are for predoctoral study in space-related fields in the physical, engineering, and life sciences. Responsibility for selection of the trainees is placed in the hands of the Dean of the Graduate School at each university. Since NASA recognizes the need for continuing support until the degree work is completed, the money is made available in three-year blocks (in exceptional cases, a fourth year would be approved), to be paid to the student on a year-to-year basis if his progress is satisfactory. NASA does not require that trainees commit themselves to NASA. Rather it views this program as its contribution to the national pool of scientific talent, from which it expects to draw its share of manpower if it maintains an attractive and creative space science program. The traineeships are the equivalent of graduate fellowships in the usual meaning of the latter term.

In FY 63, it is planned to increase the program to 700 to 1000 predoctoral traineeships in a much expanded list of universities.* A program of support at the undergraduate level is also being considered, though its nature and scope have not yet been determined.

NASA also has a program of postdoctoral associateships, which first began with a sum of about $650,000 to support about 50 resident research associateships. Originally tenure of these associateships was limited to the Goddard Space Flight Center; now the program has been broadened to include the Marshall, Ames, Lewis, Langley, and Edwards centers, and the Jet Propulsion Laboratory. NASA expects to extend the program still further to include tenure at universities as well. Residence is normally for one year.

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*NASA policies and procedures for administering training grants may be found in Appendix II.

C. Facilities

Since August, 1961, NASA has been authorized to make facilities grants to universities.* Since this authorization came too late to permit proper budgeting for FY 62, only a small pilot program has been possible. A substantial expansion in FY 63 is planned. NASA's facility legislation is broader than that of other federal agencies in that matching contributions by the grantee are not required. In lieu thereof, a substantial long-term commitment to space research is required. However, if a NASA facilities grant is to be combined with grants from one or more other federal agencies whose legislation does require matching funds, then the grantee institution must match the sum of the federal grants with funds from nonfederal sources (state or private).

NASA can, and does, when the Administrator has made a determination that it is in the best interests of the government, transfer title of facilities constructed with its grants to the grantee.

In general NASA regards facilities grants as for "bricks and mortar," not for research equipment. (Research equipment may be provided by research contracts and research grants.) Its policy is to provide facilities grants only to such universities as have active research programs whose further development is significantly retarded by lack of working space. NASA has turned down proposals for the creation of new national or regional research centers, principally because of its wish to strengthen student-training, degree-granting universities rather than to weaken them. NASA feels that the existing system of major research centers is already adequate for the activities appropriate to such organizations.

In September 1962, facilities grants were made to the following: University of Chicago, University of California in Berkeley, Rensselaer Polytechnic Institute, Stanford University, and State University of Iowa.

Within the Office of Space Sciences, the Office of Research Grants and Contracts is responsible for the processing of all unsolicited proposals submitted to NASA, regardless of subject matter, and for policies, procedures, and business and working relationships vis-a-vis the universities. All NASA grants are handled by this office as well as about ninety per cent of the research contracts with universities. Review of proposals, although not carried out by this office, is almost entirely an in-house function, the biosciences being the principal exception. Generally speaking, NASA's research centers evaluate the proposals, although they do not have the power of final decision; part of NASA's decision-making process necessarily involves evaluation of the evaluators.

The problem of personnel for management of NASA's university programs is a severe one, although at present the problem is principally one of lack of billets. The Office of Grants and Research Contracts now has a staff of four men working on the sustaining university program. To date, efforts to recruit a group of university people to serve as institutional liaison officers between NASA headquarters and the universities have met with no success.

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*The policies and procedures governing NASA facilities grants may be found in Appendix III.

III. Comment and Evaluation

A. Graduate Training and Research

A marked expansion of the existing university program in space science must be achieved if the program is to meet its objective of providing an adequate training ground for the large number of persons of high competence required by the national aspirations in space science and technology. At some point in this expansion there may arise complex questions of a social and economic nature as well as of an educational nature. The central problem in achieving any national objective of this sort seems to be the great chasm between the simultaneous national devotion to highly technological objectives on the one hand and to a completely permissive attitude toward choice of educational field and personal career objectives on the other hand. It is clear that the reconciliation of these totally divergent views is a matter of great difficulty and scope.* It must be done in such a way that the essential virtues of universities are maintained, viz., the devotion to scholarly inquiry, to the development of students' minds, and to the dissemination of knowledge. Imperfect as the functioning of universities may be, there is no other segment of our culture which performs these functions nearly as well, or is likely to.

Both the ad hoc Committee of the Space Science Board and this Study are unanimous in their favorable general impression of the NASA university program. We are particularly impressed by NASA's full awareness of the impact of its mission on the manpower resources of the country, and of its responsibility to replenish those resources commensurately. We are impressed both by NASA's intention to perform its mission in such a manner as to strengthen existing universities, and by its intention to avoid the creation of research institutes of a type that undermine universities.

We recognize that, in order to bolster a neglected field, NASA may in some cases be under great pressure to establish a research institute. This action should not, however, be taken unless it is crystal clear that the need cannot satisfactorily be met through the normal channels of the universities, of industry, and of in-house research.

NASA is to be commended on the breadth of its view regarding not only the natural sciences but also the social sciences and the humanities as areas of learning that impinge in one way or another on its broad mission.

Turning now to more specific matters, the procedures for the review and assessment of proposals are matters requiring the continuous attention of responsible persons both within and outside NASA in order to assure that the resulting program of grants and contracts is one of high quality and vigor. By avoiding dependence on a ponderous procedure for the review of all proposals by outside panels, NASA is often able to act on proposals with commendable promptness. But it is to be hoped that, under a dominantly in-house review procedure, proposals for broad and imaginative undertakings will not suffer in comparison with those offering quick results related to a recognized technical or operational problem.

The three recently added items in NASA's program -- "area" research grants, training, and facilities -- constitute broadly conceived programs for support of

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*See Appendix IV on Manpower and Recruitment.

institutions rather than of specific projects. We endorse these programs full-heartedly and expect them to set the pattern for the future evolution of federal support to higher education. Nonetheless, the actual operation of such programs during the experimental period must be watched closely to avoid the creation of large university research establishments which do not regard the mental development of students as their primary role and obligation.

A good feature of the program for training grants is that the funds available will be placed at the discretion of the Dean of the Graduate School. The pilot program of ten traineeships at each of ten universities may be expected to yield up to 100 space-oriented Ph.D.'s. It is probable that a program of ten times this magnitude can be assimilated without serious dislocation of other graduate fellowship programs (NSF, NIH, NRC).*

> RECOMMENDATION: In view of the necessity for developing a vigorous academic program in all aspects of the space endeavor, it is recommended that NASA pursue its present policy towards university research at the graduate level and extend application of the policy to as many universities as possible, both large and small. Full approval is given to the programs of research grants, research contracts, training grants and facilities grants. Broadly conceived research grants not aimed at specific projects are regarded as the life blood of a vigorous university research program.

B. Pregraduate Training

It is noted that NASA is considering the possibility of extending its cultivation of the manpower pool back into the all-important undergraduate and even high school years.* Among the ways in which this can be done most effectively is the provision, through research grants and contracts, of funds for undergraduate research assistantships both in term and in the summer. Exposure to real investigation is surely one of the strongest tools available for attracting talented students into work at the graduate level. Furthermore, the summer program for selected undergraduates currently in operation at Columbia University should be greatly extended in scope and should be operated at a number of locations.

> RECOMMENDATION: It is recommended that NASA extend the scope of summer programs for selected undergraduates and provide funds, through research grants and contracts, for undergraduate research assistantships.

C. Postgraduate and Faculty Training

The present program in which postgraduate fellows must work at NASA facilities has been a useful device in the early development of the centers -- profitable alike to the fellows and the centers. However, it is clear that a mature NASA program must extend a postdoctoral program to the university community in general. Postdoctoral fellowships give matured students the opportunity for quiet reflection and research

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*See Appendix IV on Manpower and Recruitment.

during their most creative years. Moreover postdoctoral fellowships play an important role in attracting people into academic life and in permitting faculties to "try out" recent Ph.D.'s with a minimum of commitment by the university.

Certain pitfalls must, however, be avoided. Postdoctoral fellowships in universities have the disadvantage of relaxing the pressure on students and institutions to complete the traditional education process by the age of 25. At present, people who ultimately obtain Ph.D. degrees dissipate a good deal of time in the course of their education. The progress of these people through the elementary and high school levels is nearly always unnecessarily slow, with the result that they are still obtaining in the underclass years of college the educational experience that they could, in principle, have obtained in high school. Moreover, the six years that nearly always elapse between the baccalaureate and doctorate degrees involves about two years spent completing an undergraduate type of education, two years on extracurricular activities (including teaching), and two years of genuine graduate education based on original thinking. Even so, the six-year period is often a "rat race" that makes the student a mere specialist and fails to develop his mind to the point where he can tackle with confidence whatever is novel in life.

To cure these shortcomings the concept of postdoctoral fellowships has been developed, and their undoubted success is a measure of the failure of the traditional educational system to achieve its objectives. To what extent should the nation accept this failure as incurable and go even more heavily into postdoctoral fellowships? How much better will the nation be if, in ten or fifteen years, we have obtained numerically all the Ph.D.'s that we now think we will need, but their educational level is merely that appropriate to a glorified master's program, and the genuine Ph.D.'s are the smaller number of people who have had postdoctoral fellowships?

If NASA is to provide postdoctoral fellowships in the universities, it should do so with specific safeguards. Since doctoral programs are sometimes advocated (and even exist) that do not require original work, a specific requirement for a minimum amount of prior original work should be placed upon the award of a postdoctoral fellowship, and this should not be less than that customary for the Ph.D. degree in the arts and sciences areas of the top twenty universities. Moreover, postdoctoral fellowships should be awarded for not more than two years, should not normally be renewable, should not be tenable in the university where the fellow has just completed a Ph.D. degree, and should be awarded preferentially to people who have been gainfully employed for at least one year subsequent to taking the Ph.D. degree.

A postdoctoral fellowship program, besides involving the danger of relaxing the pressure on both students and institutions to complete the traditional educational program by the age of 25, fails to solve other important post-Ph.D. problems in the universities. Young faculty members in the universities need time for serious thought before regular sabbatical arrangements (when they exist) come into play. Furthermore, there is a need for "retreading" faculty in some institutions. The normal arrangements for research contracts and grants greatly facilitate the development of graduate education, and hence also of undergraduate education. Nevertheless, they do not, by themselves, ensure initiation of these developments in colleges where graduate education is almost nonexistent and undergraduate education is somewhat obsolescent. These difficulties would be alleviated if it were possible for a professor, particularly an assistant professor, to spend a period (such as an academic year with two adjacent summers) on full-time research in space science either at his own university or at some other institution (such as another university, a graduate research center, a NASA research center, or a research institute).

> RECOMMENDATION: It is therefore recommended that NASA look with favor on research proposals that encourage faculty members, particularly assistant professors, to spend a period on full-time research in space science. It is further recommended that NASA develop a program of postdoctoral fellowships subject to safeguards that militate against the program being used to replace the requirement of original research for the Ph.D. degree or to stretch out still further the number of years required for developing students' minds.

D. Research in Systems Engineering

It is noted that, in spite of the fact that space engineering rather than space science accounts for by far the largest part of NASA's budget, NASA does not seem to have developed a recognizable policy toward engineering education, probably because engineering education itself is in a state of flux.

Many of today's problems in engineering arise from the difficulty experienced in understanding, analyzing, synthesizing, controlling, and operating large systems. Some of NASA's most severe problems fall into this category. For example, a Surveyor program technically represents a very large system by itself, and even this can be considered as an element in an even larger system. The intricate interplay of technical decisions, schedules, and cost which must be interwoven with the total program of unmanned exploration which in turn is part of the larger national effort toward manned and unmanned space exploration indicates the complexity of the situation. The difficulties that arise, in the presentation and appraisal of alternative decisions, in the early identification of crucial technical problems, in the rational synthesis of the system most likely to be successful, and in the balanced allocation of emphasis and resources, seem to be particularly characteristic of large undertakings of high innovative content such as those of NASA.

Studies of systems in recent years have been undertaken very effectively by electrical engineers, and particularly by electrical engineers in universities. This has been associated with the increasing complexity that has developed in electric circuits, electric networks, and electronics. Indeed, the part of modern electrical engineering that cannot be satisfactorily classified as "electrical science" is almost entirely concerned with the study of "electrical systems." Whereas electrical science has its basis mainly in physics, the study of electrical systems has its basis mainly in applied mathematics. Electrical engineers have consciously developed mathematics as a vehicle of thought in complex engineering problems, particularly for the study of electrical systems. In so doing they and others have realized that the methods of thought that have consequently been developed are equally applicable to mechanical systems, to electro-mechanical systems, to fluid systems and indeed to a wide range of situations in which "elements" are interconnected to form "systems." It is out of these studies that the modern concept of systems engineering is developing.

It is important to emphasize that, while NASA would generally be safe in supporting research in systems engineering, it would need to exercise much greater vigilance in moving toward support of the teaching of systems engineering at this time. The problem is that we do not understand how to think effectively about large systems and that in consequence we do not have a body of knowledge to teach; it is not that we merely have to find a way of teaching it to undergraduates. Some general questions that require exploration are:

(i) Is there a genuinely basic science of systems engineering? Could a sequence of thought begin perhaps with topology and continue successfully -- steering clear of dilettante "survey" courses on the one hand and rapidly obsolescent "design" courses on the other hand?

(ii) The electrical and communication sciences seem to have been able to develop a spectrum of durable and admirable analysis and synthesis courses. Can synthesis courses of equivalent longevity and quality be invented for other technologies?

(iii) To what extent is the "case history" technique used in industry essential in the teaching of systems engineering?

What is important is that research in systems engineering be supported in the universities, leading to broad investigations exploring the characteristics of large systems and encouraging experimentation by engineering faculties in this field by the case history technique, by seeking a common body of understanding, or in other appropriate ways.

> RECOMMENDATION: It is recommended that, in view of the importance to NASA of understanding the behavior of large systems, particularly in engineering, NASA support appropriate aspects of university research in systems engineering.

E. Technician Training

Education and training for technician occupations is generally recognized as a critical element in the educational program. Industry has more of a problem finding good technicians than in finding good junior engineers, and the problem promises to become more acute. In Southern California, where much effort has gone into analyzing and solving the problem, it has been established to be very real and very serious, but also quite capable of solution by a cooperative industry-education effort. In fact, major quantitative and qualitative improvements have already been accomplished in that area. Appendix V deals with the subject in greater detail.

> RECOMMENDATION: It is recommended that NASA familiarize itself with the problem of technician education and training and its potential solutions, that NASA stimulate industry-education cooperation in those metropolitan areas where such cooperation does not now exist, and that NASA support existing cooperative efforts as appropriate.

Appendix I

The legal basis for a formal working association between NASA and universities may be found in the following excerpts from the National Aeronautics and Space Act of 1958, as amended in 1961.

"TITLE I - SHORT TITLE, DECLARATION OF POLICY, AND DEFINITIONS

Declaration of Policy and Purpose

Sec. 102. . . .

(c) The aeronautical and space activities of the United States shall be conducted so as to contribute materially to one or more of the following objectives:

(1) The expansion of human knowledge of phenomena in the atmosphere and space;

(2) The improvement of the usefulness, performance, speed, safety, and efficiency of aeronautical and space vehicles;

(3) The development and operation of vehicles capable of carrying instruments, equipment, supplies, and living organisms through space;

(4) The establishment of long-range studies of the potential benefits to be gained from, the opportunities for, and the problems involved in the utilization of aeronautical and space activities for peaceful and scientific purposes;

(5) The preservation of the role of the United States as a leader in aeronautical and space science and technology and in the application thereof to the conduct of peaceful activities within and outside the atmosphere;

(6) The making available to agencies directly concerned with national defense of discoveries that have military value or significance, and the furnishing by such agencies, to the civilian agency established to direct and control nonmilitary aeronautical and space activities, of information as to discoveries which have value or significance to that agency;

(7) Cooperation by the United States with other nations and groups of nations in work done pursuant to this Act and in the peaceful application of the results thereof; and

(8) The most effective utilization of the scientific and engineering resources of the United States, with close cooperation among all interested agencies of the United States in order to avoid unnecessary duplication of effort, facilities, and equipment.

. . .

Functions of the Administration

Sec. 203. (a) The Administration, in order to carry out the purpose of this Act, shall --

(1) plan, direct, and conduct aeronautical and space activities;

(2) arrange for participation by the scientific community in planning scientific measurements and observations to be made through use of aeronautical and space vehicles, and conduct or arrange for the conduct of such measurements and observations; and

(3) provide for the widest practicable and appropriate dissemination of information concerning its activities and the results thereof.

(b) In the performance of its functions the Administration is authorized --

. . .

(5) without regard to section 3648 of the Revised Statutes, as amended (31 U.S.C. 529), to enter into and perform such contracts, leases, cooperative agreements, or other transactions as may be necessary in the conduct of its work and on such terms as it may deem appropriate, with any agency or instrumentality of the United States, or with any State, Territory, or possession, or with any political subdivision thereof, or with any person, firm, association, corporation, or educational institution. To the maximum extent practicable and consistent with the accomplishment of the purpose of this Act, such contracts, leases, agreements, and other transactions shall be allocated by the Administrator in a manner which will enable small-business concerns to participate equitably and proportionately in the conduct of the work of the Administration.

. . .

(7) to appoint such advisory committees as may be appropriate for purposes of consultation and advice to the Administration in the performance of its functions;

(8) to establish within the Administration such offices and procedures as may be appropriate to provide for the greatest possible coordination of its activities under this Act with related scientific and other activities being carried on by other public and private agencies and organizations;

. . .

Appendix II

NASA Policies and Procedures for Administering Training Grants

(Predoctoral)

Purpose:

The purpose of this program is to increase the supply of scientists and engineers in space-related science and technology in order to meet the growing needs of the government's space research program.

Terms of Predoctoral Training Grants

The selection of recipients of traineeships is the responsibility of the institutions receiving NASA predoctoral training grants. No amount of the stipend shall be contingent upon or represent payment for teaching, research, or other services in the nature of part-time employment on the part of the recipient graduate student unless teaching, research, or other services are required by the grantee of all degree candidates whether or not recipients of stipends or similar awards. Recipients of traineeships under these grants shall not be permitted to accept concurrently other educational assistance in such form as grants, fellowships, or assistantships.

The awards are to be made to predoctoral graduate students of unusual promise with an interest in the space sciences. The awards may be made either to graduate students already engaged in a course of study leading to the doctorate or to students about to undertake such a course of study. In making awards every effort should be made to choose individuals who show promise of being able to complete the doctorate within a three-year period.

In selecting trainees, the awarding institution is expected to consider the candidate's academic records, personal qualifications and research and study plan for the training period. Recipients should be citizens of the United States. However, in exceptional cases when the interests of NASA will be served and after prior approval from NASA, noncitizens may receive awards. Individuals receiving support under these grants do not incur thereby an obligation to the Government of the United States. However, the objectives of this program will be best served if such individuals engage in research and teaching in science or engineering following completion of their training, and they should be encouraged to do so.

Continuity of support is an essential ingredient of this program. Although the traineeships are to be reviewed each year by the institution and awarded for one year of graduate study, it is anticipated that a student who qualifies as a NASA trainee may look forward to renewal of his traineeship for a second and third year if he maintains his qualifications at a level acceptable to the institution. Only in exceptional cases and after approval by NASA may awards be made to the same individual for more than three years. In case a trainee fails to qualify for a second or third year extension and another individual is awarded a traineeship in his stead, the student should be apprised at the outset that the duration of his traineeship may be less than the customary three years.

The annual stipend to the student is $2,400. Those electing a tenure of nine months will receive 9/12's of the annual stipend. Trainees should be encouraged to pursue their graduate study on a full-year basis.

In addition to the $2,400, the trainee may receive allowances at an annual rate of up to $1,000, to be furnished, as necessary, on an individual basis in accordance with the grantee's established policy. The balance of the funds granted may be used to cover reasonable institutional expenses attributable to training, including but not limited to tuition, laboratory fees, equipment specifically required for the performance of student research and costs incurred in developing new curricula.

Reports

Annual reports are required on the fields of endeavor and the basis for selection of new trainees, as well as the status of individuals currently in training and those who have completed training within the report period, together with a statement of expenditures under the program.

Informal reports from grantees to NASA, when appropriate, are encouraged.

Publications

Publications may on occasion result from research carried on by graduate students supported under this program. Twenty-five reprints or copies of such publications should be made available to NASA.

Equipment

Ordinarily, title to equipment purchased with grant funds will be permitted to vest in the grantee. However, NASA may require transfer of title to such research equipment in particular cases. NASA should be informed when items of research equipment costing in excess of one thousand dollars ($1,000.00) are acquired under a grant.

Excess Payments

Grant funds not committed prior to the conclusion of the training program shall be considered excess payments and shall be refunded by check made payable to the National Aeronautics and Space Administration.

Accounting Procedures and Audit

It is expected that records will be maintained for each grant that will permit preparation of the required reports and make possible the determination that funds are used for the purposes designated.

Expenditures under NASA grants are subject to inspection and audit by representatives of NASA and the General Accounting Office during the course of the program and for three years thereafter.

NASA Administrative Office

For purposes of administering this program, the contact within NASA for the grantee will be the Director of Grants and Research Contracts, Code SC, Office of Space Sciences, National Aeronautics and Space Administration, Washington 25, D. C.

Appendix III

Procedures for Administering Training Grants

Facilities Grant NsG(F)______

The National Aeronautics and Space Administration (NASA) hereby grants the sum of $______________________ to ______________________________ for ____ ______________________________ further described in the formal proposal submitted by the Grantee and identified as ____________________. Of this amount not to exceed $______________________ may be spent for necessary architectural and engineering studies preparatory to construction. Further, actual construction will not be initiated prior to NASA approval of the plans and specifications for technical feasibility and reasonableness of cost. This grant is for a period of approximately __________ year(s), beginning about

This grant will be administered in accordance with NASA policy and procedures for facilities grants as set forth in the "Policy Governing NASA Facilities Grants" and the "NASA Procedures for Administration of Facilities Grants," attached hereto and incorporated herein by reference, and in conformity with other agreements between NASA and the Grantee relating to this grant.

It is a condition of this grant that it may be revoked in whole or in part by NASA after consultation with the Grantee. In the event of revocation, the Grantee shall refund to NASA any unexpended funds issued by this grant, except such portion thereof as may be required by the Grantee to meet commitments which, in the judgment of NASA and the Grantee, had become firm prior to the effective date of revocation and are otherwise appropriate.

It is contemplated that this grant will be paid in accordance with the following schedule:

Date	Amount

After the submission by the Grantee and approval by NASA of the final architectural drawings and specifications, the balance of funds hereby granted will be paid in accordance with the payment schedule to be established at such time.

The United States of America

By ______________________________
T.L.K. Smull, Director
Office of Grants and Research Contracts
(Name and Title)

NATIONAL AERONAUTICS and SPACE
ADMINISTRATION

ACCEPTANCE

______________________________ accepts this facilities grant.
(Grantee)

____
(Date Accepted)

By ______________________________
(Signature of Authorized Official)

____
(Name and Title)

May, 18, 1962

Policy Governing NASA Facilities Grants

1. General

Grants will be made by NASA to non-profit institutions of higher education, or to non-profit organizations whose primary purpose is the conduct of scientific research, for purchase or construction of additional research facilities urgently needed to conduct research in space-related science and technology. These grants will relieve the critical shortage of facilities for groups now doing research pertinent to the NASA mission. Further, they will make possible the establishment and development of new research groups in combinations of scientific disciplines relevant to the objectives of NASA. This expansion of the national base of research facilities and competent scientific manpower will permit NASA to contribute appropriately to the preservation of the role of the United States as a leader in aeronautical and space science and technology. Authority for this program is the National Aeronautics and Space Act of 1958 (Public Law 85-568) and the NASA/Appropriation Authorization for 1961 (Public Law 87-98). Responsibility for this program is assigned to the Office of Grants and Research Contracts. Other NASA units will render appropriate assistance as required.

2. Specific Purposes

A facilities grant will be made to a qualifying institution for acquisition of laboratories and other research facilities which are devoted primarily to research in space-related science and technology. Alternatively, this type of grant may be used to finance expansions of existing accommodations. Allowances for fixed equipment essential to the operation of such research facilities may also be included in these grants.

A facilities grant ordinarily will not include allowances for land acquisition or for the laboratories, lecture halls, library space, conference rooms and demonstration areas which are used for instructional purposes. Nor is it the intention that these grants be used to purchase furniture, office supplies and equipment, books, expendable office and laboratory supplies, or specialized laboratory equipment of limited use. Allowances for specialized equipment and expendable office and laboratory supplies may be included in conventional project grants or contracts.

Conditions on the use of NASA grant funds may be established on an individual case basis.

3. Ownership Considerations

Grants made under this program will be for dollar amounts determined by NASA to be appropriate in each instance, up to the full cost of the proposed facilities.

Public Law 98, 87th Congress, provides that title to facilities constructed with NASA research and development funds shall vest with the United States, unless the Administrator of the National Aeronautics and Space Administration determines that the interests of the national space program will best be served by vesting title in the Grantee. Determination to vest title in the Grantee will be made by the Administrator of the National Aeronautics and Space Administration on an individual case basis. The Grantee must own the site on which the facilities are to be constructed or have the right to control it for at least a ten year period.

Adequate maintenance and operation of the facilities pursuant to their intended use shall be the responsibility of the Grantee.

4. Facilities Planning

Institutions submitting proposals involving major construction may be unable to supply complete specifications or firm cost figures until detailed architectural and engineering studies have been made. When the institution concerned has no practical means to finance the necessary studies, NASA will consider issuing a facilities planning grant of a specified amount. Such a grant will be used to finance necessary architectural and engineering studies, preliminary estimates, and other costs necessary to the preparation of a formal proposal. The award of a facilities planning grant will be evidence of NASA's intent to make a future facilities grant if funds are available and the studies prove the proposal technically feasible and financially reasonable. A planning grant will not constitute a legal commitment for any funds beyond its own terms.

5. Selection Criteria

Major selection criteria will include:

a. The relative importance to the national space program of the particular field or combination of fields of research for which the facilities will be used;

b. The demonstrable competence, achievements, and potential further contribution to the space program of the scientific staff or group for whose immediate use the facilities are intended;

c. The nature or extent of the proposing institution's commitment to work in space-related science and technology, the quality of its supporting facilities and staff, and its willingness and ability to finance by other means any additional staff or equipment needed for maximum utilization of the proposed facilities;

d. The technical soundness and reasonableness of cost of the proposed facilities;

e. The urgency of need for the facilities at the proposing institution; and

f. Such additional criteria as may be appropriate.

6. Administration

The Office of Grants and Research Contracts will notify the Grantee and issue and administer the grant when a decision to award a facilities grant has been made. Administration will be in accordance with "Procedures for Administration of NASA Facilities Grants."

7. Compliance with Existing Laws

The Grantee and contractors shall comply with all applicable Federal, state and local laws.

June 18, 1962

Procedures for Administration of NASA Facilities Grants

1. Performance of Construction

Construction of facilities supported by NASA grants shall be in accordance with the "Policy Governing NASA Facilities Grants." Unless a specific exception is requested by the Grantee and approved by NASA, competitive bidding by formal advertising shall be obtained for all construction approved under the NASA facility grant program and the contract awarded on a fixed price basis to the lowest responsible bidder. Performance of the construction shall not be by the Grantee's own work force.

2. Proposals

A formal proposal should include detailed information about the site and design of the proposed facilities, cost and schedule of acquisition and necessary financing arrangements. The proposal should indicate the types of research for which the facilities will be used and the name and background of individuals available for the scientific staff. Additionally, the proposal should include a statement indicating the extent to which the facilities are likely to increase the institution's capacity for space-related research, and any other information required by NASA or important in evaluating the relative contribution the proposed facilities would make to the national space effort.

Proposals will be evaluated by the Office of Grants and Research Contracts with the assistance of such other NASA units as may be appropriate. NASA staff teams will make at least one site visit. The Office of Grants and Research Contracts will be responsible for any necessary coordination with other federal agencies.

3. Federal Labor Regulations

a. Construction supported in whole or in part by NASA funds is subject to the Copeland and Anti-Kickback Acts, 18 U.S.C. 874 and 40 U.S.C. 276c. Pursuant to Department of Labor regulations implementing these Acts, the Grantee and/or its contractors and subcontractors are required to submit weekly reports on prescribed Department of Labor forms (29 C.F.R. part 3). The Grantee is designated as NASA's representative to receive such reports at the site of the work, for purposes of 29 C.F.R. 3.4(a). The reports shall be transmitted by the Grantee to NASA as directed by the Office of Grants and Research Contracts.

b. Construction supported in whole or in part by NASA funds may also be subject to other Federal labor requirements. The Grantee will be required to insert in each construction contract such additional labor clauses as shall be specified by the Office of Grants and Research Contracts. The Grantee shall give NASA at least sixty (60) calendar days advance notice of its intent to advertise the specifications for a construction contract.

4. Accounting and Audit

It is expected that records will be maintained for each grant, in accordance with generally accepted accounting practices, which will permit preparation of the required reports, satisfy legal accountability requirements and make possible the determination that funds were used for the purpose designated.

All accounting records relating to expenditures under NASA grants are subject to inspection and audit by representatives of NASA and the General Accounting Office for the duration of the grant and for three years thereafter.

5. Payments

The Director of Grants and Research Contracts will establish disbursement schedules. Grant funds not committed in connection with the proposed facilities acquisition shall be considered excess payments and shall be refunded by check made payable to the National Aeronautics and Space Administration.

6. Reports

Progress reports of the work performed and an accounting of expended funds shall be given quarterly by the Grantee to NASA. The first report is due three months from the date of the grant. The reports shall be in such reasonable detail and contain such further information as may be reasonably required by NASA.

7. Inspection and Acceptance

Necessary inspections or other reviews will be made to assure that the grant is used for its intended purposes and otherwise to protect the interests of the United States.

Final inspection and supervision during construction of facilities supported in whole or in part by funds granted by NASA shall be by an architect or engineer independent of the Grantee. Acceptance of the facilities from the contractor is the responsibility of the Grantee. NASA is to be formally notified of such final inspection and acceptance, and an inspection may be made by representatives of NASA.

8. Exceptions

Consideration will be given by NASA on an individual case basis to a written request from the Grantee for exception to the foregoing procedures.

9. Construction Contract Clauses

(a) All mechanics and laborers employed or working upon the site of the work, or under the Housing Act of 1949 in the construction or development of the project, will be paid unconditionally and not less often than once a week, and without subsequent deduction or rebate on any amount (except such payroll deductions as are permitted by regulations issued by the Secretary of Labor under the Copeland Act (29 CFR Part 3)), the full amounts due at time of payment computed at wage rates not less than those contained in the wage determination decision of the Secretary of Labor which is attached hereto and made a part hereof, regardless of any contractual relationships which may be alleged to exist between the contractor or subcontractor and such laborers and mechanics; and the wage determination decision shall be posted by the contractor at the site of the work in a prominent place where it can be easily seen by the workers.

(Subparagraph (1) amended, 23 F. R. 9672, Dec. 13, 1958)

(b) The National Aeronautics and Space Administration may withhold or cause to be withheld from the contractor so much of the accrued payments or advances as may be considered necessary to pay laborers and mechanics employed by the contractor or any subcontractor on the work the full amount of wages required by the contract. In the event of failure to pay any laborer or mechanic employed or working on the site of the work, or under the Housing Act of 1949 in the construction or development of the project, all or part of the wages required by the contract, the National Aeronautics and Space Administration may, after written notice to the contractor, sponsor, applicant, or owner, take such action as may be necessary to cause the suspension of any further payment, advance, or guarantee of funds until such violations have ceased.

(c) Payroll records will be maintained during the course of the work and preserved for a period of three years thereafter for all laborers and mechanics working at the site of the work, or under the Housing Act of 1949 in the construction or development of the project. Such records will contain the name and address of each such employee, his correct classification, rate of pay, daily and weekly number of hours worked, deductions made and actual wages paid. The contractor will submit weekly a certified copy of all payrolls to the National Aeronautics and Space Administration if the agency is a party to the contract but if the agency is not such a party the contractor will submit the certified payrolls to the applicant, sponsor, or owner, as the case may be, for transmission to the National Aeronautics and Space Administration. The certification will affirm that the payrolls are correct and complete, that the wage rates contained therein are not less than those determined by the Secretary of Labor and that the classifications set forth for each laborer or mechanic conform with the work he performed. The contractor will make his employment records available for inspection by authorized representatives of the National Aeronautics and Space Administration and the Department of Labor, and will permit such representatives to interview employees during working hours on the job.

(d) Apprentices will be permitted to work only under a bona fide apprenticeship program registered with a State Apprenticeship Council which is recognized by the Federal Committee on Apprenticeship, U.S. Department of Labor; or if no such recognized council exists in a State, under a program registered with the Bureau of Apprenticeship, U.S. Department of Labor.

(e) The contractor will comply with the regulations applicable to contractors and subcontractors (29 CFR Part 3, copy of which is attached) issued by the Secretary of Labor pursuant to the Copeland Act, as amended (48 Stat. 948; 62 Stat. 108; 72 Stat. 967; 40 U.S.C. 276c), and any amendments or modifications thereof, will cause appropriate provisions to be inserted in subcontracts to insure compliance therewith by all subcontractors subject thereto, and will be responsible for the submission of statements required of subcontractors thereunder, except as the Secretary of Labor may specifically provide for reasonable limitations, variations, tolerances, and exemptions from the requirements thereof.

(Subparagraph (5) amended, 23 F.R. 9673, Dec. 13, 1958)

(f) The contractor will insert in each of his subcontracts the provisions set forth in stipulations (a), (b), (c), (d), (e) and (g) hereof, and such other stipulations as the National Aeronautics and Space Administration may by appropriate instructions require.

(g) A breach of stipulations (a) through (f) may be grounds for termination of the contract.

(h) No laborer or mechanic doing any part of the work contemplated by this contract, in the employ of the contractor or any subcontractor contracting for any part of said work contemplated, shall be required or permitted to work more than eight hours in any one calendar day upon such work, except upon the condition that compensation is paid to such laborer or mechanic in accordance with the provisions of this article of the contract. The wages of every laborer and mechanic employed by the contractor or any subcontractor engaged in the performance of this contract shall be computed on a basic day rate of eight hours per day and work in excess of eight hours per day is permitted only upon the condition that every such laborer and mechanic shall be compensated for all hours worked in excess of eight hours per day at not less than one and one-half times the basic rate of pay. For each violation of the requirements of this article of the contract a penalty of five dollars shall be imposed upon the contractor for each laborer or mechanic for every calendar day in which such employee is required or permitted to labor more than eight hours upon said work without receiving compensation computed in accordance with this article of the contract, and all penalties thus imposed shall be withheld for the use and benefit of the Government: Provided, that this stipulation shall be subject in all respects to the exceptions and provisions of the Eight Hour Laws as set forth in U.S. Code, title 40, sections 321, 324, 325, 325a, and 326, which relate to hours of labor and compensation for overtime.

Appendix IV

Manpower and Recruitment

Although the proposal by NASA to support a large training program is considered desirable, there is a question whether the program will increase the available trained manpower pool. Will 1000 new graduate fellowships attract new students into graduate study or merely shift the financing among the present population? Unless an expansion of the graduate population occurs, NASA's training programs might develop at the expense of legitimate teaching-assistant needs. There is a lack of agreement on this point and conflicting evidence; but if the NASA graduate training program does not increase the graduate body of scientists and engineers roughly by the numbers envisaged, the ultimate space manpower needs will be met at the expense of other research needs, the college teaching force, and industry.

Berelson* states that there is an undergraduate reservoir of qualified students who for reasons unknown do not now enter graduate study. Consideration should be given to ways of encouraging individuals in this reservoir to do graduate work. It should be noted, however, that although this shift would solve the immediate short-range problem, it depletes the BS-degree work force. The undergraduate program must then be developed further to attract additional students who will follow appropriate programs. The order of magnitude of the expansion necessary is suggested by the following estimates. To produce an extra 1,000 Ph.D.'s we need to produce an extra 21,000 BS candidates (the present number of BS candidates in science fields is 63,600). To produce 21,000 BS candidates, because of attrition, we need to enroll an additional 130,000 students in appropriate areas (current estimated enrollment in needed areas is almost 650,000). Can we attract the needed number of additional qualified high school graduates to enter an appropriate college program?

Before substantial improvement can be expected in the size of the scientifically-oriented student manpower pool, new information must be developed and put to use in one form or another. It certainly can no longer be argued, for example, that science and engineering activities do not receive adequate publicity. According to Berelson,* everyone who wants to enter a graduate school can be admitted somewhere. Career seminars, NSF institutes for able secondary school students, new curricula in science and mathematics, special guidance efforts, and training for counselors -- all these are either operating or moving into operation, and still the enrollments in science and engineering continue to decline.

We need to know much more precisely how young people view science and scientists. We need to know what factors are most powerful in the formation of their career plans. We need to know more accurately when these choices are made. We

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*Bernard Berelson, Graduate Education in the United States, McGraw-Hill Book Company, Inc., New York, 346 pp., 1960.

need to investigate and to map accurately the web of controls which inhibit women's choices of careers in science and engineering. We must find and develop the resources in science and engineering talent among Negroes and other groups which currently are socially disadvantaged. New federal expenditure to increase the scientifically-oriented student manpower pool certainly cannot be expected to produce substantial results unless these and related informational needs are satisfied.

Appendix V

Education and Training for Technician Occupations

The technician occupies a level of employment between craftsman and engineer, which is only now being defined in our educational system. At this level education as well as training is required, with the specialized areas for training changing from year to year with the ramifications of technology. Adequate preparation of technicians requires active cooperation between industry and education within a framework provided by government.

It is generally recognized in industry that qualified technicians are more difficult to find than competent junior scientists and engineers. Not only is there a shortage of qualified technicians, but programs for educating and training technicians are generally inadequate even if they exist. As a result, the situation is becoming more acute. Many capable college graduates are employed in jobs that should be handled by technicians, their professional development is impeded, and the shortage of scientists and engineers is aggravated.

Evidence of the severity of this problem, but also of the possibility of dealing with it, is provided by the experience in one area where the problems was analyzed and corrective measures taken. In Southern California much of industry is technologically oriented, the schools have been forced into dynamic program planning by the rapid population growth, and the state government has generally been sympathetic to educational needs.

The Southern California program is able to draw on national sources of encouragement and support. The Federal Apprenticeship Law, known as the Fitzgerald Act of 1937, was followed by the Shelley-Maloney Labor Standards Act of 1939 which authorized the California Apprenticeship Council. In March, 1962, Congress passed the Manpower Development and Training Act to provide vocational and on-the-job training for unemployed and underemployed persons. This law provides training of two to fifty-two weeks and is to be operative from June 1, 1962, to December 30, 1965. In addition, the National Vocational Guidance Association of the American Personnel and Guidance Association has been concerned with vocational guidance for the last 50 years, and is able to assist local efforts. This supporting law and organization, however, is more concerned with craftsmen than with technicians, and is only useful after industry and education in a community have banded together to face their own problem.

The Southern California Industry-Education Council was formed in July 1957 as a convenient means of bringing industry and education together to deal cooperatively with common problems in education and training. It now represents some 350 companies and all the school districts of Southern California. Affiliated County Councils and their districts carry the organization down to the local level where on selected programs needed by the schools and supportable by the companies involved. The viability of the program rests on active personnel exchanges between the schools and industry; teachers and students meet the technical staff of industry in the classroom and in industrial laboratories.

In the development of improved programs for the education and training of technicians, close cooperation between industry and education is particularly important. Technician jobs must be standardized along with related tests and curricula. Industry must anticipate its needs in the job classifications, make them known to the schools, provide apprentice positions where properly prepared students will be accepted without experience, and complete the training of the student on the job. The educational system must provide teachers, schools, and appropriate curricula, emphasizing education rather than training. Motivation of the student depends in part on the schools, but cannot succeed except in a community where industry and education have joined forces and carried a convincing message to parents and students in the public press.

Education and training for technician positions must be conducted at several levels. Such preparation must be begun at the high school level since some 30 per cent of all students do not continue their formal education beyond high school. More effective preparation of the laboratory technician can be made in junior college with a properly designed curriculum. It is being recognized, however, that this is not necessarily a terminal education but may be continued in a four-year college, if the early curriculum is properly designed.

The Los Angeles City School District in November 1961 completed an intensive study of the San Fernando Valley area, entitled "Education and Training for Technical Occupations." The study was financed cooperatively by industries in the Valley, the Los Angeles City School District and the California Department of Education utilizing federal funds under provisions of Title VIII, Public Law 85-864. The two parts of the report contain more than 350 pages of concentrated information, statistics, thoughts and plans. The San Fernando Valley contains 15 high schools with 32,000 students, two junior colleges with 6000 students, five adult schools with an enrollment of 21,000. Also in the Valley are 837 industrial companies with 114,000 employees, predicted to grow by 1970 to over 300,000 employees. The report thus rests on a basis of solid statistics.

As a quantitative indication of the severity of the problem, industry in the San Fernando Valley estimates that the 200,000 employees it expects to add during the next eight years will be distributed as follows:

Technicians	26%
Skilled craftsmen	23%
Engineers	15%

During this period the educational preparation demanded of technicians is expected to increase markedly, especially in the higher levels of employment.

Part I of the San Fernando Valley Study, issued in July 1961, made a number of recommendations for industry, for the schools and for the community, which can be summarized as follows:

Recommendations for Industry

1. Modernize and extend current training programs.
2. Standardize job classifications and levels of employment.
3. Become more familiar with school programs and curricula, and work with schools to improve the programs.
4. Increase personnel exchange between industry and the schools.
5. Provide guidance and counseling for its emplovees, and programs in orientation, indoctrination, and on-the-job training.
6. Arrange close, detailed coordination with the schools, including participation in educational advisory committees and the Industry-Education Council.

Recommendations for the Schools

1. Actively explore new curricula and relate them to current and future needs of the local community.
2. Standardize programs and curricula to job descriptions established by industry.
3. Relate plant and equipment requirements to the type and scope of education in each area, based on separate budgets.
4. Adopt a comprehensive approach to education providing in a flexible way for both academic and vocational needs.
5. Emphasize adequacy in instruction and objectivity in counseling and guidance.
6. Establish coordination between levels of education so that each leads smoothly into the next, but in itself provides a job capability.
7. Prepare outlines and text materials on the spot during exploratory phases.
8. At every level, offer alternative curricula, including technical curricula, leading to degrees or certificates, and make certain that industry and the public are aware of the school offerings.
9. In general, accept the responsibility for educating all students to serve the community's needs effectively at some level.

Recommendations for the Community

1. Work together to insure that young people do not enter the labor market in competition with the unskilled and under-employed.

2. Accept the fact that job opportunities will be based on aptitude, interest and pertinent education, and that parent-student planning is necessary.

3. Publicize the dependence of salary and promotion on adequate preparation and the fact that industry prefers workers in a higher age bracket with more formal training.

4. Emphasize in particular the employment handicaps of one with only a general high school diploma or even more advanced certificates or degrees of a general or non-pertinent nature.

Part II of the San Fernando Valley Study, issued on November 1, 1961, reported considerable progress with these recommendations. This progress began with the start of the Study and was due to better understanding by industry and education of each other, as well as to better understanding by both of the problem.

A key accomplishment has been the definition of 42 technical classifications and the discovery that they share large common areas of required knowledge in English, mathematics and the physical sciences. As a result, the two-year junior college course devotes one year to these general requirements and then differentiates during the second year, as follows: pre-engineering, design technology, electronics technology, mechanical technology and the physical science specialties. The junior college can educate according to these broad job families. Later industry can train according to narrower job assignments. The broad initial education, however will simplify later changes in job assignment.

A striking example of the tangible benefits that can result from a community effort such as that described above is the Tampa Branch of the Reseda Adult School, an element of the Los Angeles City Schools concerned with electronics training. This branch represented a cooperative effort of the Los Angeles City Schools, Litton Systems, Inc. and the California State Employment Service. During the first year of operations, six instructors directed over 1000 students through the electronics training program. Of these students, 94% have already been employed by 30 companies. Many of these students are now taking more advanced courses in preparation for job up-grading.

The following literature describes in detail this large and effective community attack on education and training for technician occupations. It is available from William J. McCann, Executive Secretary, Southern California Industry-Education Council, 700 State Drive, Exposition Park, Los Angeles 37, California, or from T. Stanley Warburton, Associate Superintendent, Division of College and Adult Education, Los Angeles City School Districts, 450 North Grand Avenue, Los Angeles 12, California.

> "The Utilization of Engineering Technicians in Industry," A Report by the SCIEC Committee on Vocation and Technical Education, March 1960, Chairman: F. A. Dickerhoff, Personnel Manager, Hughes Aircraft Company, Ground Systems Group.
>
> "A Summer Workshop Program for Electronic Instructors," and "A Proposal for an Electronic Technician Training Program," Hughes Aircraft Company, Ground Systems Group, September 1960.

''Education, Training and Retraining for a Changing Technology,'' William J. McCann, Executive Secretary, Southern California Industry-Education Council. (Address delivered at the Governor's Conference on Automation, November 28, 1961.)

''Education and Training for Technical Occupations,'' Donald D. Dauwalder, Survey Director for the Los Angeles City Junior College District.

Part I, July 1961 (166 pages)
Part II, November 1961 (196 pages)

''Reseda Adult School Electronic Training Program: 1961-1962 Year of Opportunity,'' Tampa Branch, Reseda Adult School, Los Angeles City Schools, May 1962.

Appendix VI

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Chapter Thirteen

BLOCK ALLOCATION OF PAYLOAD SPACE*

I. General Considerations

The present program of the Office of Space Sciences of the National Aeronautics and Space Administration involves the launching of both large and small spacecraft, ranging from small Explorers and Pioneers to the various large orbiting observatories. In considering the balance of the NASA program between large and small spacecraft it became clear that the central issue rests not with the size of the spacecraft, but rather with the method by which the payload space is allocated. At present, all NASA spacecraft, large and small, are controlled in the same manner. Experiments or parts of experiments proposed by scientists are selected by the Steering Committee and the payload is then coordinated and integrated by one of the NASA centers.

We believe that the present procedure serves a useful purpose. We further believe, however, that flexibility can be added to the NASA program in an important way by broadening the allocation procedure.

> RECOMMENDATION: We recommend that NASA adopt the policy of allocating blocks of payload space to research groups of proven competence.

By block allocation we mean the assignment of payload space to a single experimenter or institution without detailed technical review and control by NASA. Such a procedure has long precedent in other scientific activities. For example, it is common practice with the governing bodies of large, expensive installations such as particle accelerators and observatory telescopes to allot research time to groups or individuals of proven competence without extensive and detailed review of the research that is intended. In these cases it is recognized that science-by-committee is not a substitute for the initiative and freedom from stereotyped thinking that is provided by the individual scientist.

It is proposed that NASA adopt a similar policy and allot blocks of payload space to groups of proven competence. This block allocation could take the form of either an entire small satellite or a substantial portion of an observatory satellite or space probe.

Whether applied to large or small spacecraft, the policy of block allocation of payload space would have far-reaching results for research and in the education of students. But it is particularly in the area of small satellites that block allocation

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*See Appendix I for list of participants in the Working Group on Block Allocation of Payload Space.

can be most easily employed. Here we take as a "small" satellite one that can be reasonably instrumented by a single research institution (or occasionally by a group of experimenters acting in concert).

Several reasons for assigning a complete small satellite to a single institution can be listed:

1. A small satellite assigned to one experimental group offers the opportunity to the experimenter of designing a coordinated payload in which complementary experiments can be carried out. The orbiting observatories, of course, also have carefully coordinated payloads. However, the fact that coordination is required between different experimenters of possible different interests and backgrounds and frequently at considerable distance from one another may tend to discourage effective cooperation. When a single experimenter has an entire payload at his control, his opportunity to investigate a particular phenomenon or group of phenomena in a systematic way is greatly enhanced.

2. The small satellite program advocated here can give to space research badly needed "elbow room" for the trial-and-error development of new experimental techniques. The present system has a definite tendency to discourage long-shot imaginative experiments.

3. Small satellites can provide the possibility of obtaining a particular desired orbit and even a particular satellite attitude for a given single or coordinated experiment. They can also provide platforms for some experiments that cause or are subject to interference from others. Examples in this category are VLF experiments and accurate magnetic surveys.

4. A considerable advantage of a small satellite program is its quick reaction capability. At present, a period of about two years normally elapses between the conception of an experiment and its actual flight on a large satellite. The length of this period has a bad effect upon space research. Experimenters are sometimes no longer interested in an idea that was exciting two years ago; improvements in experimental techniques may have outmoded the flight hardware; graduate students (even assistant professors) tend to shy away from waiting so long for results; and most important, newly discovered phenomena cannot be investigated rapidly. It has been demonstrated that this long interval can be drastically reduced by the use of a small satellite in the hands of a single experimenter. It should be possible in general to have a conception-to-flight time of the order of six months to a year.

5. In the experience of some experimenters who have worked with both large and small spacecraft, interface problems and the effort involved in their solution are greater with the large spacecraft. The opportunity to put together a small payload entirely in one laboratory reduces the time and effort spent in solving coordination and interference problems to a reasonable level.

6. Also in the experience -- admittedly limited -- of those who have developed a small satellite in one laboratory, the cost is less on any reasonable basis than it is to prepare an experiment for integration into a large spacecraft.

7. With regard to its educational aspects, the small satellite program advocated here can provide a unique and highly desirable experience in the education of

students associated with it through the combination of scientific and engineering efforts required. Students are forced to become familiar with many engineering aspects of space research that can only make them more valuable contributors at a later date. Benefits will accrue to space science, to the manpower supply, and to the university from this practical integration of engineering with physical science. We note that the few organizations that have already participated in small-satellite programs are strongly in favor of their continuation. The one university in this category, the State University of Iowa, is at present unique, but it certainly need not remain so. We urge NASA to encourage other universities to develop similar capabilities.

With regard to the array of arguments for such a small satellite program, we are, of course, aware that some of the reasons for small satellites are not necessarily reasons against large ones. Certainly problems of cost, reaction time, payload coordination, interfaces, and indeed of scientific "elbow room," are problems of the space business, not confined uniquely to one size of satellite and to be eliminated or solved immediately by the choice of a different size of satellite. The allocation of a block of payload space on a large satellite to a single experimenter will in fact transfer some of the advantages of the small satellite to the larger spacecraft. It is with this in mind that we urge NASA to allot some of the space on large spacecraft in the form of blocks to groups of proven competence.

A further problem considered was that of the selection of groups of "proven competence" both for the small satellite program and for blocks of space in larger payloads. It is clearly necessary that those receiving payload space in either case have previously demonstrated their potential ability to use it well. It is not, however, our intention to strengthen those who have had experience in space science at the expense of those who have not but would like to enter the field. Present NASA policy and programs provide excellent opportunities for new entrants to demonstrate their competence, and we confidently rely upon sound NASA judgment to keep an appropriate balance between the newcomers and the old professionals.

II. Practical Aspects

Two practical aspects of the proposed block allocation of payload space must be examined further.

A. Launchings

The geophysical program in Space Science has a total of 11 Scouts and 6 Deltas planned for calendar years 1963 through 1965. It would be highly desirable if several of these could be used to launch small satellites prepared under a block-allocation program. In addition, it appears at the present time that there may exist some opportunities for the launching of small scientific satellites in the Navy and Air Force space programs. These opportunities are described in some detail in Chapter Fourteen, Scientific Uses of Spacecraft Launched by Other Federal Agencies.

B. Data Acquisition

The development of small, light, reliable tape recorders would make a small-satellite program easier to implement. Until such devices are available (and even then as backup), real-time readout will be required. In this connection the desirability of extending the telemetry reception capability to areas of the world not presently covered — for example, the Middle East, India, and the Pacific — is clear. The international aspect of data reception from an extended world-wide network has much to recommend it through encouraging participation in space research in nations where such would otherwise be impossible, thereby broadening the base of our overall effort.

RECOMMENDATIONS: NASA should:

1. Adopt a policy of granting blocks of payload space on large and small spacecraft to experimenters of proven competence.

2. Sponsor the construction of small satellites by single experimenters or experimental groups.

3. Provide appropriate vehicles to launch such small satellites.

4. Obtain from its own tracking network or from the Department of Defense tracking systems orbital elements for all small scientific spacecraft and make them available as required to experimenters.

5. Extend its telemetry reception capability to portions of the world not presently covered and assure adequate facilities for the coverage of an increased number of future scientific satellites, including those launched by the Department of Defense.

Appendix I

Principal participants in the Working Group on Block Allocation of Payload Space included the following:

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Chapter Fourteen

SCIENTIFIC USES OF SPACECRAFT LAUNCHED BY OTHER FEDERAL AGENCIES*

I. Introduction

The mission of the National Aeronautics and Space Administration, as defined in its enabling legislation, clearly qualifies it as the principal federal agency for the long-term support and conduct of basic space science. Hence the central endeavor of the Space Science Summer Study has been to review NASA's programs, policies, and procedures. Members of the Study were, in addition, privileged to receive several informative unclassified briefings concerning activities in space by representatives of the Army, the Navy, the Air Force, and the Department of Defense. These briefings brought to our attention significant opportunities for space-science research in connection with the defense missions.

Members of the Summer Study have considered the role of basic research within these programs. We fully appreciate the seriousness of the defense missions in space and the responsibility of the Department of Defense and the Services for their successful conduct. Thus, we recognize that basic research must be conducted within these programs in a manner subsidiary to the basic objectives of the programs. Even so, significant ad hoc opportunities for basic research in space exist within the defense missions and the Services have on many occasions in the past facilitated the use of their resources by members of the outside scientific community.

Our purpose was to examine the ad hoc opportunities for basic space science research within the service programs and the policies and procedures relevant to them, and to judge whether greater or better utilization of these opportunities can be made to the benefit of the scientific community, the defense missions, and the national space effort.

II. Information on Service Programs

A. Navy Information

1. Opportunities. The Navy has two major satellite programs: the Solar Radiation series at the Naval Research Laboratory and the Transit program at the Applied Physics Laboratory of The Johns Hopkins University. Opportunities exist within both of these programs for the outside experimenter to obtain payload space for a particular experimental package.

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*See Appendix I for list of participants in the Working Group on Scientific Uses of Spacecraft Launched by Other Federal Agencies.

In addition, the Navy is sponsoring a continuing series of launches of the "composite" type, a group of several payloads launched by the same vehicle which then separate in space to become separate satellites. Here the opportunity exists for the outside experimenter to design and construct a complete satellite of his own.

Additional opportunities that may be available in the future stem from the ability of the Navy to launch a satellite vehicle or geo-probe from any part of the ocean (including the Arctic Ocean). Moreover, beginning in 1964 hundreds of the present versions of the Polaris will become obsolescent. With appropriately designed top stages the Navy estimates a good satellite capability from such boosters, e.g., 180 kg on a 350-km circular orbit. The use of these obsolescent boosters for small scientific satellites or geo-probes definitely holds promise.

2. Procedure. The following procedure has been established in general terms in the Office of the Chief of Naval Operations; formal details are presently being worked out in the Bureau of Naval Weapons.

An interested experimenter, after informal discussions at the working level with in-house Navy laboratories, submits a formal proposal in detail to the Bureau of Naval Weapons, Astronautics Division, code RT. Technical evaluation of the proposal may be carried out by the Office of Naval Research and appropriate subsidiary laboratories. Consolidation of an acceptable proposal into a planned operation is accomplished by the Bureau of Naval Weapons. In this context, the Navy's interests in space are stated to be of a broad and general nature.

Depending upon the scope of the proposal and its interest to the Navy, funding may be necessary. Under some circumstances, there may be a charge to the experimenter for a pro-rated share of the vehicle and launch costs; on the other hand, should the experimenter require support, it may be provided by the Bureau of Naval Weapons, the Office of Naval Research, or elsewhere. In any case, a formal contractual arrangement between the experimenter and the Navy (even if only a no-cost contract) is necessary.

3. Commitments to Experimenter. Should the experimenter provide a package to be incorporated into a Navy payload, appropriate environmental tests and interface requirements must be satisfied. Generally, power for the experiment will be provided from the main payload supply and telemetry data will be acquired along with that for the main payload. The experimenter's data and appropriate orbital information will be made available to him.

Should the experimenter provide a complete piggy-back satellite that will separate in space from the main payload, it must be self-powered and have its own telemetry system. The experimenter then has the responsibility for acquiring telemetry data and orbital information, generally obtainable from NASA. NASA has the capability to receive telemetry data at its Minitrack stations and the responsibility to support Department of Defense programs by providing this service, upon request, within the limits of its network load-carrying capability. By agreement between the Defense Department and NASA, Defense Department tracking systems regularly determine and provide to NASA orbital elements on all unclassified objects. Under appropriate circumstances the experimenter may obtain assistance from Navy-sponsored programs and/or agencies.

B. Air Force Information

1. Opportunities. Numerous opportunities exist within the many Air Force satellite programs for the outside experimenter. These involve sounding rockets and Scout-launched satellites, as well as pods, canisters, and piggy-back subsatellites launched along with major Air Force payloads.

2. Procedure. An interested experimenter should send a detailed proposal to the Office of Aerospace Research, Headquarters, U. S. Air Force. The proposal is evaluated by the appropriate project scientist in this Office and the decision whether to accept or reject it is made by him. It is fundamental requirement that an acceptable proposal must contribute to an Air Force program objective. In addition, it is necessary that the Air Force have a contractual arrangement with the experimenter.

3. Commitments to Experimenter. Once an experiment is integrated into the Air Force system, the responsible project officer is then charged with obtaining for the experimenter both his data and requisite orbital information. The Air Force states that at present the experimenter can count on receiving telemetered data with a 75% probability of success and on the physical recovery of a capsule with 50% probability of success.

C. Additional Information

Considerable additional information about flight opportunities, payload capabilities, policy, procedures, and current programs has been supplied by the Services. The following information is available in the NASA Office of Space Science and from the originating agency:

> "Roster of Projects and Tasks" (for official use only), Air Force Cambridge Research Laboratories, L. G. Hanscom Field, Bedford, Massachusetts; "Discoverer Program," Col. C. L. Battle, USAF (U. S. Air Force Space Systems Division, Inglewood, California), and Mr. J. W. Plummer (Lockheed Missiles and Space Company, Sunnyvale, California).

D. Discussion

In the past, members of the outside scientific community attempting to do basic research within the military space programs have sometimes met with serious difficulties. It is our judgment that while some of the problems have arisen from failure of the scientists fully to appreciate their secondary role in the military program, other difficulties have stemmed from Service procedures. For example, there have been widespread complaints from the scientific community concerning the manner in which basic research-rocket flight proposals are handled by the Air Force, specifically with regard to review of proposals at the project-scientist level of the Air Force Cambridge Research Laboratories. The current procedure is unsatisfactory because members of the scientific community feel that their proposals are not reviewed impartially, as the review occurs where the in-house competition exists. Consequently, the Air Force is now unable to obtain the full measure of cooperation from the scientific community. In view of this situation we believe that the interests of the scientific community and of the Air Force will continue to be poorly served until the Air Force solves this procedural problem.

In our view, experience will be necessary to test the efficacy of the new Navy procedure. The group noted that the scientific community has had long and satisfactory experience with the Office of Naval Research, and believes that the community's interests will be well served if ONR plays a major role in the review of scientific proposals submitted to the Navy.

A further suggestion was made to the working group to help alleviate problems of technical competence and impartiality in the review procedure: viz., the employment of outside consultants as anonymous reviewers, a system successfully used by other federal agencies (e.g., NSF, NIH).

We are also concerned about the release of orbital information and telemetry data from classified military flights which are also used to launch unclassified basic research experiments. Here we stress the mission orientation of the military programs and the fact that the experimenter must rely on the discretion of the Services concerning the release of such data. It is advisable for the outside scientist to understand clearly beforehand any limitations on the availability of data to him. We urge also that the Department of Defense consider, when formulating classification policies, the potential value of the free release of such data to itself as well as to the scientific community.

Certain other necessary functions of the experimenter, such as the ability to supervise the installation of the experiment in the payload and access to the payload for pre-launch checkout, have also been sources of difficulty. As in the case of the release of tracking information, improvements in these areas would result in the scientific community being better able to utilize opportunities for space-science research afforded by the Service programs.

III. Recommendations

FINDING: Better utilization of *ad hoc* opportunities for basic scientific research aboard military spacecraft by scientists outside the Service complex can be made to the benefit of all concerned.

RECOMMENDATION: The scientific community should be adequately informed by the Navy and Air Force of the opportunities for scientific payload space on sounding rockets and satellites launched by these agencies.

RECOMMENDATION: Careful attention should be paid by the Services to their experiment review and selection procedures, and to the establishment of sound and consistent policies with regard to engineering tests of payloads, the release of data from military flights, and other matters concerning the relationship between experimenter and launching agency. In particular, the Air Force should examine the role of the Cambridge Research Laboratories in letting and monitoring basic space-science research, more specifically where selection of payloads for rocket and satellite flights is concerned.

RECOMMENDATION: NASA should make known to the scientific community the availability of orbital elements for unclassified spacecraft as provided to NASA by DOD tracking systems, and that it will provide pertinent orbital data required by experimenters.

RECOMMENDATION: NASA should be alert to the use of spacecraft launched by the military services for basic research, and expand its telemetry reception network to assure adequate world-wide coverage and data handling capacity for these flights.

Appendix I

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December 1962 Addendum

As this Report was going to press, the USAF Office of Aerospace Research furnished, in connection with the material in Chapter Fourteen, the following account of current procedures for handling proposals to fly scientific experiments on USAF rockets, satellites, and space vehicle tests:

This Addendum outlines procedures for handling proposals to fly scientific experiments on USAF rockets, satellites, and space vehicle tests.

1. Headquarters, Office of Aerospace Research (OAR) provides the management and mechanics whereby scientific exploration and investigation of the aerospace environment can be accomplished. This involves the arrangement and funding for rocket vehicle support, collection of scientific experiments from the many interested agencies, and the scheduling of the many activities to accomplish the objectives of the experiments.

2. In accomplishing this mission, this Headquarters forms and chairs a committee, called the Payload Review Committee, consisting of representatives from all Air Force agencies submitting requirements to conduct an experiment. The procedure and criteria for selecting experiments follow:

Committee

a. Chairman is Assistant Deputy Chief of Staff, Plans and Programs, Headquarters, OAR.

b. Scientific representatives of all Air Force agencies submitting requirements.

Procedures

a. Proposals for experiments are submitted in accordance with a prescribed format (available on request).

b. These proposals may be carried to the committee meeting, or mailed to Headquarters OAR either directly or through channels.

Criteria for Consideration of an Experiment

a. Requirements must be approved by the commander of a military agency or other proper authority.

b. Adequate funds for the proposed experiment must be available.

c. The experiment must be part of approved Air Force basic or applied research program. Experiments which are funded from sources other than the Air Force may be flown under certain circumstances: for example, special contractual arrangements might be made with non-government experimenters, or, in the case of experiments funded by other government agencies, Air Force approval of the program will constitute authority for the flights.

d. The proposed experiment should not unnecessarily duplicate other similar efforts within the United States.

Partial List of Criteria by which an Experiment is Selected

a. Its scientific importance to the Air Force.
b. Scientific technique.
c. Its feasibility.
d. Competence of experimenter.
e. Adequacy of technical support available to the experimenter (in-house or contractor).
f. Availability of suitable vehicle.

3. Following the procedures described above, the Payload Review Committee applies the above procedures, criteria and selection of experiment factors to all requirements received. Each experiment requirement is explained and thoroughly discussed. A method of scoring or evaluating experiments is agreed to and adopted. The Committee action results in a list of experiments that are recommended for vehicle support, in order of descending priority.

4. The selected experiments are then grouped or packaged into payloads for various types of vehicles in accordance with the priorities established for the experiments. The matching of the selected experiments with various possible vehicles is packaged into a program. Next, the payload and vehicles are scheduled for launch at the appropriate range facility, and finally the program is carried out.

5. The size of the program depends on the amount of funds provided for vehicle support. All experiments and objectives are thoroughly coordinated with all other government agencies conducting similar activities before final approval and implementation by the Air Force.

6. Vehicle support provided by OAR consists of:

a. Procurement of rocket vehicle or secondary payload capability.
b. Telemetry (data transmission).
c. Destruct system (if required).
d. Radar beacon.
e. Vehicle guidance (if required).
f. Flight programing.
g. Standard payload carrier.
h. Electrical power for the vehicle and experiment.

All of the foregoing items of equipment must be standard developed items and readily available. Design and development of equipment in these categories will not be permitted.

Chapter Fifteen

INTERNATIONAL COOPERATIVE PROGRAMS*

I. Introduction

There are three broad areas of possible international cooperation in the study and use of space: (i) fundamental research in a large number of scientific fields, (ii) applications for weather, communications, and navigational purposes, and (iii) exploration of celestial bodies, including manned exploration of the Moon.

Scientific research is the main area in which extensive cooperative programs have been undertaken successfully or are now underway, as represented by the International Geophysical Year (IGY) and the continuing work of the International Council of Scientific Unions (ICSU), particularly through its interunion committee on space, known as the Committee on Space Research (COSPAR). Bilateral governmental programs of cooperation under the National Aeronautics and Space Administration (NASA) also emphasize scientific research. The application of space techniques for communications, weather, and navigation purposes also have been the subject of cooperation. In the present experimental stage of these practical applications, such cooperation has been largely between NASA and a number of cooperating countries. In the next few years, as the use of data from weather satellites becomes operational, cooperation in this area will expand into a broader multilateral framework, probably under the auspices of the World Meteorological Organization (WMO). As arrangements for operational communications satellites develop, cooperation in this area will expand within a broader multilateral network, probably within the purview of a combination of intergovernmental, semigovernmental, and private commercial organizations. Cooperation in programs for the exploration of celestial bodies, including manned exploration of the Moon, have been tentatively suggested at different times, largely in the context of possible cooperation between the U. S. and the USSR, but to date these suggestions have not been adopted.

Existing channels through which international cooperative programs are implemented particularly for scientific research include both nongovernmental and governmental organizations. The nongovernmental organizations are largely within the framework of ICSU, its constitutent discipline unions (such as Physics, Biology, etc.), and through interunion committees concerned with specific research programs of an interdisciplinary nature. Cooperation through governmental channels includes bilateral arrangements between NASA and appropriate agencies of other governments, multilateral regional organizations, such as the European Space Research Organization (ESRO), and world-wide intergovernmental organizations, primarily the United Nations (UN) and its specialized agencies.

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*See Appendixes I and IV for a list of participants in the Working Group on International Relations.

A. International Council of Scientific Unions (ICSU)

The International Council of Scientific Unions (ICSU) is composed of 14 international scientific unions, and more than forty national scientific institutions representing as many countries. Each of the 14 international unions is devoted to one of the following scientific disciplines: astronomy, biochemistry, biology, chemistry, crystallography, history and philosophy of science, geography, geology, geophysics, mathematics, mechanics, physics, physiology, and radio physics. Membership in the unions is held by representatives of the appropriate scientific discipline from national scientific institutions throughout the world—primarily academies of sciences, national research councils or their equivalent. The present group of international scientific unions grew out of the need to exchange information by means of personal contact among scientists in each discipline from many nations. The unions provide forums for scientific meetings and the discussion of new scientific results and future research. They publish international journals, and solve common problems of nomenclature, standardization of measurements, etc. ICSU itself provides a council for solving problems common to one or more unions and for organizing the interunion committees that are concerned with research on an interdisciplinary basis.

Cooperation in space research under ICSU can be divided into two periods: first, the period of the IGY during which space research itself was initiated, and second, the period after the IGY during which space cooperation was continued under a new interunion committee, COSPAR. The interunion committee for the IGY was known as CSAGI, after the French initials of its name (le Comité Spécial de l'Anné Géophysique Internationale). Under the auspices of this Committee, a series of IGY assemblies were held in the course of which hundreds of specialists from the 66 countries involved were gathered together during the years just prior to and during the IGY. As in other scientific programs for the IGY, the space research was planned and coordinated under the auspices of a working group which organized assemblies for discussion in which plans for projects by national scientific communities were reported and harmonized one with another. In addition to reports from the U. S. and USSR on planned satellite projects, rocket flight plans and summaries from seven nations were interchanged, tracking information for satellites was issued, including orbital elements and/or station predictions, and research results were interchanged through the network of World Data Centers set up by the IGY for this purpose.

For the U. S., the National Academy of Sciences is the adherent body to ICSU. U. S. participation in the IGY was organized through the Academy committee known as the U. S. National Committee for the IGY, which coordinated the IGY activities of both governmental and private scientific institutions within the U. S.

B. Committee on Space Research (COSPAR)

Before the IGY closed, proposals to continue the activities it had initiated led to the establishment of separate research programs in the fields of oceanography and space, and for the Antarctic, and a few years later successor programs in geophysics were established, known as the International Year of the Quiet Sun (IQSY) and the World Magnetic Survey (WMS). Programs for meteorology (including hydrology) and for a study of the upper mantle of the earth's crust (UMP) are now being planned. Each of these special research programs is now, or will be, run under the auspices of an interunion committee; at present the existing committees include: Special Committee on Oceanographic Research (SCOR), Special Committee on Antarctic Research (SCAR), the International Committee on Geophysics (CIG) for the IQSY and WMS, and COSPAR.

The Committee on Space Research (COSPAR) was established in October 1958 and held its first meeting in London during November 1958. Like ICSU, it has dual membership, and is now composed of representatives of national scientific institutions from 18 countries and representatives of 10 of the subject matter unions in ICSU. Its internal organization consists of a Bureau, an Executive Council, and four working groups. Like CSAGI, COSPAR conducts its business through a series of assemblies, usually held once a year, which combine both scientific symposia and working sessions at which plans for national space research projects are discussed and harmonized. In addition to reports by national representatives on their past year's programs and future plans, COSPAR has also continued and improved agreements on interchange of data developed during the IGY. Launching announcements and orbital elements for satellites are transmitted over a rapid communication network for satellite information known as SPACEWARN; rocket flight summaries are interchanged and scientific results are reported to the network of World Data Centers.

COSPAR has held three international space science symposia, each of which has been attended by scientists from between 25 to 30 countries and each of which has included about 100 scientific papers. The published volumes of these symposia constitute major compendia of the literature of space science research. An annual series of international rocket intervals has been established, with an increasing number of countries participating each year. A new Panel on Synoptic Rocket Soundings shows promise of developing these intervals into rocket launchings coordinated for synoptic measurements of parameters of the atmosphere. Studies of special topics have been initiated: notably the study of the remarkable events of July 1959 and November 1960, which were observed at the same time by an interplanetary probe, a near-Earth satellite, and on the ground by a network of observing stations—all equipped to measure particle fluxes or magnetic field strengths. Reference tables for properties of the upper atmosphere above the previous altitude limit of 32 km have been developed and published, and these tables will be revised as new scientific results are acquired. A comprehensive world-wide list of tracking stations has been developed and will be published soon. A similar list of radio and radar stations will follow. COSPAR has been active in the ICSU Interunion Committee on the Allocation of Radio Frequencies (IUCAF) in bringing to the attention of appropriate intergovernmental agencies, particularly the International Telecommunications Union (ITU), the needs of space experimenters for allocations of radio frequencies. A consultative group of competent scientists is now being established by COSPAR to make quantitative studies of space experiments having potentially harmful effects on other scientific activities. At the request of CIG, COSPAR has taken an active part in planning experiments aboard rockets and satellites to be conducted as contributions to both the IQSY and the WMS. One of its working groups has been reorganized for the purpose of giving major attention to these two programs. COSPAR has also provided the forum through which NASA made its offer—resulting in the recent joint U. S. -UK satellite, Ariel I — to launch satellite experiments designed and developed by scientists from other countries.

Coordination between COSPAR and the discipline unions of ICSU and the other interunion committees concerned with oceanography, the Antarctic and other aspects of geophysics is accomplished through liaison membership and joint meetings on special topics. As in COSPAR, the discipline unions are represented, where appropriate, in the membership of other interunion committees and frequently special topics are discussed in meetings jointly sponsored by one or more of the interunion committees and one or more of the subject matter unions.

The National Academy of Sciences is the adherent body to COSPAR for the U. S. , and U. S. activities are coordinated by the Committee on International Relations of the Academy's Space Science Board. Coordination with NASA is close since principal members of the NASA staff participate in meetings of the U. S. Committee and act as advisors to the U. S. delegate during meetings of COSPAR. Appendix III outlines the corresponding structures within ICSU and the nongovernmental scientific organizations in the U. S. actively concerned with these channels of international cooperation.

C. United Nations and Its Specialized Agencies

Space cooperation under the United Nations (UN) is within the purview of the General Assembly, rather than under a specialized organization, such as programs in atomic energy under the International Atomic Energy Agency (IAEA). The General Assembly identified the following as areas of cooperation: general legal principles to insure that space will be used for peaceful purposes only, scientific research, communications satellites, weather satellites and meteorology, and training and education. While some of these activities are clearly within the competence of existing UN specialized agencies, such as the World Meteorological Organization (WMO), the General Assembly has established a Committee on the Peaceful Uses of Outer Space to coordinate and organize the over-all UN program. This Committee held its first meeting in March 1962 and established two subcommittees; one to deal with legal matters and the other to deal with scientific and technical cooperation. These two subcommittees met in Geneva, Switzerland during June, 1962; no agreement was reached on legal matters, but the Scientific and Technical Sub-Committee adopted recommendations on exchange of information, support and assistance for existing international cooperative programs, such as the IQSY, and on the establishment of an international equatorial launching range for sounding rockets. The implementation of these UN recommendations will facilitate the participation by the wide membership of the UN in cooperative programs in the field of outer space. In doing so, the UN recommendations make full use of the existing programs of cooperation conducted by the nongovernmental organizations within ICSU, particularly the activities of COSPAR.

The WMO, a specialized agency of the UN, has prepared a detailed report proposing an expansion of the network of meteorological stations around the world in order to take advantage of the new types and larger amounts of data available from meteorological satellites. It also proposes a large effort in the field of atmospheric sciences in concert with the ICSU.

The ITU has expanded its role in the allocation of radio frequencies to take account of the new needs of space experimenters, and of operational space uses (such as weather and communications) which involve telecommunication from and to space vehicles. The ITU is now preparing for an Administrative Radio Conference to take place in 1963 at which these matters will be discussed again. In preparation for this meeting the ITU has developed consultative relations with the scientific community through its usual channel, the International Radio Consultative

Committee (CCIR) which advises on all scientific aspects of radio communications, including operational radio services. The International Radio Science Union (URSI) of the ICSU is a consultant to this group. In addition, the ICSU Interunion Committee on the Allocation of Radio Frequencies (IUCAF), of which COSPAR is a member, coordinates and presents views to the ITU on the needs of scientific experimenters for radio frequencies.

The UN's Educational,Scientific and Cultural Organization (UNESCO) has worked closely with ICSU for a number of years, has contributed generously to the support of ICSU activities, and was one of the sources of international funds in support of the IGY. UNESCO has continued to provide this kind of assistance to COSPAR, and now plans to extend similar assistance to the IQSY Committee of CIG and to assist some countries in establishing or improving observatories that can contribute to the IQSY. Appendix II shows the corresponding international and U. S. Government structures concerned with the UN and regional organizations.

D. Regional Groups

To date regional groups have been established most actively in Europe and include the European Space Research Organization (ESRO) and the European Launcher Development Organization (ELDO). Like COSPAR, ESRO will confine its attention to scientific experiments conducted in space though, unlike COSPAR, it may undertake the pooling of national effort for the design and development of actual experiments to be flown. The countries involved are Austria, Belgium, Germany, France, Italy, the Netherlands, Norway, Spain, Switzerland, and the United Kingdom. ELDO is being established to develop propulsion capacity for launching based on the UK Blue Streak rocket. Countries now considering joining ELDO include Australia, Austria, Belgium, Denmark, France, Germany, Italy, the Netherlands, Spain, Sweden, Switzerland, and the United Kingdom.

Latin American countries, in cooperation with the U. S. , have established a Provisional Inter-American Committee for Space Research to explore possibilities of regional cooperation. The Committee includes representatives from Argentina, Bolivia, Brazil, Chile, Peru, Uruguay, and the U. S.

E. Bilateral Governmental Cooperation

While there is evidence of bilateral cooperation between the USSR and the other Eastern European countries in the form of satellite tracking, theoretical analysis (especially of satellite orbits), and exchange of information, no formal programs of cooperation between these countries have been announced to the rest of the world. On the U. S. side, NASA has cooperative arrangements for about 118 projects with at least 57 different countries — arrangements which include from one to eleven separate projects with each country. These include cooperative projects for satellites (3), sounding rockets (10), and for such ground-based activities as meteorology (35), communications (5), ionosphere-beacon (8), and ionosphere sounding (14); also operational activities in support of tracking, such as Minitrack (7), Mercury (7), deep space (2), optical (9), Moonwatch (15), and data acquisition (3). Personnel exchanges, as part of the NASA cooperative program involve the following numbers of countries: resident research associates (15), international fellowships (5), and training (13); short-term visits to NASA installations have included persons from 51 countries.

II. General Findings

Based on a review of the foregoing background, the discussions in the balance of the present chapter support findings and recommendations concerning international cooperative programs in space science on (i) the size and scale of existing programs, (ii) current NASA policies, (iii) relative emphasis between nongovernmental and governmental programs, (iv) the complexity of nongovernmental organizations, and (v) the future of COSPAR. The first three of these topics are largely concerned with NASA's international program, and the last two with the activities of the National Academy of Sciences and ICSU, particularly COSPAR. In addition, a number of specific items from other chapters are discussed in terms of their international aspects.

A. Size and Scale of Existing Cooperative Programs

> FINDING: The present level of emphasis by the U. S. on programs of international cooperation in space science seems to be amply justified by the results to date and by those expected in the future. Indeed, the U. S. should place even greater emphasis upon programs of international cooperation, but only to the extent that the projects involved shall continue to have a sound scientific, technological, and economic basis.

Programs of international cooperation in space science contribute to the advancement of scientific knowledge through (1) exchange of scientific ideas and results, (2) the organization of collaborative enterprises such as the IGY and IQSY, (3) increasing the level of activity of existing advanced national scientific communities, and (4) raising the level of competence of the scientific communities in the less developed countries of the world. United States programs contributing to these objectives include (i) cooperation in fundamental science through ICSU and COSPAR, (ii) bilateral governmental cooperation through NASA, and (iii) participation in the United Nations and its specialized agencies.

Cooperation in fundamental science through ICSU and the bodies associated with it provides forums for exchanges of scientific ideas and results and the organization of collaborative scientific enterprises. Reports on national space programs are exchanged annually. International symposia are also held yearly. Often such scientific meetings create the specific scientific goals and the initial contacts that initiate bilateral cooperation between governments. The joint scientific enterprises that grow out of these bilateral discussions are enlarging the level of competence, both scientific and technical, in a number of countries. Important activities within the UN are just beginning after an initial delay, and show promise of making complementary contributions to these other efforts. The UN, in addition, is better suited to achieving political agreements intended to ensure that outer space will be used exclusively for peaceful purposes and that regulations and controls desirable for space activities will be applied. Initiatives in this area are only just beginning.

No precise measure of the present level of emphasis represented by the foregoing cooperative programs or of their results is available, but based on the presentations at the Summer Study some general statements are warranted. International cooperative endeavors in science established by ICSU and its committees (COSPAR, CIG), beginning with the IGY and continuing with the present IQSY and WMS, have and are significantly increasing the general level of scientific activity, particularly in the scientific disciplines involved in space research. Satellite experiments were initiated as part of the IGY, and the number of rocket experiments particularly for synoptic measurements was also significantly increased through this same program. Bilateral cooperation during the IGY and since has in many cases made possible the contributions of other countries to the accomplishment of international scientific goals. In particular, the NASA bilateral program has, in a very few years, increased from 2 to 6 the number of countries actively developing satellite experiments, and has increased from 5 to 13 the number of countries launching rocket experiments. Moreover, the level of activity involved in ground-based research related to experiments in space has also increased significantly, as has the number of countries capable of engaging in such activities. For example, NASA now has cooperative projects for ground-based activities with at least 44 countries. In addition, operational activities in support of the U. S. space programs, principally tracking, involve some 24 countries. In many of these countries such activities will contribute to the advancement of the level of scientific and technological competence.

The rate of increase in world-wide scientific activity and in the development of scientific and technological competence is impressive, and probably reasonably in accord with what can be expected in view of important competing demands for trained manpower and financial resources. U. S. cooperative activities contributing to this rate of increase should be continued at about their present levels with increases that permit taking advantage of new opportunities for sound scientific endeavors by national scientific communities.

In addition to the foregoing results related to the scientific objectives of cooperative space programs, there are a number of tangible results of direct benefit to the U. S. space program which are considered deserving of strong emphasis.

Scientific Discoveries: Exchange of scientific data resulting from U. S. flight programs makes possible interpretations of these data by scientists from other countries, thereby increasing the value of U. S. scientific experiments. For example, a Belgian, M. Nicolet, interpreting data from the U. S. Echo balloon satellite, predicted the presence of free helium in the atmosphere, a theory which was later substantiated experimentally by U. S. investigators.

Cooperative Experiments: The Canadian topside sounder satellite is a second-generation experiment which would have been included in the U. S. program if not undertaken by the Canadians. The Canadian concept of the experiment was more advanced than the thinking in the U. S. at the time the experiment was first proposed, and it is being financed at least in the amount of about $2 million by the Canadians, not to mention their contribution of creative ideas, effort, and time.

Synoptic Experiments: The cooperation of other countries is essential to the success of United States scientific objectives that require synoptic measurements, and the participation of scientists from other countries increases the available manpower and reduces the costs of the U. S. part of any program of this type.

Use of Rocket Sites not Otherwise Available: Through international cooperation sites for rocket launchings otherwise not available to the U. S. can be used for experimental needs that can be satisfied best, or, in some cases, only from these sites, for example, launchings from Sweden for the study of noctilucent clouds, from Australia for studies of the sky in the southern hemisphere, and from India for studies of the ionosphere above the equator.

Meteorological Satellites: International cooperation is vital for the solution of the problem of handling the tremendous amount of data from meteorological satellites, and the manpower pool of competent scientists for this task is significantly increasing the cooperation of other countries.

Global Tracking Networks: Sites in other countries are essential for a global tracking network, and a number of U. S. tracking stations are manned and paid for by other countries, thus reducing the cost of the U. S. program.

Communications Experiments: Satellite experiments for worldwide communications cannot be undertaken without the cooperation of other countries, and the British and French have each spent about $17 million for ground receiving stations which otherwise would have been built and paid for by the U. S.

B. Current NASA Policies

> FINDING: The NASA International Program is considered to be imaginative and effective in achieving the scientific objectives of international cooperation, and it should be continued under its present policies and guide lines.

The NASA program of bilateral cooperation is making a direct contribution to the impressive increase in the number of countries developing satellite experiments, launching rocket experiments, and engaging in ground-based research related to experiments in space. (See the Introduction and Section II. A for details.)

We are conscious of the wisdom of Congress in including the following mandate for international cooperation in the basic legislation establishing NASA:

> "The aeronautical and space activities of the United States shall be conducted so as to contribute materially to . . . (7) Cooperation by the United States with other nations and groups of nations in work done pursuant to this Act and in the peaceful application of the results thereof; . . . (Section 102 (c))."

NASA policies for implementing this mandate are contained in the following guide lines issued by the NASA Office of International Programs:

> "1. Designation by each participating government of a central agency for the negotiation and supervision of joint efforts.
>
> "2. Agreement upon specific projects rather than generalized programs.
>
> "3. Acceptance of financial responsibility by each participating country for its own contributions to joint projects.

"4. Projects of mutual scientific interest.

"5. General publication of scientific results."

These policies are considered useful in avoiding many of the difficulties experienced in other types of bilateral cooperation, and they are particularly suitable for cooperation in science, where effective work by the cooperating country depends almost entirely on the competence attainable by their scientific personnel and requires domestic support and resources in substantial amounts.

Bilateral cooperation should continue to be based on limited projects within the attainable competence of scientists in the cooperating country, and should continue to require financial responsibility on the part of the cooperating country for its contribution to the joint project.

C. Relative Emphasis Between Governmental and Non-Governmental Channels of Cooperation

> FINDING: The U. S. should continue to use a multipronged approach to international cooperation. Ideally, we should strive wherever possible to secure multilateral programs (COSPAR and UN), but it is recognized that bilateral programs are often desirable and necessary, and that the two types of programs are complementary. Cooperation through government organizations (UN and bilateral) will continue to be necessary and should be encouraged, but the importance of nongovernmental scientific organizations should continue to be recognized.

The existing channels of international cooperation include (i) multilateral cooperation through nongovernmental scientific organizations (ICSU and COSPAR), (ii) bilateral and limited multilateral cooperation with other governments, and (iii) multilateral cooperation through inter-governmental organizations, such as the UN and its specialized agencies. Each of these channels is best suited for different purposes.

Nongovernmental scientific organizations are essential for mobilizing the worldwide scientific community for common or collaborative endeavors in behalf of fundamental scientific goals, as exemplified in such international enterprises as the IGY and IQSY. More fundamentally they provide the forum for the exchange of scientific ideas and results in which proposals for common scientific goals germinate and develop best. Specific scientific projects conducted as contributions to these goals are normally undertaken by national scientific communities. Often bilateral cooperation between governments contribute to, or make possible, these national scientific projects. Such organizations as COSPAR, however, are not used as the channel for the mutual commitment of finances and other resources to the implementation of specific projects. Also, nongovernmental international scientific organizations have not to date been used for the administration of substantial amounts of common funds for the implementation of specific international scientific projects.

Bilateral cooperation between governments usually involves the commitment of resources for specific joint scientific projects. Normally, the contribution from the U. S. takes the form of technical advice, launching of satellite experiments, instrumentation, and sounding rockets not available in the cooperating country. Bilateral cooperation, therefore, is best suited for extending or developing the scientific and technical competence of the cooperating country. Often the initiative for such joint projects results from the desire to contribute to the common scientific goals established by the nongovernmental international scientific organizations. In addition, projects in other countries resulting from U. S. bilateral cooperation not only help the other country, but are often an extension of the U. S. space program, making possible the achievement of U. S. program objectives at less cost, and with scientific assistance from the cooperating country.

Multilateral governmental cooperation among a limited number of countries, as exemplified by the European Space Research Organization (ESRO), is just coming into existence, and experience with this type of cooperation is limited. ESRO, however, may be considered essentially as an extension of bilateral cooperation, in which the cooperating partner is a group of neighboring countries each of which can do more by combining its resources with those of the others.

Multilateral intergovernmental cooperation as exemplified by the UN (Committee on Outer Space and its Scientific and Technical Sub-Committee), is also just beginning. Because of its wide membership, the UN has the power to undertake projects on a world-wide basis, but it is not as well suited as ICSU to tap the creative enthusiasm of scientists. The UN also has the power to commit finances and resources, but not, experience has shown, in the substantial amount committed through bilateral channels. Because of its political nature, the UN is better suited for reaching and enforcing agreements on the regulation and control of space activities, but until such measures are politically possible, the UN can contribute to cooperative activities. It has already begun this by endorsing and complementing cooperative scientific activities carried on through the other two channels. The UN specialized agencies serve slightly different purposes. UNESCO has been providing small amounts of money in support of meetings of ICSU and its committees, and is planning to continue such support. The ITU has extended its role in allocating radio frequencies to include space needs. The WMO has recently reported on its plans for the operational handling of data from meteorological satellites.

In principle, multilateral nongovernmental scientific organizations should be used for fundamental science, bilateral cooperation and UN specialized agencies for increasing scientific competence and operational activities, and the UN itself for political measures requiring formal intergovernmental agreements. However, each channel of cooperation, to some extent, serves as a complement to or as an alternative for the others, and the flexibility made possible by the existence of these alternatives is desirable.

In general, the relative emphasis of existing programs is reasonably in accord with the foregoing considerations. If any shift of emphasis is desirable, it would be to ensure even further that, wherever possible, bilateral cooperative projects be undertaken as contributions to international scientific goals established by multilateral nongovernmental scientific organizations.

D. Complexity of Existing Nongovernmental International Organizations

FINDING: The present complexity of international scientific groups interested in space science reflects the growth of specialization in science. This complexity of organization should be simplified, but change will have to be brought about through evolution. The U.S. should assist this evolutionary process by strengthening organizations like COSPAR which serve interdisciplinary needs.

The chart in Appendix III lists corresponding international and domestic scientific groups, most of them established since the IGY to advance international collaboration on specific programs of scientific research. In general, each group serves as the focal point for the interests of a number of scientific disciplines interested in the area of research concerned, such as space, the oceans, the Antarctic, etc. This pattern of organization is considered too complex, since the disciplines interested in each area overlap, particularly between geophysics (including the IGY, IQSY, and WMS), meteorology and space. As a result of these arrangements, the traditional discipline unions in ICSU sometimes find themselves in competition with the interdisciplinary committees, and at times one interdisciplinary committee finds itself in competition with another. These problems of overlapping competence could be improved by a rational redistribution of tasks to a smaller number of committees; however, because of existing traditions within the scientific disciplines involved, change could not be accomplished satisfactorily except through evolution. In the meantime, special care must be taken to achieve maximum coordination of programs and activities, and to clarify and strengthen the role of such interdisciplinary committees as COSPAR.

COSPAR activities to date have been based on furthering coordination with the unions and with CIG, particularly for the IQSY. Ten of the ICSU unions have membership in COSPAR. Union representatives are invited to participate in COSPAR working groups of interest to each union. Meetings of such working groups are often held in conjunction with appropriate union assemblies and frequently are cosponsored by the union, for example, the working group on the IQSY held at Kyoto, Japan, in conjunction with the International Conference on Cosmic Rays and the Earth Storm. (This conference itself is an example of coordination since it consisted of the seventh meeting of the IUPAP Cosmic Ray Commission and an IUGG Symposium on the Earth Storm, cosponsored by IAU, URSI, and IUPAP.* COSPAR symposia on specialized topics are organized in association with appropriate unions and other organizations, for example, the Washington symposium on meteorology was cosponsored by WMO and IUGG. This coordinated approach requires further effort and refinement, and the U.S. should encourage such efforts.

E. Future of COSPAR

> FINDING: The U.S. should continue to support and strengthen COSPAR as the forum for nonofficial (scientific) contacts between national space committees and as the focal point for the space interests of the ICSU (discipline) unions.

The most useful purpose served by COSPAR is to provide a catalyst for the definition of scientific goals, to be achieved by experiments in space behind which

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*See Table of Abbreviations, Appendix III.

can be marshalled the scientific communities of the world. In the few years of COSPAR's existence it has evolved significantly toward fulfilling this purpose. Increasingly the scientific symposia held in conjunction with annual COSPAR assemblies are becoming the principal international forum for the discussion of the significance of new and future space experiments. Attendance by the scientific community has been large and the number of countries represented by the scientific presentations has been increasing. The published proceedings of these symposia have become important compendia of space science literature. Functioning through its four working groups, COSPAR has a number of creditable achievements, including the establishment of international rocket intervals, improved exchange of launching and orbital information, reference tables for properties of the upper atmosphere, an interdisciplinary study of the solar and geophysical events of July 1959 and November 1960, and contributions to the IQSY and WMS. In full recognition of the importance of the accomplishments of COSPAR, steps should be taken to make this organization even more effective as an instrument of international cooperation in fundamental science in space.

COSPAR should retain its character as a nonofficial (nongovernmental) forum for contacts between national space committees and this role of COSPAR should be kept distinct from such official intergovernmental contacts as occur in bilateral cooperation or in the context of the UN. To strengthen COSPAR's role, however, cooperative activities based on official (governmental) contacts should wherever possible be used to supplement and endorse decisions taken by a scientific community in COSPAR. For example, specific scientific projects conducted under bilateral auspices should wherever possible be undertaken as contributions to international scientific goals established by COSPAR. At times this may mean asking COSPAR to consider adopting ideas initiated in bilateral negotiations. Similarly activities undertaken in the context of the UN involving scientific cooperation should be carried on when appropriate, as an endorsement of, or in supplement to, scientific programs established by COSPAR.

COSPAR should continue to serve as a focal point for the space interests of the international scientific unions, thus effecting a coordination among the various disciplines involved. COSPAR has taken the position that its symposia should not conflict with scientific meetings more properly held by the several ICSU unions. However, it is felt that symposia held by COSPAR are necessary for broadening the base of scientific participation in COSPAR, and for the stimulation of scientific ideas essential for creative working sessions. To accomplish this objective without conflicting with the unions, COSPAR symposia have included (i) general review papers synthesizing and interpreting latest results, (ii) sessions on special topics, such as tracking and orbital problems, tied to the needs of its working groups, and (iii) new scientific results not reported at other recent major symposia. On topics of direct mutual interest, COSPAR has assisted the unions in arranging special symposium sessions in conjunction with their assemblies, for example, a session on exobiology held at the 1961 Biochemistry Congress in Moscow. Further attention and care should be given to this problem with a view to arranging joint sponsorship with appropriate unions and the holding of specialized symposia in conjunction with union assemblies. In addition, COSPAR should emphasize further the role of review papers in its symposia and tend to limit presentations of new results to those that are directly derived from, or at least closely related to, the use of high-altitude and space vehicles.

The representatives of unions with membership in COSPAR should have the advice and support of a committee within the union which can speak for the space interests of the discipline involved, and steps should be taken to ensure the creation and strengthening of effective committees within each of the unions involved.

III. Topics Referred from Other Chapters

A. Development of Meteorological Rocket System for World Network

> RECOMMENDATION: To implement the synoptic meteorological sounding rocket measurements planned for the IQSY, low-cost rockets and compatible instrumentation for measuring winds, temperature, and pressure or density up to an altitude of at least 60 km will be required. This system may be compatible with current or modified radiosonde ground equipment, and should in any case not require elaborate radar support. Sufficient effort should be applied in the United States to complete the development of such a meteorological rocket system prior to 1 January 1964, and thought should be given to arrangements that would permit NASA to make it available to other countries under existing policies.

An important meteorological program is being planned for the IQSY using meteorological rockets to study changes in the atmosphere caused by solar activity at altitudes above those reached by radiosondes. Reliable, inexpensive rockets and instrumentation, suitable for use at geographic locations remote from elaborate launching sites and yet having adequate altitude capability to carry out the desired scientific functions are not presently available, but could be developed by extending present techniques. Measurements of wind, temperature, and pressure or density at heights above those reached by radiosondes are required. It would be desirable to extend such measurements up to a height of over 100 km; however, development of suitable rocket instrumentation having this capability in time for the IQSY seems doubtful on technical grounds, and therefore it is recommended that equipment capable of making measurements up to 60 or 70 km be developed as a first phase effort. Measurements up to over 100 km are recommended as a second phase of this program. (Development of appropriate equipment by NASA is also recommended for work in meteorology, quite apart from its international applications; see Chapter Eight.)

Existing alternative systems of obtaining the desired wind and temperature measurements (i.e., sodium trails and grenades) require large, expensive rockets and have other features that make them difficult to use for synoptic measurements. For example, sodium cloud measurements can be taken only at twilight on clear days. Grenade measurements involve the use of a rather elaborate system of microphones in a carefully laid out pattern on the ground, which is currently available at only a few sites in the world. Also, the payload required for a grenade experiment is so large as to require something like a Nike-Cajun rocket, which costs about $8,000. A modification of balloon radiosonde systems for rocket use is being developed by the U.S. Army Signal Corps that will employ a modified radio-theodolite costing $30-40,000, capable of giving angle and range information. (The modification to give range properly is apparently causing difficulty.)

Extensions of present techniques could include further development of the ARCAS and HASP II. The ARCAS has not been very reliable and the current payload requires an advanced radar to give information about winds. The HASP II rocket is a recent Navy development that shows promise. Its acceleration at launch is high compared with that of the ARCAS, and consequently it can be launched in a relatively high wind. However, at present it can only carry light chaff payloads to a height of 65 km and chaff requires radar tracking. Its cost is said to be about $650, as compared to about $1,300 for the ARCAS. A second payload involving much more sophisticated instrumentation, i.e., a temperature sensor and radio telemetry, costs about $1,300. The required further development of the ARCAS or the HASP II rocket system could probably be achieved by 1964 if sufficient resources and effort were applied.

Continuation of the Army or Navy development programs with more support, or initiation of some comparable program by NASA, thus appears to be necessary. Arrangements for making rockets or instrumentation available to other countries, however, should be undertaken by NASA under existing policies in consonance with the civilian character of the IQSY program.

Some declassification problems may exist in connection with making the ARCAS, HASP II, or an equivalent rocket available outside the United States; this question should be explored further by the Space Science Board.

B. Sterilization

> RECOMMENDATION: Additional attempts should be made to discuss with other launching countries the question of avoiding contamination of celestial bodies, especially Mars.

No matter what the degree of U.S. efforts to avoid contamination of celestial bodies, the success of this policy also depends on the application of similar safeguards by other launching countries, and the ultimate aim of international discussions on this topic should be to reach agreement on standards and procedures of sterilization. Special caution is required to avoid the risk that this issue could be used to cause unwarranted and unilateral delays in the U.S. planetary program. Past attempts to consider standards and procedures of sterilization with other countries have not yielded sufficiently concrete information about policies or technology.

A number of alternative approaches were considered by the Summer Study. A unilateral declaration by the U.S. would permit other countries to criticize our standards and procedures without revealing their own. The holding of a symposium with wide participation would serve to educate a large segment of the world scientific community and might exert pressure on others, and this approach is desirable. A proposed symposium on exobiology, cosponsored by IUB and COSPAR, may provide an appropriate opportunity.

The initial aim of such discussion might be limited to ascertaining the attitudes of other countries toward this problem, or if possible to the exploration of the prospects of reaching common agreement on standards and procedures for sterilization. Further decisions on the U.S. official attitude should be deferred until some concrete indications of the attitudes of other countries are available, although NASA will have to make certain interim decisions to govern its own program in the meantime.

C. Continuous Telemetry

> RECOMMENDATION: NASA is urged to consider the use of continuous telemetry and cooperation with scientists in other countries on read-out and data reduction on some of the solar monitoring satellites and similar monitoring experiments, and to review carefully the possible use of continuous telemetry on satellite projects of intermediate size and complexity.

The use of continuous telemetry to permit read-out of data by scientists in other countries would serve both scientific objectives and the purposes of U.S. international cooperation in space activities. It would increase the numbers of scientists doing supplemental research and analysis from each satellite launched, and would thus contribute to the development of scientific competence in other countries. As part of the U.S. program of international cooperation, it would further emphasize the open civilian character of the U.S. space program. (It is noted that among present NASA activities the Nimbus program already includes plans for direct read-out by weather services in other countries.)

The large observatory satellites require equipment that is far too complex for this purpose, and unavoidable delays in issuing calibrations and codes makes continuous telemetry impractical for short-lived satellites. Long-lived solar monitoring satellites now included in the NASA program are more suitable for the use of continuous telemetry, probably as a supplement to command telemetry, and the Summer Study urges that some of these satellites include continuous telemetry with appropriately timed announcements on this possibility to the international community. It is further urged that satellite projects of intermediate size and complexity be carefully reviewed for inclusion of continuous telemetry where possible.

D. Planet Patrol and Lunar Charts

> RECOMMENDATION: The National Academy of Sciences should recommend to COSPAR that the IAU through its representatives in COSPAR be asked to encourage observatories in other countries to cooperate in maintaining a continuous planet patrol, and in observing the Moon and assisting in gathering data for lunar chart construction.

The foregoing recommendation relates to two other recommendations (see Chapter Four on Lunar and Planetary Research):

1. That other nations with optical observatories cooperate in maintaining a continuous "planet patrol."
2. That the progress in making 1:1,000,000 topographic sheets of the Moon could be much enhanced by obtaining

the assistance of other countries which have observatories. The United States produces three sheets a year. More observatory time is required and more people to work on the map construction. Possible assistance in doing the photogeology of some of the sheets might also be desirable.

E. Miscellaneous

Several topics dealt with in other chapters were discussed in terms of their international aspects, but specific findings or recommendations were not considered necessary. A proposal to launch experiments developed by scientists in other countries as part of the effort to detect extraterrestrial life is included in Chapter Nine. However, since the present status of the Mars program suggests that 1966 is the earliest date by which payload space could be available, it was felt that this proposal should be deferred. A similar proposal to share secondary observing time in use of the instruments aboard orbiting astronomical observatories is contained in Chapter Two. It is suggested that this be announced only after successful launching of the observatory, and that the IAU be asked to form a committee to review proposals for inclusion in the secondary observing schedule. An increase in the number of exchange visits of scientists and students between universities in the U.S. and other countries, especially in the Soviet Union and Eastern Europe, was considered desirable. For this purpose the opportunities available in NASA research fellowships and training programs and in the exchange agreement between the U.S. and USSR Academies should be used to the fullest.

Appendix I

Report of the Meeting of the Working Group on International Relations

10-12 July 1962

The Working Group on International Relations met 10-12 July 1962. Attendance at the opening morning session (July 10) is given in Appendix IV. The principal active participants in the balance of the working group discussion were:

Porter, Richard W., Chairman
Brown, Harrison, Alternate Chairman
Brown, Allan H.
Hess, Harry H.
Kaplan, Joseph

Kellogg, William W.
Nelson, Norton
Odishaw, Hugh
Orlen, Joel
Porter, William E.
Wexler, Harry
Wood, George

The working group began its deliberations with a review of broad policy questions and a summary of present programs and activities based on the following opening statements:

1. Richard W. Porter	Why cooperation in Space Science?
2. Peter Thacher	Contribution of scientists to U. S. foreign policy objectives.
3. Arnold W. Frutkin	International Program of NASA European Organizations — ESRO. United Nations — Committee on Outer Space and its Scientific and Technical Sub-Committee.
4. Richard W. Porter	COSPAR, IQSY and WMS
5. Harry Wexler	Meteorology and WMO Report
6. Harrison Brown	Role of Academy in International Cooperation

Subsequent discussion centered around United States positions on five broad policy questions and eight specific questions and project proposals. The findings, and a summary of the discussion of each, is contained in the body of this report.

Appendix II

Governmental Structures

	International	U. S. National
1.	ESRO and ELDO	State/NASA
2.	PIACSR	NASA
3.	COPUOS	State/NASA
4.	UNESCO	State ....
5.	WMO	State/USWB/NASA/...
6.	ITU	FCC/IRAC

COPUOS	- UN Committee on Peaceful Uses of Outer Space
DOD	- U. S. Department of Defense
ELDO	- European Launcher Development Organization
ESRO	- European Space Research Organization
FCC	- Federal Communications Commission
IRAC	- Interdepartment Radio Advisory Committee
ITU	- International Telecommunications Union
NASA	- National Aeronautics and Space Administration
PIACSR	- Provisional Inter-American Committee for Space Research
State	- U. S. Department of State
UNESCO	- UN Educational, Scientific and Cultural Organization
USWB	- U. S. Weather Bureau
WMO	- World Meteorological Organization

Appendix III

ICSU and NAS Structure for International Programs in Geophysics

Program	ICSU	NAS
1. IGY	CSAGI	USNC/IGY
2. Oceanography	SCOR	CO
3. Antarctic	SCAR	CPR
4. Space	COSPAR	SSB
5. Geophysics*	CIG	GRB
6. IQSY	CIG, COSPAR	GRB, SSB
7. WMS	IUGG/IAGA, CIG, COSPAR	GRB, SSB
8. Meteorology**	IUGG (IAMAP-IASH) CIG, COSPAR	GRB (CIPASH) CAS, SSB
9. UMP	IUGG, CIG	GRB

* Other than 1, 2, 3, 4, but including completion of IGY

** Meteorology is part of all the other programs. However, an international meteorological program is emerging and involves the ICSU and NAS organizations listed above. The UN and its specialized agencies, principally the WMO, are also involved.

CAS = Committee on Atmospheric Sciences
CIG = Comite International de Geophysique
CIPASH = Committee on International Programs in Atmospheric Sciences and Hydrology
CO = Committee on Oceanography
COSPAR = Committee on Space Research
CPR = Committee on Polar Research
CSAGI = Special Committee for the International Geophysical Year
GRB = Geophysics Research Board
IAGA = International Association of Geomagnetism and Aeronomy
IAMAP = International Association of Meteorology and Atmospheric Physics
IASH = International Association of Scientific Hydrology
IAU = International Astronomical Union
ICSU = International Council of Scientific Unions
IGY = International Geophysical Year
IQSY = International Year of the Quiet Sun
IUGG = International Union of Geodesy and Geophysics
IUPAP = International Union of Pure and Applied Physics
NAS = National Academy of Sciences
SCAR = Special Committee on Antarctic Research
SCOR = Special Committee on Oceanic Research
SSB = Space Science Board
UMP = Upper Mantle Project
URSI = International Union of Radio Sciences
USNC/IGY = United States National Committee for the IGY
WMO = World Meteorological Organization
WMS = World Magnetic Survey

Appendix IV

Principal participants in the Working Group on International Relations (opening morning session of July 10) included the following:

R. E. Barry
J. C. Bellamy
H. G. Booker
A. H. Brown
E. U. Condon
E. R. Dyer, Jr.
V. Eshleman
G. Gloeckler
L. Griffis
H. Hedberg
H. H. Hess
J. Kaplan
W. W. Kellogg
G. Kessler
J. R. Macdonald
G. R. Marner
N. Nelson
H. Odishaw
J. Orlen
G. F. Pieper
R. W. Porter, Chairman
W. E. Porter
F. Seitz
S. Sternberg
P. S. Thacher
G. Thome
J. A. Van Allen
L. C. Van Atta
H. Wexler
G. Wood

NASA Contributors:

J. F. Clark
J. A. Crocker
R. F. Fellow
A. W. Frutkin
H. J. Goett
J. T. Holloway
A. Hyatt
L. H. Meredith
H. E. Newell
W. Pickering
T. L. K. Smull

CHAPTER SIXTEEN

SOME SOCIAL IMPLICATIONS OF THE SPACE PROGRAM*

I. Introduction

The accelerating rate of technical innovation, as represented by the rapid succession of programs for the use of atomic energy, missiles, and space vehicles, makes it increasingly difficult for a reflective observer or policy maker to take stock of where we are going. There is a tendency to become preoccupied with one cluster of affairs, at the expense of almost everything else that is happening at the same time. Science, itself, while comprised of innumerable specialties, also grows increasingly interdependent, and the trends within science are linked to the social context within which it works. New developments in science create new problems and are also affected by and have an impact on man's social environment. Our culture, economy, and society have already been modified in dramatic ways by these impacts, and the coming years will see vastly greater effects. The nation must face up to enduring changes in our way of life. Appropriate studies responsive to many of the questions raised in this chapter urgently need to be undertaken. Although the results of this first effort in a great many respects are preliminary and inconclusive, an attempt is made to elucidate a number of significant questions and problems which deserve the attention of officials concerned with the space program, of scientists and scholars, and of the public at large.

The original structure of the Summer Study was expanded to examine this interaction between science and society with respect to the national effort to explore outer space. Social scientists and physical scientists, each group representing a diversity of specialized disciplines, were brought together to review some implications of this interaction to attempt (within a period of four days):

(i) To pose and clarify some of the questions pertinent to economic, social, and political implications; to identify key questions that are in need of study and are amenable to systematic research and analysis — given the state of the art; and to identify those problems which may become important in the future and which, if they are to be studied later on, will require an early effort to accumulate data as base lines for later analyses and extrapolations.

(ii) To suggest some conclusions, based on our best present knowledge, which may be useful to NASA's program and its current planning.

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*See Appendix III for list of participants in the Working Group on the Social Implications of the Space Program.

(iii) To learn something about how social scientists and space scientists can work together in the future, and how to conduct the interdisciplinary studies which will be needed continually.

An initial agenda (see Appendix I) posed some of the key questions but they were not covered with equal attention: some topics received relatively detailed scrutiny, some were superficially touched upon, and others were not discussed at all. The report reflects the consensus of the Summer Study, but, just as some of the social scientists were not fully familiar with the space program, the physical scientists recognize their lack of experience with the types of social analysis used in reaching the conclusions in this Chapter. It should be realized also that time was too short for the social and physical scientists to learn sufficiently each other's language, perspective, and way of thinking, or jointly to benefit from the deliberations of other groups who had examined related problems. Some participants saw the task and judged the effort from the standpoint of the decision maker looking for the precise ''answers'' and directly applicable policy advice. Others, while appreciative of NASA's pressing problem, saw a need to make haste more slowly, given the novelty of the problems and the lack of a solid body of knowledge of social implications of science and technology generally. To the social scientists present, at least, the meeting represented a bare beginning of what will necessarily have to be a long-term continuing inquiry. They felt that discovery of the right questions is a worthwhile and necessary intellectual endeavor and that the dispelling of some of the myths and unverified assumptions is a useful initial accomplishment. Finally, it was generally agreed that mechanisms should be developed to assure continuing contact among social scientists, physical scientists, and administrative groups. Whether this contact should be accomplished through the establishment, by the Space Science Board, of a new Committee on Social Implications, or through an Advisory Committee, within NASA, on Research into Long-Term Social Implications, or both, or some other device, was not considered.

> <u>RECOMMENDATION</u>: Action should be taken to ensure that the continuing attention of social scientists is brought to bear on the nation's space program. Further discussion with interested agencies and qualified researchers should readily resolve the question of organizational arrangements.

The recent appointment of a social scientist to implement NASA's statutory obligation to conduct studies of the long-term implications of the national space program is a welcome development and should be followed by determination to activate programs more comprehensively than in the past. Since he assumed office shortly before this meeting, no discussion was devoted to his future functions. He has, however, a larger function than the conduct of in-house research or the awarding of research grants or contracts. NASA can do a great deal to interest nongovernmental research centers and university scholars to undertake useful research at no cost to the government. It also can encourage private foundations to solicit and finance projects of both primary and secondary interest to NASA.

II. Impact of the Space Program on the Economy

A. General Considerations

What we value highly, as a goal or national purpose, is influenced by the feasibility of its accomplishment, the costs involved, and the fact that to pursue one goal may obstruct or prevent the accomplishment of another. Analytical evaluation of alternative means, and their consequences for achieving postulated goals, is as necessary when considering space programs as it is for national defense or public health.

Men are exploring space for a variety of purposes, including advancement of knowledge, enhancing national prestige, and giving expression to a sense of adventure and national vitality. Not all of these purposes can be served, however, without some cost to others. The task of analyzing goals in space and alternative means for accomplishing them is complicated by the different (and unknown) rates of technical accomplishment and of resource expenditures over a long time span, during which other events — political, military, economic, social — may change the environment so that the best-laid plans are frustrated.

With these considerations in mind, a number of economic questions were discussed in relation to the space program:

(i) The Federal budget — requirements for funds over the next ten years, and the effects of the projected expenditures in relation to gross national product (GNP), and in relation to less-than-gross national effects, that is, on regional development and on special sectors of the industrial economy.

(ii) The potential contribution to economic growth resulting, over a long time span, from:

(a) practical applications of space science (e. g., communication satellites, weather satellites, etc.)*

(b) the transfer, or "spin-off," to non-space sectors of the economy of new materials, new products, and new processes resulting from the space program.

(iii) The increasingly significant division of the economy into two broad sectors: traditional enterprise operating according to market mechanisms on the one hand; and on the other, a major sector in which the only buyer is the U. S. Government and in which the usual market mechanisms are substantially absent. (Here we have a situation similar to those created by some major expenditures of the Department of Defense.)

(iv) The requirements for specialized and scarce manpower resources: scientific, engineering, and skilled supporting personnel.

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*Shortage of time prevented a discussion of the economic significance of these practical applications. NASA has sponsored some research in this field, particularly at The RAND Corporation.

B. Budget Requirements for Space and the GNP

Federal budget requirements for the space program should be viewed not in terms of a single year, like FY 1963, but rather as a continuing activity extending at least to 1970 and beyond. When viewed over this period the amounts involved change on an annual basis from the less than $2 billions for 1962 and less than $4 billions for 1963 to amounts of $11-, $12-, or $13 billions each year for the last years of the period.* Even these larger numbers are likely to prove under- rather than over-estimates if past experience with similar programs, largely military, can be taken as a guide.

Since annual gross national product is now over $550 billions, and probably will increase to $700 billions in 1970, the projected space expenditures come to less than two per cent of the total each year. In grossest terms of what the nation can afford, space expenditures do not seem on first examination alarmingly high. While space expenditures of one or two per cent of GNP seem small, the economic significance of this figure should not be underestimated. It represents something like three to five per cent of the industrial sector of the economy, and for the aerospace industries and institutes, the cumulative expenditures may represent 35 to 40 per cent.

Total expenditures over a ten-year period may reach $70 - $80 billions. Viewed in terms of this grand total, a number of circumstances could arise — serious economic recession, major failures of the program, or relaxation of international tensions — which could raise questions with respect to the worth of expenditures as large as these. In such event, arguments for and against reducing, maintaining, or increasing the rate of expenditures may involve considerations of the effect of these expenditures on the economy and the relationship between space and other national goals. For example: Is a very large space effort on behalf of scientific progress worth the cost even if the expenditures, if devoted to other purposes, would yield greater economic return? Would curtailment of space expenditures add to economic recession problems? If space expenditures declined, would the nation in fact commit them to other economy-bolstering alternatives? The effect of expenditures as large as those forecast for the space program, as well as the comparative effect of similarly large expenditures on alternative national goals, can be the subject of intelligent study provided methods of analysis are developed and necessary data are collected now.

C. The Potential Economic Contribution of Space Expenditures

Definitive answers to the foregoing questions are of extreme importance but impossible to give at present because the economic data and the research tools for analyzing such data are not now available. To establish some guidelines for looking into these questions, a number of special studies of subsidiary topics are possible.

1. Multiplier effect on GNP of space expenditures. It is important to determine the extent to which space investments are or might become a major contributor

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*See Appendix II for tables of budget projections derived from NASA and Bureau of the Budget sources.

to national economic stability and growth. Economic studies should be initiated on the "multiplier" effect of expenditures on space compared with those for military hardware, housing, education,* or other nation-wide programs. Such studies, now nonexistent, should be made to determine the comparative effects on capital formation, flow of funds and related economic impacts of NASA expenditures as compared to alternative programs. Present knowledge does not point to a foregone conclusion one way or another. The hypothesis that points in favor of greater gross economic impact of non-space programs is untested, is important to test, and may turn out to be invalid, especially in its long-run cumulative effects rather than in terms of shorter run periods when investments have not had time to mature.

2. Multiplier effect at regional or local level. There is also a significant local and regional, if not gross national, impact of such expenditures. In communities like Houston and New Orleans (Michoud), NASA expenditures will be a major direct economic force. This will also be true in communities like Los Angeles and Seattle where aerospace activities are immediate economic forces and where NASA expenditures are, or may become, a significant part of the aerospace totals.

NASA is creating major new facilities which provide opportunities for much-needed studies of what happens to a community or an area when a large new economic force is brought to bear. Such studies would have several values: First, they would expand and intensify NASA's knowledge of how to deal with a variety of problems; second, they would provide measurements of the relationship between given types of expenditures at different times; and third, an examination of the results would permit better future decisions on the size and timing of NASA's directed expenditures.

Although there has been increasing emphasis on economic studies of particular regions and localities, such studies are still a very new field and one for which techniques and data are in a formative stage. Economic impact becomes increasingly significant as we move down the scale of dis-aggregation, and, therefore, it is important that we develop more adequate tools for these analyses.

> RECOMMENDATION: Houston and Michoud should be intensively and continuously studied; they are "laboratories" of the social and economic impact of space activities. The interaction of community trends, before and after activation of the new facilities, should be assessed. The effects on manpower recruitment, attraction of supplementary space industries and other non-space capital, community facilities, housing, land values and regional "multiplier" effects — all should be measured and analyzed. Regional case studies are considered essential, both for operational use by NASA, and to develop a capability to assess economic and social consequences of NASA's activities. Comparative regional studies should throw considerable light on questions of space expenditures as a lever in the

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*The Summer Study did not attempt to lay out any detailed research design for this and other recommended studies. Research design requires further exploration of the problem and considerable time for formulation, and preferably by the people most likely to undertake the research itself.

economy, whether or how such expenditures reverse local economic stagnation or serve to intensify trends of growth already present in selected regions.

3. Transfer of space technology to general economy. The benefits from investment in space to the gross economy and to significant regional and industrial sectors may depend on the significance of practical applications derived from space science and technology, and on whether the "spin-offs" of new products, processes, and materials actually materialize in the near future — five to ten years, or less perceptibly over much longer periods.

NASA has recognized the importance of facilitating the "spin-off" process through its interest in patent policies, in making information available to the non-space sector of the economy, and in other ways. However, factors outside of NASA's control may determine the rate and scope of general industrial utilization far more than do its own actions. Expectations that major pay-offs will materialize in as brief a time period as five or ten years may be overoptimistic. Such forecasts are based on expectations that, in the near future, the new technology developed for space will increase productivity of labor and capital and stimulate new product development, that an effective demand for new products will arise, and that significant "multiplier" effects will result.

Major pay-offs will occur, but probably only for specialized and limited sectors of the economy, and then, only over a long period of time. Despite the popular impression, industry at large has been relatively slow to introduce new products arising from the atomic energy, missile, or space technologies. The reasons are many, and vary from firm to firm and among different industries. Economists and businessmen cite as the major inhibiting factor the uncertainty of the profitable return required to attract risk capital and the need for a return on investment within a relatively short time span. Industrial utilization of technical advances such as the transistor and other innovations in the relatively small electronics industry was exceptional and had limited economic consequences. Unless the government underwrites the risks of adopting new technologies, which is not expected nor advised, the "spin-off" consequences may be evident only in terms of very long time spans, say 25 years or more.

> RECOMMENDATION: NASA's efforts to speed up the transfer of space technology, products, and processes to the general economy should be continued and increased. NASA is urged not only to seek ways to encourage industrial utilization and applications, but also to appoint someone within the organization, or by research contracts to make it his business to follow in detail the actual instances in which this encouragement is acted upon or resisted. Since findings on similar problems in the past are both few and offer little grounds for optimism — again, from a relatively short-run time perspective — it is important to begin now to collect information on current experience and to analyze the factors pushing toward and against the adoption of innovations.

It should be noted that a study is being made by a group of senior business executives and social scientists at the National Planning Association under modest financial sponsorship of the National Science Foundation. The study will provide a set of preliminary findings which NASA should review with an eye to its own interests and to extension of the study. The NPA study is known as Project CARMRAND — Civilian Application of the Results of Military (and Space) Research and Development.

4. Analysis of costs in a one-customer market. Large expenditures for space activity add to a continuously growing volume of activity in which the government is the only customer and the research and development firm is the only supplier. This means that an ever-increasing portion of the nation's resources are committed on an administrative rather than on a market basis. The absence of the market test creates very real problems with respect to the evaluation of alternatives, measurements of efficiency, and similar economic decisions normally part of the market function. This situation poses new demands, particularly for the government buyer. It requires new techniques for the analysis of the various alternatives that are available, and of their costs.

The Department of Defense and the Air Force, confronted with similar situations, have undertaken to provide themselves with extensive analytical capabilities, independent of contractor figures. Techniques are sufficiently well developed so that NASA could adapt them to its own problems. A professional staff of requisite size should more than pay for itself in reduced costs and more efficient performance by contractors. We view the analytical approach as offering greater promise than in-house "pilot plants" or government-owned "yardstick" facilities.

> RECOMMENDATION: NASA should develop expert capability to estimate and analyze program costs, since NASA's suppliers are not in a competitive market, and since NASA may be the only customer.

D. Manpower Resources

The resource picture must be analyzed in component terms and not just in dollars. Probably the most significant element is manpower. Since space activities require substantial numbers of engineering and scientific personnel and of appropriate support personnel, the projected expenditures should be translated into equivalent manpower demands, and these should be measured against both the potential supply and possible competing demands in the years ahead. Manpower data now available are not particularly useful for this purpose. Gross figures cannot be applied to the specific mix of scientific and engineering talents that are required by the space program. Although the National Science Foundation has this subject under continuous review, much more work is needed.

Chapter Twelve, on NASA-University Relations, assesses a variety of steps that might be taken to ensure an adequate flow of scientific personnel into the space sciences and engineering fields. The problem is approached here from a somewhat broader standpoint.

For example, a study is needed to learn whether fellowship and scholarship incentives may be reaching the end of the road, and whether and when the long-term increments to the total pool of needed manpower through such economic assistance may be approaching the point of marginal return. Certain pervasive social trends overlie the problem of inducing qualified young people to enter or to stick with the four- to eight-year graduate study requirements. For example, the trend toward marriage at an early age is an important factor in leading potential professional people in all categories to enter the job market rather than to invest in the years of low-income graduate training, despite the fact that in the long run a person's earning power is highly correlated with advanced education.

Manpower specialists participating in this meeting, who know the limitations of existing data thoroughly, agreed on the following recommendations:

> RECOMMENDATION: NASA, in cooperation with basic data-gathering agencies (National Science Foundation, Bureau of Labor Statistics, Department of Commerce, and others) should support new studies of manpower requirements and availability to develop necessary refinements of factual information and to develop techniques for obtaining more precise data on a continuing basis.

> RECOMMENDATION: Retraining potentialities of existing groups offer a short-run possibility. We urge that NASA sponsor a study of the problems, methods, and levels of success it achieved in converting thousands of former NACA personnel to the space program.

This latter recommendation, bearing directly on space manpower needs rather than on the aggregated national problem, is especially important. Specific relationships might be established between what a man has done, is doing, and might be capable of doing, with or without retraining. This applies not only to professional levels but to critical support personnel down the line.

> RECOMMENDATION: New manpower studies, geared to present and anticipated problems, should inquire not only into the questions of utilization and retraining, but also into the potential supply for enlarging the total pool.

For example, how much untapped potential exists among special groups: women, men and women in the age brackets of 45 to 60 years, Negroes and other minority groups? What can be done to identify and then to induce the qualified among such groups to enter short-course and full-course technical training programs?

It was pointed out that the Negro population in the United States is about the size of the whole population of Canada. Profound social changes and the redevelopment of educational facilities from grade school on could conceivably, in the next twenty-five years, result in a major increase in the country's technical manpower resources.

The case studies of Houston and Michoud, recommended earlier, should include data collection and analysis of manpower problems, not only in economic terms, but in relation to the cultural and social determinants.

III. Sociological and Psychological Implications

A. Impact on Patterns of Thought

Underlying patterns of thought and belief are affected by the advent of the space age, and these changing beliefs, in turn, constitute part of the conditions in which future decisions in shaping the space program will be made. Certain major historical events and technical innovations have created major "discontinuities" in thought with consequent profound changes in outlook, customs, political and economic institutions, and in art, literature, and religion. A major example of such a "discontinuity" was the discovery and settlement of the New World beginning in the late fifteenth century. Perhaps the advent of nuclear weapons with their risk of demolishing life on Earth may constitute such a discontinuity, and the opening of outer space to human exploration may prove to be another.

For the present we are probably limited to making observations and collecting data relevant to recording the process of change in attitudes and beliefs as they occur. These changes can be observed in the general public and its representatives in terms of the degree and character of support for large expenditures for the space program, and in the scientific community in terms of its response to the changes in the organization and conduct of scientific work made necessary by the large scale of the enterprise.

B. Public Support of the Space Program

The pursuit of space objectives thus far has received remarkable Congressional support as indicated by bipartisan votes in Congress and the statements of leaders of both parties. The basis for this near-unanimous support, putting aside controversies over program details, is complex and varies, including: current high esteem in which science is held, the hope for practical applications, the desire to achieve national prestige and leadership in this most modern field, the drama and adventure of conquering the unknown, the potential contributions to national defense of space technology, and many other factors.

Despite overwhelming Congressional support, there is also reason to believe that for some people the major thrust of the space effort is a subject of indifference, anxiety, or dissatisfaction. Their worries have not yet coalesced into a major, effective, vocal opposition, but there are scattered signs of a latent uneasiness, if not direct hostility. Public opinion polls, inadequate though they may be, do not show the unanimity evidenced by the Congress. A number of prominent scientists, and people in other professions who usually are "opinion leaders" in their circles, have expressed doubts and lack of sympathy. Religious spokesmen have raised questions. Some educators are worried about the disruption of balance in education and intellectual values by the influx into universities of space-designated funds. Some students and officials concerned with international politics, fearing that the use of space for military purposes will heighten the probability of war, seek to prevent its use for any purpose other than science or practical civilian applications. Some psychologists sense that the onrush of technical change, a trend heightened by space exploration, is destroying man's sense of identity in the modern world and depriving him of a sense of belonging, is cutting off his roots in a broad culture and from traditional values.

It would be useful to have more information on who does and does not support the space program and why, and, in particular, to get some insight into possible circumstances which might lead to a sharp curtailment or increase of public support.

First and most critical: we do not have enough facts. We have no adequate collection of information on the distribution of views and intensity of conviction among various age, occupational, and other social groupings, and their behavioral consequences.

Research on similar matters is sporadically undertaken by social scientists for the sake of knowledge about society. It is not suggested that NASA should or could sponsor research across the board on all these matters, but NASA might stimulate private foundations and universities to direct their attention to some of the questions, especially because very few social scientists have yet had the imagination or interest to pursue them on their own.

> RECOMMENDATION: Private foundations, universities, and opinion research institutions should be stimulated to study public attitudes toward the space program, and particularly to collect data on opinions by age, occupation and other social groupings. The behavioral consequences of such conceptions should be elucidated.

C. The Consequences of "Big Science"

Space activities are of necessity conducted as part of a large-scale enterprise. Some scientists have expressed concern about the effect of "big science" on the creative productivity of the individual scientist resulting from the interposition of administrative judgments, and about the obstacles to access to the hardware necessary to conduct experiments based on unorthodox ideas. Educators and others are concerned over the disruption in the balance of intellectual endeavor if the relative availability of financial support for space research attracts undue proportions of bright young minds.

Some of these concerns may be valid, most probably are not. More information is required to clarify the issues. Again, these are fundamental issues which do not lend themselves to direct solution by analysis of or the collection of facts, but a few subsidiary questions are amenable to study.

1. Creative productivity. Not much is known about the relationships between creative productivity and the conditions of organization (small or large-scale). It is important to discover how to arrange work environments which maximize individual productivity. At least since the Manhattan Project, experience has been gained in a number of large-scale enterprises in which the major product has been scientific results, and the various organizational arrangements used could be critically examined for guidelines.

2. Interposition of administrative decisions. Since there is little prospect of returning to small-scale enterprise in the space program, both scientists and administrators are necessary. Studies would be useful of the various techniques and forms of managerial and decision-making functions pertinent to space programs,

particularly those related to the conduct of programs of scientific research, e.g., committee review procedures for evaluation of program direction and choice of experiments deserving support.

3. Access to hardware to test unorthodox ideas. Some scientists not in contact with space centers may represent an untapped potential. Two complementary studies might explore the extent, if any, of this problem: (i) It would be useful to have an inventory of alternative paths available to a hypothetical scientist seeking support for an unorthodox project, both within NASA and as piggy-back experiments on Department of Defense satellites. (ii) It also might prove useful to review case studies of actual problems encountered by scientists in obtaining support for space projects.

4. Funds earmarked by NASA for self-study. The activities instituted by NASA are the best and most direct source of data for developing useful information on the social implications of the space program. This conclusion is in line with the previous recommendation for studies of Houston and Michoud. The usual "project historian" functions now standard in the military services are not adequate. The research objectives should be broader in conception and purpose, to include data collection for operations analysis, and to contribute to the solution of many of the problems, such as those reviewed by this Summer Study. The project analysts on the spot should compile not only accounts of formal decisions and the acknowledged processes preceding them, but should be especially alert to the informal and otherwise unrecorded "natural history" of how the project was initially conceived, motivated, and modified in the face of new developments. The interaction between the facility and the surrounding community — economic and cultural — should be observed and described. Well-conceived programs to this end would be a significant way to implement NASA's statutory obligation to study the long-term implications of its activities.

> RECOMMENDATION: A small portion of the budget of major installations and contractor projects should be earmarked for self study with particular emphasis on the effect of scientific productivity of organizational arrangements, administrative decisions, and access to experimental facilities to test unorthodox ideas.

5. Disruption of balance in intellectual endeavor. Some argue that large expenditures on space curtail the allocation of resources to other branches of science, to social science, and to the humanities. The evidence for and against this contention is lacking. If space programs were eliminated, there is no certainty that the funds would be made available for other educational pursuits or for other public purposes. One could also contend that the general expansion of resources for science and its enhanced prestige has spilled over and enlarged the size of the pie to be divided among the larger range of claimants.

> RECOMMENDATION: The "multiplier" consequences of space science centers should be studied.

What have been the consequences for the State University of Iowa of its active involvement in the space sciences? Has the University curriculum been "distorted" as a result? What have the effects been in the recruitment of young people into the science and into other academic fields? Has the surrounding community increased its knowledge of and support for space and for the University as a whole? How have high school and even primary school curricula and teachers been affected?

6. "Post-Mortem" of the Summer Study. An analysis of the Summer Study would have the practical value of improving the organization and performance of similar study groups in the future. It also may produce a store of information about space scientists and their orientation to the space program. By a careful study of all conference records, and by intelligent interviewing of a sample of Summer Study participants, NASA participants, and Space Science Board staff, much could be learned about the meaning of this eight-week "experiment." Did the participants leave with ideas and attitudes not held by them before June 18, 1962? How will their future focus and interests be modified as a result of this experience? What objectives did they feel were accomplished and what were the difficulties and disappointments?

> RECOMMENDATION: Either NASA or the Space Science Board should sponsor a systematic "post-mortem" of the Summer Study itself.

IV. Space and Politics

The advent of the use of space vehicles has a significant impact on world politics involving both risks of conflict and competition and opportunities for co-operation. Space accomplishments are a constituent of national power and are used to bolster competitive positions in international politics. At the same time, efforts are being made to achieve cooperation in space science and applications between the United States and the Soviet Union. An entirely new subject has been added to the deliberations on disarmament, and a somewhat new type of issue has arisen in connection with the potentially harmful effects of space experiments upon other scientific activities, such as astronomy and radio astronomy. All of this makes more urgent the continued striving for consistency in the coordination of policy at the highest levels of the U. S. Government in an effort to relate policies controlling space activities to the many foreign policy problems that are affected. Some aspects of the foregoing topics are dealt with in what follows, and a number of special problems created by some ofthe new activities made possible by space vehicles also are mentioned but not discussed in any detail.

A. Space and Foreign Policy

Space is a medium into which activities of terrestrial importance have been extended. We have to understand how this newly extended dimension of human activity may facilitate, disrupt, or complicate the continuing political concerns of terrestrial society. The world politics of our time is dominated by the cold war, and the politics surrounding space activities is bound up with it, as are military issues. Besieged as all nations are with manifold non-military problems, all are affected by the struggle of the major powers, a struggle in which the possibility of war plays a prominent role. Therefore, the problems of space activities, involving a revolutionary technology, are not remote from cold war and its military backdrop, as in the case of the political implications of medical science or the conversion of sea water. Although it is impossible to forecast with precision just what military applications will develop from the present primitive state of astronautics, we do know that space technology emerged from military technology (specifically the V-2 rocket development in Germany) and that it became a cold war issue.

When Sputnik I was launched successfully on October 4, 1957, a shock hit the United States. The psychological, political, and military complexion of what was intended primarily for science suddenly became sharp and clear. The military and foreign policy authorities were confronted with substantiation of the earlier Soviet claims of an ICBM capability. Many people and their governments around the world — allies of the United States and neutrals — questioned the once unquestioned reputation of the United States as the world's leading scientific-industrial-military power. Underdeveloped areas, already predisposed to regard their problems as more akin to Soviet than American experience, were tempted to identify even more with the "backward Russians" who in 40 years were able to achieve a technical feat unmatched by the United States.

Gradually the American technical program gained momentum and recouped some of its fallen prestige. By mid-1962, the United States had successfully launched well over 75 satellites and deep space probes to some 20 for the Soviet Union. It is obvious that the extent of effort and funds devoted to the U. S. space program is influenced by the fact that the Soviet Union launched the first satellite and that this constituted a major achievement with international political implications.

A definitive objective assessment of the "space race"* is not possible with respect to "prestige" for interrelated reasons: (i) the lack of continuous, calibrated data, country by country, from 1957, on the many attributes that may be viewed as indices; and (ii) the lack of data on the distribution of attitudes and expectations within a country's population according to the role each group may play in the shaping of its politically relevant decisions.

Further complicating the task of both analyst and official is the fact that many other important events are occurring simultaneously. Events are never completely under the control of even a totalitarian super-power. Whereas nations try to guide the course of events or manipulate the unexpected to their own advantage, they differ widely in objectives, skill, information, and political doctrine as well as in physical power. Comparative international information leaves much to be desired. Thus, for example, we are not adequately equipped to assess the successes and weaknesses of the strategy adopted by the Soviet Union in exploiting its space activities for political-military purposes.

B. Cooperation with the Soviet Union

In considering the conditions under which the United States support for space activities may decline, the international political scene — particularly Soviet space activities — was mentioned as one of the important variables. The decision of Chairman Khrushchev in March 1962 to enter into bilateral negotiations with the

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*The phrase "space race" is used in its political sense, and not as a measure of comparative scientific achievement.

United States on space cooperation was an apparent, and perhaps real, reversal of the previous Soviet refusals. In his letter to President Kennedy, Chairman Khrushchev mentioned the costliness of future space activities, a comment which some have interpreted as an indicator of Soviet readiness to call off the space race.

One estimate, therefore, was that the Soviet interest in cooperation may be motivated by an effort to share the high costs of space science. At the same time there was a mounting suspicion, reinforced by Soviet boasts, threats, etc., that the Soviet Union not only will intensify its space activities but also is developing military capabilities in space.

The benefits to be derived in international scientific advancement and possibly in an international division of labor and costs justify the efforts of NASA and the U. S. Government to seek agreements with the Soviet Union to cooperate in space exploration. However, limited scientific cooperation will not, by itself, significantly diminish the cold war differences which divide East and West.

C. Control of Armaments and Space

Technical cooperation with the Soviet Union in space—on governmental or nongovernmental levels—should be distinguished from the issues involved in regulation, control, and disarmament. This is a vast area involving complex interrelated problems requiring the most careful systematic analysis. U. S. policy has emphasized efforts to reach agreement on preserving the use of outer space for peaceful purposes only. Space offers a new opportunity for a first step in disarmament, since it is easier to reach agreements in an area where armaments do not exist than in areas where they do. Some argue that the development of military applications in space would automatically destabilize the existing over-all strategic equilibrium and create a situation of unmanageable conflict. This assumption is questionable, and studies are needed particularly to assess the probability that militarization in outer space would tend to increase the chances of a major war.

The possibilities of military applications and uses probably will increase rather than decrease as research and development proceed. Investigations of military defense applications of space technology and solid analytical evaluation of the military and political worth of such capabilities have become increasingly necessary. The outcome of these investigations could entail a greater emphasis on military applications with some de-emphasis of civilian efforts. Pending the results of these analyses, cooperation between NASA and the DOD, as required by the National Aeronautics and Space Act of 1958, needs to be continued and extended particularly through increased liaison and joint planning.

D. Space Experiment with Potentially Harmful Effects

A few experiments conducted in space have been considered by some scientists to be potentially harmful to other scientific activities. To date the most striking example of such experiments have been projects conducted for military purposes, some of the results of which, quite independent of the main objective of the experiment, have been of interest to science. These included the Argus experiment, Project West Ford, and the Johnston Island high-altitude nuclear tests.

Other experiments conducted for scientific purposes also may be potentially harmful because of the cumulative effects of introducing into the atmosphere additional small amounts of such materials as sodium vapor. Furthermore, satellite components of space communications systems, such as the Echo balloon or active communications satellites, might eventually become numerous enough to cause radio interference with other scientific activities, e.g., radio astronomy. These experiments, both scientific and applied, are generally reported in the scientific and technical literature, so that their potentially harmful effects can be discussed openly by the international scientific community. Decisions concerning the release of information on military space activities is the prerogative of military authorities, in consultation with scientists conducting the experiments. The larger scientific community of the country launching the experiments may not always be informed until the effects of the experiment become obvious.

Experiences with the three military space activities mentioned above indicate that no rigid formula can be established for handling similar cases in the future. Information on the Argus experiment was not made public until after the experiment had taken place. Some scientists were critical of the potential effects of this experiment but such criticism was not extensive. Information on Project West Ford was released to the scientific community before the experiment was conducted. A considerable amount of controversy followed the release of this information, and a critical resolution was adopted at the August 1961 Congress of the International Astronomical Union. Since the belt of needles never went into orbit, however, this case does not provide a full test. Information was released in advance on the Johnston Island high-altitude experiments which would permit scientists from abroad to observe their effects. Some scientists were critical of the potential effects, but, again, this criticism was not extensive.

Following the action of the International Astronomical Union, the International Council of Scientific Unions requested its Committee on Space Research (COSPAR) to establish a consultative group on potentially harmful effects of space experiments. This group was instructed to seek competent quantitative analysis of future experiments in which there was serious concern on the part of the scientific community. The existence of the consultative group should increase the likelihood that, in the future, discussion of such problems will take place in a forum of appropriately broad scientific competence and require that criticism be accompanied by factual arguments based on quantitative calculations or estimates. The assistance of such a competent forum could be of considerable advantage to the scientific community. Scientific groups planning to launch experiments which have become the subject of controversy can be assured that arguments based on emotional or political considerations will have to meet the test of sound factual analysis. While a mechanism now exists for any national scientific institution to request a review by this Consultative Group, there is no obligation on the part of the country launching the experiment to initiate a review of the potentially harmful consequences; however it should be noted that planned experiments, if known, can be reviewed or criticized by scientists abroad whether or not they are invited to do so by the government launching the experiment. It is clearly impossible for members of the scientific community to enter into any arrangement involving obligation to solicit international review of potentially harmful effects of experiments essential to national defense; the existence of a review mechanism involves a danger that such an obligation might be established by precedent.

E. The Need for a Coherent National Strategy on Space Matters

In the nature of the American political process, national policy evolves from the pursuit of a multiplicity of values, interests, and competing demands on resources. We do not plan policy from the top to the extent that is characteristic of a totalitarian society.

Since what the nation as a whole does in space has international political (foreign policy) consequences, whether intended or not, it is important to anticipate these consequences as realistically as is possible so as to guide them in favorable directions and to minimize undesirable repercussions.

The American process of evolving policy, rather than planning it, encourages government agencies to push agency-sized programs and points of view rather than to submerge their specific objectives in considerations of national strategy. The Executive and Congressional levels of authority have a responsibility to reconcile differences and establish compatibility. The advantages inherent in our system are not questioned, but neither is the price we sometimes pay — especially in the foreign field — easily dismissed.

Foreign governments — allies, neutrals, and opponents — observe the seeming multiplicity of pronouncements and actions of agency officials and, lacking the sophistication to understand our system and its pluralistic interests, find it difficult to deduce what the national position is. The costs are visible enough in the reactions of foreign governments, especially among allied and neutral nations: an attribution of vacillation and unsteadiness to the United States and a lack of confidence that we will abide by earlier commitments, that we mean what we say. But costly as our way of evolving policy sometimes may be, it also has its advantages. The chances are reduced that we will adhere rigidly to a given interpretation of events in the face of changed circumstances; we are better able to adapt to new conditions. The Soviet Union must, therefore, find it difficult to forecast American actions and reactions — as Stalin discovered in our responses to the Berlin blockade in 1948 and in our intervention in Korea in 1950, for example — and thus may move with greater caution.

In recognition of both the costs and advantages of the American approach to world politics, we have been able to make a virtue out of traditional necessities; for example, the need to accede to pressure for maximum public disclosure. The opening to world view of the Project Mercury launches was a decision that probably could not have been avoided. The American practice of disclosure stands in sharp contrast to Soviet practice.

The United States probably has derived advantage from its programs of international cooperation in space research, exchange of information, training of foreign space scientists, and related international programs. This willingness to share the benefits and to encourage foreign participation stands in sharp contrast to the Soviet unilateral program.

To take full and continuing advantage for foreign policy purposes of space activities undertaken for other purposes (scientific, military, etc.), requires some degree of prior planning.

The usual mechanisms of "interdepartmental" coordination and information exchange should be further improved to derive maximum foreign policy gains or to minimize political risks. The political branches of the government have their own lead-times to consider, as do technical agencies.

Perfection in interagency cohesion on behalf of major foreign policy and national security objectives certainly has not yet been reached. Early in the research and development process there should be increased working-level contact on potential political opportunities and costs, the better to anticipate and prepare for them.

This involves more than informal or formal exchange of information. It requires intensive working-level contact on problems, and a practice of jointly drafting "contingency plans," as a means for each agency involved to obtain a deeper sense of understanding of the goals and problems of the others. While coordination now exists, NASA should take further initiatives in this direction with the Department of State and with the appropriate agencies in the Department of Defense. This recommendation is made with awareness of the operations of the National Aeronautics and Space Council. The essential point is that there is room for additional efforts and that NASA might play a more active role in this respect. A pattern of continuing joint efforts should be assured so that we can learn how to achieve coherence before rather than after we are overtaken by events.

F. Other International Political Implications of the Space Program

Lack of time prevented a consideration of a number of developments and issues in which space and foreign policy intersect. These are important problems, and are mentioned here for the record without discussion or findings:

(1) Retention and acquisition of overseas ground facilities.

(2) The terms and conditions under which foreign communication agencies (usually government bodies) will participate in U. S. communication satellite systems.

(3) The international political, economic, and cultural impact of communication satellites.

(4) The international political, economic, and military implications of weather prediction, control, and modification by space means.

(5) The role of the United Nations and its specialized agencies — e.g., the International Telecommunications Union and World Meteorological Organization — in communication satellite and meteorological satellite programs.

(6) The implications of internationally constructed and operated launch facilities.

(7) The "N-th country problem" in space: the consequences of the independent European Launcher Development Organization (ELDO), and the European Space Research Organization (ESRO); the political implications of space and rocket capabilities of other countries.

Both with respect to the problems discussed and those merely mentioned, there is need for research by space and social scientists on an interdisciplinary basis. While many of the problems have been scrutinized within the government, NASA should raise the level of its support for longer-term studies. In many cases it will be up to NASA to pose problems and to seek out potentially qualified investigators. NASA must share its technical and operational knowledge with outside researchers, while not restricting their freedom and scope of inquiry. As was mentioned earlier, the appointment of a qualified social scientist to foster an enlarged research program on social implications is welcome, and it is urged that he be given ample support and backing so that much-needed studies can be gotten underway across a broad front with minimum red-tape and delay.

Appendix I

Agenda for Working Group on Social Implications of the National Space Program

Prepared by the Chairman, June 27, 1962

Topics For Discussion

A. Economic Considerations

1. The trend of space expenditures.
    - (a) How much? Necessarily upward?
    - (b) Governmental versus private sources of funds.
    - (c) Where does the government money go?

        Industry
        Universities
        Government (''in-house'')

    - (d) Manpower involved
        - How well utilized?
        - Future needs?
2. What do we get from the expenditures?
    - (a) How much is ''science'' and how much technology and supporting services?
    - (b) Will the trend continue of an increasing ratio of hardware equipment (for experiments and for boosters) in relation to ''units'' of scientific gains or number of scientists doing scientific work?
    - (c) Do we see shortages of dollar and human resources?
3. Can cost in relation to results be reduced? How?
    - (a) Recoverable boosters?
    - (b) Cheaper boosters?

(c) Managerial improvements, such as shortened check-out time?

(d) Airborne launchers?

(e) Centralization of government space agencies?

(f) Other approaches to more efficient allocation and utilization of resources?

4. Implications for the national economy

(a) Fiscal implications.

(b) On the "balance" of the economy and rates of economic growth.

(c) Manpower, characteristics (new specializations, phasing out of conventional skills, etc.)

(d) Transferability of scientific results and new technologies to the conventional economy.

(e) On economic institutions:

- The emergence, enlargement, and separation from the market, of whole new industries and specialized manpower pools.
- The dependence of the space sector of the economy on one customer: Uncle Sam.
- With government paying the bills, is there and will there be a neglect by private groups of self-financed R&D? Of inventiveness in meeting new economic problems? Of developing new products and new mechanisms of financing, pricing, distribution?
- Effects on national economic policy: the interdependence between "free" "private" enterprise and government — the question of freedom, authority and power; the Eisenhower farewell warning of a military-industrial power elite.
- Effects on the economics of higher education and the trickle-down influence on educational economics at high school and grade school levels.

5. Research needs on above issues.

These might range from the immediate, practical need for a way to improve cost estimates of space programs, to acquisition of better statistical data, to more complex projections of broad economic and institutional consequences.

6. Policy recommendations.

B. The Social Setting and Organization of Space Activity

1. The appearance or reality of national support for the space program?

   (a) Among scientists themselves?

   (b) How exceptional is the position stated in Dr. Tuve's letter?

   (c) What are the key issues between small science and Big Science?

   (d) Why the relative absence of a national debate on the worth of the space effort? If there are skeptics about the worth, why aren't they more vocal — in scientific, journalistic, public, and Congressional forums?

   (e) Could this Summer Study Group itself serve as an anthropologist's laboratory for elucidating the efforts of space-sponsors to mobilize support, to widen the participation of scientists in space-connected activities, and to enlist their interest by asking them to evaluate program details?

   (f) Does the availability of money for space research influence entry into the field by scientists and universities? If money were available for everything, would there be so much scientific interest and enthusiasm for space?

2. What do large-scale space programs do to science and the scientist?

   (a) In the space sciences themselves, can individual creativity be nurtured and encouraged in circumstances which necessitate enormous investment in equipment, "team" or "group" research, time for administrative chores, political pressures, etc. ?

   (b) How does the scientific work get done when so many outstanding scientists spend so much of their time negotiating contracts, conferring with administrators both in Washington and in their universities, recruiting staffs, attending conferences, etc. ?

   (c) What effects do such administrative preoccupations have on the quality of scientific work? On the training of a new generation of scientists?

   (d) What consequences result from the multiple roles played by the senior scientist who often is an advocate or applicant for a pet project, yet sits on interlocking advisory committees which review proposals and suggest new ones? Or as Van Allen put it, how can we preserve and protect the motivation and professional integrity of the individual scientist?

   (c) Are <u>other</u> important areas of science under-valued, under-supported and under-trained for as a result of heavy support for space research?

3. The new prestige and status of science.

   (a) The image of the scientist in American society: in government, industry, general public, among the youth, among scientists themselves.

   (b) The transfer of prestige earned in science to other tasks on the assumption that the scientist's advice is equally expert.

   (c) The care or carelessness with which some scientists define the boundaries of their competences in playing a role in public or in rendering advice to policy-makers.

4. Implications for education and the youth.

   (a) Has the heightened respect for "science" been reflected in greater respect for intellectual life and values generally? Or is it more the momentary glamor of a Colonel Glenn that becomes the model for the younger generation?

   (b) Will youth surge into scientific careers in response to illusions of a glamorous career? Will social-economic and political pressures and, incentives to enter science and engineering create unbalance in our cultural life: i.e., siphon away talent and resources from the humanities, social sciences, and other professions and vocations?

   (c) Should NASA be the medium for heavy funding of educational opportunities, or will it better achieve a national purpose by contributing to a broader-gauged agency or program of educational support?

   (d) What can NASA and other agencies do, as part of their legislative mandate, to foster greater awareness and knowledge of the non-technical consequences of their primary activity? How can we stimulate social as well as technical creativity in the solution of economic, social, political and administrative problems surrounding the space effort?

5. Space achievement and man's orientation in the universe he expanded.

   (a) What happens to man's sense of purpose, his conceptions of the universe, his hopes and his fears as the space sciences confront him with new worlds?

   (b) If even the sky is no longer the limit, and man no longer can believe he is unique in the universe, what profound shifts may we anticipate in his fundamental conceptions of progress, of the limits of his powers to change anything, his belief not only that anything can happen but perhaps should?

   (c) At the same time that science fulfills the modern conception of progress, will tension and fear increase in a world whose weather we can modify, whose far-off inhabitants we discover, whose Earth-bound population is in revolutionary ferment, and whose very own planet is capable of self-extinction?

(d) What requirements or outlines can we discern to replace a shaken confidence that science can save us and to instill confidence in man's ability to match the mastery of his natural environment?

C. <u>Space and the Cold War</u>

1. The "space race."

   (a) From V-2 to ICBM's: U.S. and S.U.

   (b) The IGY Rocket and Satellite Program.

   (c) Sputnik I and the Soviet political exploitation of its "spectaculars."

   (d) The American response: NASA (1958) and the build-up of a broad-front scientific program.

   (e) Cuba, Laos, and Gagarin in the Spring of 1961: the big jump in the U.S. effort.

   (f) 1962-1970: still a "race"?

2. International cooperation and conflict.

   (a) Scientific, technical and economic motivations.

   (b) Political value vs. political-military risks and constraints

      — as seen by the Soviets

      — as seen by various groups in the U.S.

   (c) American bilateral and multilateral cooperative activities.

   (d) Intra-European cooperation: a future "third force" in space?

      — ELDO and ESRO

3. Arms and space control.

   (a) The efforts in the UN:

      — The Committees on the Peaceful Uses of Outer Space

      — The Disarmament Committees and negotiations

   (b) The attitudes and role of scientists

      — Is what is good for science necessarily good for U.S. foreign and defense policy?

  - National decisions or international authorization for national programs — e. g., West Ford, high altitude nuclear tests, contamination of space or celestial bodies, etc.

4. Our needs in the politics of science and the science of politics.

   (a) Requirements for a cohesive national policy or "strategy": a problem of optimizing multiple purposes and values

      - National Aeronautics and Space Council
      - NASA
      - Department of Defense
      - Department of State
      - U. S. Information Agency
      - Congress
      - Bureau of the Budget
      - NSF, etc., etc.

   (b) The research needs: within the social sciences and the need for transdisciplinary collaboration.

   (c) Recommendations.

Appendix II

Space Budget Appropriations and Estimates

TABLE 1

Space Science

NASA Appropriations Fiscal Years 1961 - 1963*
(Millions of Dollars)

	FY 61	FY 62	FY 62(Sup)	FY 63
Research, Development & Operation				
Sounding Rockets	$ 12	$ 14	$ --	$ 19
Scientific Satellites	54	118	--	175
Lunar & Planetary Exploration..	91	170	--	274
Scout.........................	10	8	--	9
Delta.........................	10	3	--	--
Centaur.......................	65	66	9	67
Total RD&O..............	$242	$379	$ 9	$544
Construction of Facilities............	9	27	--	12
TOTAL Space Science.....	$251	$406	$ 9	$556

TABLE 2

Applications

NASA Appropriations Fiscal Years 1961 - 1963**
(Millions of Dollars)

	FY 61	FY 62	FY 62(Sup)	FY 63
Research, Development & Operation				
Meteorological Satellites	$ 20	$ 54	--	$ 51
Communication Satellites.......	34	49	--	85
Total RD&O..............	$ 54	$103	--	$136
Construction of Facilities............	4	4	--	3
TOTAL Applications......	$ 58	$107	--	$139

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*From 1963 NASA Authorization, Hearings before the Committee on Science and Astronautics, U. S. House of Representatives, 87th Congress, 2nd Session, Part I, U. S. Government Printing Office, Washington, 1962 (page 112).

**Op. cit., p. 117.

TABLE 3

Manned Space Flight

NASA Appropriations for Fiscal Years 1961 - 1963*

(Millions of Dollars)

	FY 61	FY 62	FY 62(Sup)	FY 63
Research, Development & Operation				
Mercury	$124	$ 68	--	$ 13
Advanced Manned Space Flight	6	147	--	864
Saturn C-1	174	282	--	249
Advanced Saturn	1	28	$ 50	335
Nova	--	6	--	164
Total RD&O	$305	$531	$ 50	$1625
Construction of Facilities	53	180	71	635
TOTAL Manned Space Flight	$358	$711	$121	$2260

TABLE 4

Advanced Research & Technology

NASA Appropriations for Fiscal Years 1961 - 1963**

(Millions of Dollars)

	FY 61	FY 62	FY 62(Sup)	FY 63
Research, Development & Operation				
Aircraft & Missile Technology	$ 38	$ 41	--	$ 53
Spacecraft Technology	27	37	--	54
Launch Vehicle Technology	14	23	--	32
Launch Operations Development	--	2	--	21
Nuclear Systems Technology	25	50	--	123
Electric Propulsion	7	18	--	31
Liquid Propulsion	73	104	$ 26	163
Solid Propulsion	2	4	--	8
Space Power Technology	9	15	--	20
Total RD&O	$195	$294	$ 26	$505
Construction of Facilities	28	29	--	114
TOTAL Adv. Res. & Tech.	$223	$323	$ 26	$619

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*Op. cit., p. 104.

**Op. cit., p. 124.

TABLE 5

Summary of NASA Appropriations Fiscal Year 1961 - 1963*
(Millions of Dollars)

	FY 61	FY 62	FY 62(Sup)	FY 63
Space Sciences......................	$251	$ 406	$ 9	$ 556
Applications........................	58	107	--	139
Manned Space Flight.................	358	711	121	2260
Advanced Research & Technology.....	223	323	26	619
Tracking & Data Acquisition..........	76	124	--	212
TOTAL......................	$966	$1671	$156	$3786

TABLE 6

Projected Space Financial and Manpower Requirements**

	Total(a) Expenditures (Billions of $)	Total Contractor(b) Manpower (Thousands of men)	Total Contractor(c) Engineering and Scientific Personnel (Thousands of men)
FY 1960.........	$ .8	32	5
1961.........	1.5	62	9
1962.........	2.4	100	15
1963.........	4.2	171	26
1964.........	6.2	252	38
1965.........	7.8	317	48
1966.........	9.3	378	57
1967.........	10.6	431	65
1968.........	11.7	476	71
1969.........	12.5	508	76
1970.........	13.0	528	79

(a) Also see Table 7.
(b) Prime contractors only.
(c) Those persons engaged in scientific or technical duties which require formal education or its equivalent, such as aerodynamists, physicists, chemists, electrical engineers, mathematicians, mechanical engineers, metalurgists, thermodynamicists, etc.

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*<u>Op. cit.</u>, Part I, page 127.

**<u>Op. cit.</u>, Part II, p. 1034, Manpower estimated on the basis of a RAND CAD unpublished set of manpower cost factors.

TABLE 7

Actual and Projected R&D and Space Expenditures *

(Billions of Dollars)

Year	Total R&D	Space	Year	Total R&D	Space
1945	1.5	--	1958	10.5	0.09
1946	1.7	--	1959	12.0	.59
1947	2.0	--	1960	14.0	.79
1948	2.5	--	1961	16.0	1.53
1949	3.7	--	1962	17.8	2.45
1950	3.1	--	1963[a]	19.3	4.2
1951	3.5	--	1964[a]	21.6	6.2
1952	4.0	--	1965[a]	23.6	7.8
1953	4.7	0.08	1966[a]	25.8	9.3
1954	5.5	.09	1967[a]	27.8	10.6
1955	6.3	.07	1968[a]	29.5	11.7
1956	7.5	.07	1969[a]	31.0	12.5
1957	9.0	.08	1970[a]	32.6	13.0

[a] Estimated.

TABLE 8

Funding and Performance of Research and Development, 1958-62**

(Billions of Dollars)

Year	Funding		Performance		
	Industry	Government	Industry	Government	Total R&D
1958	3.8	6.7	8.4	2.1	10.5
1959	4.8	7.2	9.5	2.5	12.0
1960	6.3	7.7	11.3	2.7	14.0
1961	6.8	9.2	13.1	2.9	16.0
1962 (estimate)	7.2	10.6	14.6	3.2	17.8
Percentage Analysis					
1958	36.2	63.8	80.0	20.0	100.0
1959	40.0	60.0	79.2	20.8	100.0
1960	45.0	55.0	80.7	19.3	100.0
1961	42.5	57.5	81.1	18.1	100.0
1962	40.4	59.6	82.0	18.0	100.0

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*From 1963 NASA Authorization, Hearings Before the Committee on Science and Astronautics, Part II, U. S. House of Representatives, 87th Congress, 2d session, Part II, U. S. Government Printing Office, Washington, 1962 (page 1034).

**Op. cit., Part II, Page 1035.

TABLE 9

Funding and Performance of Space Effort, 1958-62*

(Billions of Dollars)

Year	Funding		Performance		
	Industry	Government	Industry	Government	Total R&D
1958	(a)	0.09	0.01	0.08	0.09
1959	(a)	.59	.24	.35	.59
1960	(a)	.79	.38	.41	.79
1961	(a)	1.53	.95	.58	1.53
1962	(a)	2.45	1.65	.80	2.45
Percentage Analysis					
1958	--	100.0	11.1	88.9	100.0
1959	--	100.0	40.6	59.4	100.0
1960	--	100.0	48.1	51.9	100.0
1961	--	100.0	62.1	37.9	100.0
1962	--	100.0	67.3	32.7	100.0

(a) Negligible amounts.

*_Op. cit._, Part II, p. 1035.

TABLE 10

National Aeronautics and Space Administration
Fiscal Year 1963 Estimates

Identification of Research, Development, and Operation Programs with President's Budget Activities

Budget Activity	Research, Development & Operation Programs	Fiscal 1963 Estimates, millions
1. Manned space flight:		
a. Spacecraft development & operations	Mercury	$ 13.3
	Advanced manned space flight	863.6
b. Launch vehicle development	Saturn C-1	249.2
	Advanced Saturn	335.2
	NOVA	163.6
2. Space applications:		
a. Meteorology	Meteorological	51.2
b. Communications	Communications satellites	85.4
3. Unmanned investigations in space:		
a. Spacecraft development & operations	Sounding rockets	19.2
	Scientific satellites	175.2
	Lunar & planetary exploration	273.6
b. Launch vehicle development	Scout	8.9
	Delta	.3
	Centaur	66.7
4. Space technology		
a. Launch vehicles & spacecraft	Spacecraft technology	54.1
	Launch vehicle technology	31.7
	Launch operations development	21.5
b. Propulsion & space power	Electric propulsion	30.6
	Liquid propulsion	163.1
	Solid propulsion	7.9
	Space power technology	20.1
	Nuclear systems technology	123.0
5. Aircraft & missile technology	Aircraft & missile technology	52.6
6. Supporting operations	Tracking & data acquisition	158.4
	TOTAL	$2,968.4

NOTE: Extracted from NASA budget estimates, fiscal year 1963.

TABLE 11

Space Activities of the United States Government*
New Obligational Authority/Program Basis -- in millions

Historical Summary and FY 1963 Budget Recommendations

	NASA[a]	Defense[b]	AEC[c]	NSF[d]	WB[e]	Total
FY 1955	$ 56.9	$ 3.0	---	---	---	$ 59.9
FY 1956	72.7	30.3	$ 7.0	$7.3	---	117.3
FY 1957	78.2	71.0	21.3	8.4	---	178.9
FY 1958	117.3	205.6	21.3	3.3	---	347.5
FY 1959	338.9	489.5	34.3	---	---	862.7
FY 1960	523.6	560.9	43.3	.1	---	1,127.9
FY 1961[f]	926.2	793.8	63.2	.6	---	1,783.8
FY 1962	1,630.3	1,147.2	120.1	1.6	$50.2	2,949.4
Recommended supplemental	156.0	---	---	---	---	156.0
Total 1962[f]	1,786.3	1,147.2	120.1	1.6	50.2	3,105.4
FY 1963 budget[f]	3,732.9[g]	1,517.7	192.9	1.7	47.2	5,492.4

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a National Aeronautics and Space Administration amounts for 1961 and subsequent years exclude amounts for aircraft and missile technology. Amounts for 1960 and prior years are totals for all activities of NASA and include totals for NACA prior to establishment of NASA.

b The Department of Defense amounts include identifiable Defense funding for space and space-related programs, but exclude the cost of developing missiles and related equipment which are also used in the space programs, military personnel costs, and certain operating and supporting costs which it is not feasible to separate from other military expenses. Amounts for 1960 and prior years also exclude construction and operating costs of the national missile ranges and certain supporting research and development applicable both to space and other programs.

c Atomic Energy Commission amounts are those identified with Rover nuclear rocket, the SNAP atomic power source project, and related activities.

d National Science Foundation amounts are those identified with Vanguard and with the NSF space telescope project.

e Weather Bureau amounts are those identified with the meteorological satellite program.

f These amounts used in table on "New Obligational Authority for Federal Space Programs" on page 329 of the 1963 budget.

g Rounded to $3.8 billion on Bureau of the Budget chart.

Bureau of the Budget January 19, 1962

*United States Aeronautics and Space Activities, 1961, 87th Congress, 2d Session, House Document No. 324, U. S. Government Printing Office, Washington, 1962. (Page 107 of Appendix D).

Appendix III

Principal participants in the Working Group on Social Implications of the Space Program included the following:

Regular Participants

Goldsen, Joseph M., Chairman
Brodbeck, Arthur
David, Henry
Morgenthau, Hans J.
Novick, David
Orlen, Joel
Porter, William E.
Saunders, Harold
Steg, Leo
Barry, Robert E. - Rapporteur

Occasional Participants

Booker, H. G.
Cudaback, David D.
Eshleman, Von R.
Griffis, LeVan
Hedberg, H. D.
Kellogg, William W.
Van Allen, James A.
Van Atta, L. C.

NASA Contributors

Barre, Robert L.
Frutkin, Arnold W.
Hagen, John P.
Jastrow, Robert
Levine, Arthur
Meredith, Leslie H.
Newell, Homer E.
Smull, Thomas L. K.

Air Force Contributor

Colchagoff, George D.

Supplement
BRIEFINGS
on the
NASA SPACE SCIENCE PROGRAM

SUPPLEMENT TO

REPORT OF THE SPACE SCIENCE SUMMER STUDY

As noted in Chapter One, the Space Science Summer Study opened with a series of briefings on existing space science programs and the existing procedures and policies used for their implementation. Since the Summer Study was mainly focused on the NASA Space Science Program, we have reproduced in the pages that follow only the NASA briefings, listed in Section A below. Briefings were also given by representatives of other agencies, and these are listed in Section B below, but not printed in this supplement.

A. NASA Briefings

B. Briefings by Other Agencies

Department of Defense Program, by L. Allen

Army Program, by R. M. Hurst

Navy Program, by W. Berg and W. E. Wright

Air Force Program, by H. L. Evans and C. Stergis

Atomic Energy Commission:
    Space Power Supplies, by G. M. Anderson
    Nuclear Rocket Propulsion, by H. R. Schmidt
    Biomedical Research Program, by N. Barr and J. Liverman
    Research Program in Physical Sciences, by A. R. Van Dyken

National Science Foundation Program, by W. Koltun and L. Karel

National Bureau of Standards Briefing, by K. Kessler and R. J. Slutz

PROGRAM OF THE OFFICE OF SPACE SCIENCES

By Homer E. Newell

Much of this presentation will be devoted to a discussion of the philosophy and policies that are followed in the Office of Space Sciences. First, the principal objective of the space science program is the extension of knowledge. In other words, the program is a basic research program. At the same time, there is a secondary objective, and that is to obtain environmental data on the atmosphere and space for other users.

In the space science program, we undertake problems which the tools of rocketry make it possible to attack, but which also are important scientifically. Among these problems are the study of the Earth as a planet; the study of Earth-Sun relationships; the investigation of the solar system (that is, the planets, planet-Sun relationships, interplanetary medium, and so on); the investigation of the universe (for example, extension of astronomy to the wavelengths that cannot be observed from the surface of the Earth); the collection of data for cosmological studies; and cosmic scale experiments (for example, gravity-type experiments). Finally, we undertake studies of life in space, by which we mean both the search for extraterrestrial life and the study of terrestrial forms of life carried out into space.

The phrase "space science" has come into being in the last several years, and even has acquired some aura of mystery about it in the mind of the layman. But, to the scientist, it is clear that space science is just science in space, and that there is nothing mysterious about it. Because of its breadth, this science in space has many ties with the whole spectrum of scientific activity. This association often leads to requests for support of activities which are not really science in space. These requests for support are very tempting to the scientists in NASA, and we would very often like to provide this assistance. The justification for this support is often very convincing; indeed, no one need argue with those in NASA that it is necessary to provide broad support for these other activities. Nor is it necessary to argue that the support of good ground-based research is more worthy than the support of mediocre flight experiments. And we try to avoid supporting mediocre research.

However, I would like to make the point that NASA resources are limited, even though large, and that when choices must be made, we must have a basis for making selections and for making decisions. This need which leads to my initial statement that our program focuses on the researches which can be advanced by the use of the unique tools of rocketry available to NASA and which, I reemphasize, also meet the test of being scientifically important.

Earlier descriptions of the NASA program had headings or labels which referred to these basic tools, and these headings were used in the budget breakdowns.

For example, there were the headings "sounding rockets", "scientific satellites", and "lunar and planetary probes." We have moved away from these characterizations and now describe the program in our budget breakdown in terms of scientific areas, namely, geophysics and astronomy programs, lunar and planetary programs, and space bioscience.

In developing our program, we attempt to achieve a balance between what we can do now with the reliable tools that are available, and the preparation of new tools for future needs. We are trying to establish on-going programs based on the use of reliable systems, such as the Delta and the Thor Agena, now while, at the same time, carrying on new developments such as the Centaur for our future needs. At the same time, we are scheduling the use of the reliable and proven vehicles far enough into the future that there will be a significant overlap during the expected development period. Historically, we have not been able to apply this policy completely. In the course of the development of these vehicles and spacecraft, you will recognize that there must be frustration and temporary failure—and that one must live through these frustrations in the process of developing the tools.

At the same time it is clear that we need successes to keep our science moving and, from a practical point of view, to maintain adequate support for the program by the Congress and the Nation. Hence, we are continually reviewing our program to evaluate the balance or the imbalance between the on-going program and preparations for the future program. It must be recognized that there is a rather long time constant that has to be reckoned with if major changes are to be introduced. A review of the program must be made with due consideration for the long time constant needed for making changes and with consideration for the present plans and actions of the Office of Space Sciences.

For example, we are making recommendations to the Administrator that the dependence of the early planetary and interplanetary programs on Centaur development vehicles be lessened considerably and that the Centaur continue mainly as a development effort for some time. We recommend that the vehicle only be used operationally much later when it is in better shape. Missions based on more reliable vehicles are being introduced to make up for this removal of operational missions from the Centaur. Pioneer V type missions are being recommended to the Administrator. We plan a continued use of the Orbiting Solar Observatory in order to realize its maximum potential for space science study. A long series of Delta vehicles has been scheduled in our long-range plan, extending throughout this decade. When the relatively simple Scout vehicle has been proven, it is our intention to use it extensively.

As another policy in the Office of Space Sciences, we attempt to build on the participation by the competent scientific community. It is thoroughly recognized by the Office of Space Sciences that we would have no program worth talking about or attempting to sell if the competent scientists of the Nation were not involved in it. The importance of the individual scientist to us is paramount. This scientist comes from the scientific community outside NASA and inside NASA. The university community is a major element and has an exceedingly important role since in addition to conducting research, it is the one element of the whole program which reproduces itself and is involved in the training of new scientists. Other Government participation is also significant and important. We feel that NASA in-house competence is essential to the program not only in technology and engineering, but also in the sciences.

We have found that in the international community there is tremendous interest in space research and we feel that the development of participation in the program by scientists in other countries is important. I would emphasize that we seek the participation of the competent scientist wherever we find him—in the University, in NASA, other Government agencies, industry, and the international community.

Those who participate in the space science program are always interested in the policies concerning the use of the data obtained. Traditionally, a scientist conceives, prepares for, and carries out his experiment, analyzes the data, and publishes his results, assuming responsibility for what he has done. The question always arises as to whether the NASA policy on the use of data will support this traditional responsibility on the part of the scientists. The NASA has issued management instructions on the use of data obtained from NASA-supported experiments.

The first section describes the purpose. This instruction defines the procedures to be followed in relations with individual scientists or groups participating in the NASA space sciences program with regard to release and publication of experimental data obtained in NASA sounding rocket satellites, space probes, or other spacecraft operation.

Another section describes the scope. First, scientists as a group feel a responsibility for making the results of their efforts known to the worldwide scientific community. Scientists have been the most vigorous proponents of free dissemination of the knowledge gained from their investigations and, in general, can be expected to release experimental scientific information rapidly. Second, in the usual case, an agreement that will assign the responsibility for the analysis and publication of the data to the scientist will exist between NASA and the scientist. In the period before the publication of the results of the experiments it may happen that another scientist may wish to have access to the experimental data in its preliminary form. This is generally arranged informally by a request made directly to the experimenter. Next, occasions may arise when it will be necessary for NASA to release the results of experimental investigations made under its sponsorship without waiting for publication by the cognizant scientist. It is expected that this will happen only when the responsible scientist is unable to meet his commitments within a reasonable time. Next, in many instances the data acquired from space vehicles, such as the observations made in astronomy, will have a cumulative value. Whenever this is the case, it is desirable that such data be made available for the general use of the scientific community. Next, the original data received from a satellite or space probe is meaningless until the various channels are demultiplexed, the calibration of the instrumentation is introduced, and the data are arranged in chronological order. Provisions for these operations must be included in the arrangements made for supporting the applicable experiments.

The final section states the policy. First, the original data recordings received from a satellite or probe shall be stored at the cognizant NASA field installtion. Second, secondary records containing reduced data shall be prepared under the cognizance of the responsible NASA field installation. On occasion this operation may take place at the contractor's or experimenter's establishment. In the secondary records, all extraneous material and all confusion of data will be removed to the fullest extent possible. The records will be prepared by the NASA field installation or contractor with the cooperation of the experimenter so as to satisfy the needs of the experimenter and NASA for reduced data. Third, the experimenter shall be given whatever original or reduced data from his experiment his agreement with NASA calls for as soon as possible after they are received. The cognizant NASA field installation will retain copies of the reduced data furnished the experimenter. The experimenter will have sole use of the reduced data for study for a period previously decided upon between him and the Office of Space Sciences, NASA Headquarters. Fourth, no general fixed times shall be set for the period of the exclusive use of these data by experimenters. Each case shall be considered individually and no general rules shall be established. Next, if during a period in which the experimenter has not recorded the results of his experiment, information on the progress of the experiment or portions of the reduced data are requested by another scientist for designing a space science experiment or for the preparation of a technical paper, informal arrangements for the release of the needed data will be made by the experimenter concerned. Both the cognizant NASA field installation and the Office of Space Sciences, NASA Headquarters, will be informed of these arrangements. Next, at the end of the agreed-upon period during which the experimenter will have the opportunity to analyze and report on the reduced data, the reduced data may be released for general use if such a course seems best for developing space science. An extension of the period of sole use requested by the experimenter will require the approval of the Office of Space Sciences, NASA Headquarters.

A review of the policy on the application of our resources involves the allocation of such things as funding, and space on space-flight vehicles. First, we have felt it desirable, and in fact necessary, to make the selection of experiments and experimenters at NASA Headquarters, because of the need to have selections carried out by scientists who are not contenders for the space or the resources. I think it is clear that there must be a place where careful decisions can be made, since the launch vehicles and the spacecraft are so exceedingly expensive and every time we use one of these we throw it away. It also seems clear to us that these decisions must rest with those who are held responsible for the use of the resources; hence, the NASA Headquarters, and in particular the Office of Space Sciences, seems indicated.

We have created within OSS a Space Science Steering Committee to handle this difficult and thankless task. The present membership is as follows: myself as Chairman, in my position as Director of OSS; John Clark, Vice-Chairman, in his position as Associate Director and Chief Scientist of OSS; Edgar Cortright, in his position as Deputy Director of the Office of Space Sciences; Oran Nicks, Director of Lunar and Planetary Programs; John Naugle, Director of Geophysics and Astronomy Programs; Orr Reynolds, Director of the Space Biosciences Program; John Nicolaides, Director of Program Review and Resources Management; Charles Sonett, Chief Scientist in the Office of Lunar and Planetary Programs; and Jesse Mitchell, Chief of Space Flight Systems in Geophysics and Astronomy.

The Space Sciences Steering Committee makes its selection of experiments and experimenters on the basis of the following criteria:

1. Scientific importance. There are at least three categories in which experiments may fall: (a) those categories in which the experimenting is of an exploratory nature; (b) those categories in which the experiments are of a more predictable nature; and (c) the monitoring type of experiment. We try to achieve a reasonable balance among these various categories.

2. The excellence of the scientific method proposed.

3. The technical feasibility of the method proposed.

4. The competence of the experimenter as evidenced not only by his abilities as an experimenter but also by his theoretical strength and his publication record.

5. The adequacy of the technological support organization on which the experimenter can draw. This does not mean that those who do not have technological support organizations in their own agencies are excluded, but it does mean that before the proposal of such an experimenter can be accepted, arrangements must be made for him to have the services of such a support organization.

The Space Sciences Steering Committee has the advice and guidance of consultants and in-house experts distributed among a number of subcommittees. The Space Sciences Steering Committee with its subcommittees has membership from within NASA and from the general scientific community. There are about 50 consultants who participate in the deliberations of these subcommittees.

The Space Sciences Steering Committee and its subcommittees has a broader assignment than just the selection of experiments and experimenters. Indeed, the major assignment of the subcommittees and the Steering Committee is to provide basic guidance to the Office of Space Sciences on the long-range science plan. These groups have the further task of providing guidance to the Office of Space Sciences directors on the translation of the long-range thinking into immediate action in terms of budget requests, initiation of projects, and so forth.

The following approach is developed in the choice of areas of research to be included in the space science program. The areas included are those which are judged important scientifically. The problems attacked are those which require the use of rockets and space vehicles. Having chosen the various scientific areas of interest and importance, we must then face up to the very difficult problem of priorities. We find that the judgments of our various advisers usually differ quite markedly. If we can, we therefore try to give a reasonable amount of support to all the areas. We insist on very competent work. When competent workers can be found, NASA supports an amount in each area which allows the workers reasonable comfort but not a luxurious living. When the requirements for such support outstrip available financial resources, then roughly each area is cut proportionally. This procedure is followed until its further application would simply reduce all or many areas to an unreasonably limited activity. At that point it does become necessary to face up to the task of cutting out an area or subarea in order to provide reasonable support for the remaining areas.

The scope of the space sciences program may be reviewed briefly as follows: At the present time the areas in the NASA Space Science program are: geophysics, astronomy, lunar and planetary investigations, interplanetary investigations, and biosciences.

In the area of geophysics, sounding rockets are used for the exploration of the atmosphere, initial tryouts of experiments, the testing of instrumentation, and so forth. The satellites used in the program include Scout-launched, Delta-launched, and observatory-type satellites. Those launchable by Scout and Delta are used for special purpose missions. Scout and Delta have been scheduled for continued use into the forseeable future. The observatories, launched by the Agena vehicle and later by Centaur, are also scheduled for continued use. The cost for experiments in observatories is far below the cost of performing them in a specially tailored satellite. The observatories appear to provide the only way to attempt to meet the demands which now exist and will exist for flight space. To meet the demand, however, will require scheduling more than the two per year now scheduled.

In the area of astronomy, as in the case of geophysics, sounding rockets have an important role. The mainstay of the astronomy program, however, is the observatory-type satellite. Two Orbiting Solar Observatories per year are scheduled, and two Orbiting Astronomical Observatories per year are scheduled starting in early 1964. The X-15 will be used to support some of the astronomy experiments. Although NASA cannot undertake to provide general support for the field of astronomy, there are occasionally nonrocket type experiments of such importance being conducted by experimenters of such competence that NASA support appears warranted. An example is Project Stratoscope, to which NASA is contributing considerable funding support.

In the area of lunar investigations, we are developing our approach to the study of the Moon by means of what we consider reasonably sized sound engineering steps. We are planning the program so as to carry over from one step as much developmental experience and hardware as possible in undertaking the next step. This results not only in economy but also in increased reliability. We try to schedule enough attempts for each mission to assure achievement of the mission. The scientific program is so phased as to give maximum support to the manned lunar mission. The principal missions being worked out at the present time are Ranger (several per year through 1963 and perhaps further) and Surveyor (four or five per year beginning in 1964). In addition, studies are being made of the need for and the method of attack on more advanced missions. The lunar sample return is high on our scientific priority list, but it is not early in our schedule. This is because of our plan to move ahead in reasonably sized engineering steps. Moreover, we are mindful of the very low probability for success in special, limited-resource side efforts which are described as able to do a special job quickly and economically. Such add-on projects, in addition to having a low probability of success, are of limited objective and detract from the development of the ability to do the thorough job as fast as possible.

In the area of planetary investigations, again we plan a sequence of reasonably sized, sound engineering steps. We plan to develop a program so as to carry over from one step as much development experience and hardware as possible to the next step. Because of the limited opportunities to launch a spacecraft to a planet and the small size of the window at each opportunity, we plan to use dual launchings for each mission, that is, to get off two firings during the window without waiting to see whether the first spacecraft succeeds.

In the area of interplanetary investigations, which we regard as an important area of research in itself, there are missions devoted just to this area, for example, the Interplanetary Monitoring Probe. In addition, the lunar and planetary spacecraft will be instrumented to do as much in the interplanetary field as they can.

Finally, in the area of biosciences, we are in the process of developing a program. It will involve the following elements: (1) continuation of university and other supporting research; (2) development of prototypes for flight instrumentation; (3) development of a small recoverable satellite for bioscience experiments; (4) instrumentation of planetary probes; and (5) possibly participation with the Air Force in an extensive series of scientific recovery-type experiments based on the Discoverer technology.

The program presented to Congress is under review by NASA Headquarters in an effort to remove early emphasis on flights that require the Centaur vehicle, and to introduce smaller vehicles of the Pioneer V type, the Interplanetary Monitoring Probe, and so forth. The trend we are trying to develop is to move away as much as possible from reliance on development vehicles for operational missions.

The resources that are required to support the Office of Space Sciences program total $573 million. The $573 million is not the same as the OSS budget presented to and defended before Congress. It represents the amount that we have requested of the Administrator. The reason for requiring and requesting additional funds is that the earlier budget was constructed before the reorganization at NASA had assigned to the Office of Space Sciences a sustaining university program, a space bioscience program, and before the tremendous overruns on the Centaur project were known.

This budget reflects some historical commitments. The budget is a tremendous jump from earlier years, yet at the same time it is not a tremendous jump in program. It is in large measure a follow-through on programs started in earlier years, with some additions. The number of attempts for assuring success in firings is often minimal. If there were significant cuts in this budget, one would quickly be brought down to the point where percentage-cut approach would become unreasonable. Supporting research and support of advanced technical developments by this budget is significantly below what it ought to be. This marginal support leaves little flexibility for following up on discoveries, or introducing special programs, or for handling the problems of overruns, or for mobilizing quickly to cooperate with special programs such as the World Magnetic Survey or the International Year of the Quiet Sun.

The Headquarter's role in the business of the space science program is primarily to bring into being and maintain a viable sound integrated national program in the space sciences. It is not to carry out that program. This is the role of the participants and in particular of the Centers. A draft of the functions and responsibilities for the Office of Space Sciences has been prepared, although this draft is still in the process of review by the Administrator. The section on responsibilities reads as follows:

The Office of Space Sciences is responsible for the planning, direction, and execution of all NASA efforts in the area of scientific investigations of the Earth, Moon, Sun, planets, stars, the galaxy, and space. This area of responsibility includes the conduct of necessary research and development in support of approved projects and to evolve the techniques for future experimentation in space including the operational and logistic systems required for unmanned missions. The Office of Space Sciences is also responsible for the administration of the NASA grant and research contract program.

The role of the Centers is as follows: the NASA Centers are responsible for project management and technological support for spacecraft systems and subsystems, and associated tracking, data recovery and data reduction. In this respect the Goddard Space Flight Center is responsible for satellite, the sounding rocket, and geoprobe areas, and the Jet Propulsion Laboratory is responsible for assigned projects in the lunar and planetary area.

An important secondary role of the Centers is that of experimenter. In this role the Center experimenters are required to compete for space on the various satellites and space probes under the same ground rules as the rest of the scientific community. As stated previously, NASA Headquarters makes the decisions on assignment of experiments to the various flight missions and on the choice of experimenters. The Space Sciences Steering Committee at Headquarters tries to make these choices and assignments as objectively as it can, selecting the experiments and choosing the experimenters on the basis of worth and competence, appropriateness, likelihood of success, and so forth. The Steering Committee is often accused by the NASA Centers of being partial to the outside scientific community. It is often charged by the outside scientific community with favoring the Centers. We have recently been told by our Centers that we run a closed shop and they have no input. For years we have been told by the outside scientific community that they have no real input, since we run a closed shop in collusion with the Centers. We are taking steps internally to open this activity more to the scrutiny of the Centers but without abrogating the responsibility of the Steering Committee.

The secondary role of the NASA Centers as experimenters in the space program has an important bearing on their primary role. Participation in the scientific program tends to orient the Center toward science as the primary objective rather than toward engineering. This insures competent, realistic, and effective technological assistance to the whole scientific community. It has been suggested for some time by members of the university community that the NASA Centers either drop this secondary role or adopt in its place a secondary role of experimenting in areas where no one else is interested. I am convinced that full consideration of this matter will lead to the conclusion that this is not a sound recommendation and is not in the interests of the outside scientific community.

The question of relations to other programs can be reviewed briefly as follows. Where support to other programs can be provided, we give added weight to appropriate areas or subareas by supporting more "good research" in such areas, but not at the expense of "excellent research" in the other, for the moment, less practical areas. For example, we are giving particular attention to obtaining the necessary interplanetary radiation data to support manned flight, military projects, and communication satellite efforts; to obtaining lunar pictures and surface data, solar data in support of manned flight; and to obtaining micrometeorite data in support of general engineering requirements and manned flight.

The cooperation with other programs and other activities is also a two-way affair. We expect that our support of the manned-flight effort will be repaid in full many times over by the tremendous support that the man on the Moon and in outer space can give to space science. Similarly, the meterological applications effort leads to the performance of very good scientific experiments on the atmosphere. The same can be said about the communications effort. In fact, the Echo satellite made a tremendous contribution to the measurement of upper air density, although it was launched as a communications experiment.

The cooperation with the military is also a two-way affair. In fact, the use of military vehicles such as the Thor and the Atlas for the first stages of our launch vehicles is one of the mainstays of our program. Cooperation on the scientific side is also extremely important. There is coordination on the working level in day-to-day contact which I think is effective and good, but the matter of coordination is not left entirely to this sort of informal getting together. There is an Astronautics Coordinating Board which is chaired jointly by Harold Brown and Hugh Dryden. Members of this Coordinating Board represent the various interests in the Department of Defense and the various program areas in NASA. The members are top-level people, including the program directors in NASA and the associated administrators of the deputy administrator. Members from the Department of Defense are at corresponding levels. The members are not permitted to send substitutes, since the aim is to emphasize the attendance of all principals at these meetings. The board does not attempt to vote on issues but rather to arrive at a common agreement which the members can implement in their organizations. The Coordinating Board has six panels covering various areas of interest. These areas are: manned space flight; unmanned spacecraft; launch vehicles; space flight ground environment; supporting space research and technology; and aeronautics.

Finally, there is the international program. I have mentioned earlier that we found a tremendous interest all over the world in space research and in cooperating with the United States. Of great importance are the minitrack net, the Smithsonian Astrophysical Observatory optical net, Moonwatch, and so forth. These are important in supporting the space science activities. Through these activities, many countries are assisting in the tracking of our satellites and space probes and the collection of data from them. They participate sometimes by permitting the establishment of ground stations on their soil and sometimes by going as far as participating in the maintenance and operation of these stations. Some countries have assumed full responsibility for both technical and financial support and for operation of the stations.

At the present time, the following countries have or are planning sounding-rocket programs: Argentina, Australia, Brazil, Canada, France, India, Israel, Italy, Japan, New Zealand, Norway, Pakistan, Sweden, the U.S.S.R., the United Kingdom, and the United States. Of those listed, Canada, the U.S.S.R., the United Kingdom, and the United States also have satellite programs, and Italy and France are considering satellite programs. NASA is cooperating with most of the countries engaged in sounding-rocket research.

In the satellite effort NASA will launch the satellites now being prepared by Canada and the first several of the United Kingdom satellites. Moreover, discussions are in progress with other countries, including the U.S.S.R., on the possibility of additional cooperative satellite projects. These cooperative projects are based on the recognition of substantive value to each participant. The two cooperating countries divide responsibilities of various parts of the project and share in the costs. The apportionment of responsibilities and costs is often not on a fifty-fifty basis. Each country pays for its own participation in the cooperation and for the portion of the project for which it has assumed responsibility. No exchange of funds between the countries is involved; but there may be an exchange of equipment and instrumentation. The scientific results of such a cooperative project are to be made generally available through open publication.

PROGRAM OF THE OFFICE OF MANNED SPACE FLIGHT

By Joseph F. Shea

This paper presents a description of the status of the manned space program at the present time. It will not concern policies with respect to the program; rather it is an attempt to give an instantaneous picture of a very complex technological development job which is now in the process of evolution and definition. This program represents a unique requirement for the wedding of science with the somewhat pragmatic approach of development engineering. This wedding is required in two areas: one, a definition of the program itself, so that it indeed is meaningful and not merely a stunt when we finally accomplish manned lunar landing, and second, the establishment of design requirements, and the assurance that these requirements are correct before implementation of the first landings. In the second area we will look to the scientific program, particularly the Space Science program, for the fundamental engineering data which will be required.

One year ago the President made the decision to implement manned lunar landing sometime within the 1960 decade. This last year has, in retrospect, been spent converting the program which we then had, the Apollo program, with its initial set of goals which related to exploration in Earth orbit, in cislunar space, and in lunar orbit, but did not involve an actual lunar landing, to one which provides a meaningful manned lunar landing program with a significant capability for growth in that program beyond the first emplacement of man on the Moon. We are now in the last stage of selecting the mode or mission profile by which we will implement the program; the three modes which are still under consideration, some of the characteristics of each of these, the vehicles and spacecraft which are presently in development, and those that would have to be developed in order to complete the overall set of hardware required to do the job will be described in this paper. A brief discussion will also be given of the mission capabilities which we expect and the tie which we have to, and the daily requirements which we have from, the unmanned programs. The present program is aimed at developing a manned lunar landing and return for several reasons. One, and probably the main one, is that the manned lunar program provides a focus for the development of the broad area of technological entities that must be brought into being to provide a significant space capability for the Nation. Although we intend to focus a heavy portion of our efforts and a heavy portion of our publicity on the actual landing of a man on the Moon, the real underlying motivation for the program is one of advancing technology across a very broad front.

Second, the Moon program will provide a strong, easily understood demonstration of our national technological capabilities; last, not necessarily in order, the lunar landing itself and the placement of men on the Moon will be a step toward the overall exploration by men of our solar system and, in particular, will provide a singular possibility for additional scientific exploration.

The present manned program is composed of three projects. One is the Mercury program; it is now in its middle or later phases. There have already been two three-orbit missions. Basically they have proved the capability of man to exist in space and to function reasonably well. They have also pointed up certain directions in which the evolution of our mission planning and our hardware development ought to go. It is expected that over the course of the next 6 months there will be one and possibly two more short-time orbit missions. A redesign of the Mercury capsule is underway at this time to extend its capability to an 18-orbit mission, but one of the great unknowns is the long-time effect of weightlessness and space environment on individuals; a 24-hour mission is scheduled for the first quarter of 1963. Also planned for Earth-orbit operations is a redesigned upgraded spacecraft called Gemini. It has been in design and development for approximately 9 months. It will have the capability of placing two men in Earth orbit for a period varying from 7 to 14 days; in addition it will have significant maneuvering capability and will be a basis for the development of several techniques in Earth-orbit operations.

The Gemini program is scheduled for first flights in the latter part of 1963. Its flight test program would then extend through a series of some 12 scheduled shots from 1964 to 1966. The Apollo program, which started as a cislunar exploration program without landing capability, now represents the focus for the provision of man landing on the Moon. The major Apollo contracts which are presently committed were let late in 1961 and early in 1962. We have several major procurements still to be released before we have all the necessary hardware under development. We expect to be flying the Apollo spacecraft itself on some of the early Saturn vehicles in unmanned tests in late 1963. The first manned tests in Apollo are scheduled for the latter part of 1964. The launch vehicle for the Apollo mission, the advanced Saturn, is scheduled for flight testing in mid-1965. The time period by which we might expect to accomplish the first manned lunar landing is still clouded by several of the development problems and uncertainties associated with being able to schedule programs five, six, or seven years in advance.

There are basically three general classes of missions which might be employed to get a significant payload to the Moon and back. (See Fig. 1.) One is the direct mission, which is, in effect, the simplest. In this approach, all items needed to accomplish the mission are loaded onto a single vehicle which is fired from Cape Canaveral; items other than those needed to complete the mission are discarded after use.

The second approach is to assemble the Earth-escape payload in Earth orbit. This would mean two or more firings of payload from Cape Canaveral, the joining of these parts in Earth orbit, and the injection from Earth orbit to a translunar trajectory. Again, all items needed to complete the mission are present and separation is only a rejection of stages which have been used.

The third approach is the lunar orbit rendezvous in which the payload is launched in a single launch from the Earth to injection on the translunar trajectory. It is dropped into a lunar orbit and, then, from lunar orbit a capsule is launched toward the Moon. The mother ship containing the fuel required to return to Earth remains in lunar orbit. The capsule provides for existence while on the lunar surface, and then is returned to lunar orbit to rendezvous with the mother ship. The capsule is left in lunar orbit and the mother ship is brought back to Earth.

<u>Discussion of Direct-Mission Approach</u>. Four stages of the direct-mission approach are shown in Figure 2. The main elements involved in this mission are the Nova launch vehicle, the Apollo command capsule, and the lunar landing modules. Basically, the direct mission approach requires a 150,000-pound Earth orbit escape capability; this implies a capability to place more than 400,000 pounds into Earth orbit. For the direct mission, the launch vehicle can, theoretically, provide a direct ascent trajectory to injection; as a matter of practice, in order to accommodate launch windows at the Cape and to provide the opportunity to check out operating systems in orbit prior to commitment to the lunar mission, the spacecraft would be launched into Earth orbit with the first two stages of the launch vehicle. After considerable checkout and arrival at the proper launch-window position, the spacecraft would then be injected from Earth orbit to translunar trajectory with the third stage of the launch vehicle. Thus, in translunar trajectory would be the spacecraft, its service module which contains the return propulsion, and a large stage which could be used to decelerate the entire payload upon lunar landing. The operation in the vicinity of the Moon would be basically to use the lunar landing stage to achieve lunar orbit, at which time the instrumentation would again be checked and there would be a limited amount of surveillance for possible landing sites; then that stage would be reignited to provide power to descend to the lunar surface. After completion of mission time on the lunar surface, the entire stage used in landing, in effect, becomes the launch tower for the return mission. The equipment or the propulsion needed for the return is contained in the service module. The service module is launched, goes into lunar orbit, ejects from lunar orbit back to the trans-Earth trajectory, and reenters the Earth's atmosphere using aerodynamic braking for the slowdown and recovery operation. The characteristic of this mode is fundamentally a very large launch vehicle. The total weight of the launch vehicle is about 9 million pounds at take-off; it requires a total thrust at take-off of approximately 12 million pounds. The advantage of the mission is an operational simplicity associated with the fact that it requires no connections, rendezvous, or maneuverings, and has no particularly difficult operational requirements.

<u>Discussion of Earth-Orbit-Rendezvous Approach</u>. Figure 3 describes what is required for Earth orbit rendezvous. In this approach there is still the problem of providing a 150,000-pound Earth orbit escape capability, and therefore, a total spacecraft-plus-third-stage injection capability of about 400,000 pounds. The launch vehicle which would be required, the advanced Saturn, is somewhat smaller than the Nova launch vehicle. The advanced Saturn has 7-1/2 million pounds of thrust and weighs about 6 million pounds on the launch pad. The basic operation would involve two successive launches of this vehicle from the Cape. An unmanned vehicle would be launched first and probably injected into the higher of the two orbits. The unmanned vehicle would be the first two stages of an advance Saturn launch vehicle and the third stage would be a tanker containing all the liquid oxygen necessary to fuel the escape stage. This unmanned vehicle will be placed in a 300-mile Earth orbit; that orbit will be tracked from the ground, the ephemeris of the orbit established, and then a guidance constant will be generated for the second launch vehicle. The second vehicle would then be launched into a lower Earth orbit in the same plane as the unmanned vehicle. The reason for the lower orbit and not direct injection into the higher orbit of the first vehicle is to provide a larger launch window from the Cape; there will be a firing period of approximately 2 hours a day. Again, tracking from the ground is required to establish the ephemeris of the manned vehicle; a series of commands are then generated from the ground

which, in effect, tell the vehicle in lower orbit when to add velocity and the direction in which to add this velocity so that the two vehicles will be brought together, or close together, in the higher orbit. This maneuver can be accomplished with an accuracy of approximately two to three miles; then these two vehicles will approach each other in the higher orbit with relative closing velocities on the order of 100 feet per second or so and relative displacements on the order of tens of miles. We then expect local sensors onboard the two vehicles to provide relative position and velocity information. The thrust capability of engines onboard one of the two vehicles, either the tanker or the manned vehicle, provides the necessary maneuvering capability to bring these two vehicles together in Earth orbit. The tanker would lock on behind the assembled spacecraft injection stage. Liquid oxygen would be transferred from the injection stage, or from the tanker, to the launch vehicle in orbit; at that point the tanker would be dropped away and suitable checkouts would be made to assure that all systems are working. After waiting for the launch window, the spacecraft would be injected from Earth orbit to a translunar trajectory. The characteristics of this mode are, in general, requirements for a smaller launch vehicle which provides a great gain in terms of time schedule associated with implementation of the program. It does have the requirements for bringing the tanker into being and for performing some relatively complex maneuvers in Earth orbit; neither the rendezvous nor the tanking operation have yet been attempted. It also involves the complexity of two launches of fairly massive vehicles from the Cape. The Mercury launches have shown that the state of the art of launching vehicles at this time does not produce great confidence in meeting tight launch windows in short periods of time.

Discussion of Lunar-Orbit-Rendezvous Approach. The third mode is called lunar orbit rendezvous (Fig. 4). In this mode, the concept is to launch with a single vehicle, the advanced Saturn, from the Cape. The first two stages of this vehicle place the spacecraft into Earth orbit; again, the spacecraft is checked out in Earth orbit. Then, the third stage of the launch vehicle injects the spacecraft from Earth orbit into the translunar trajectory. On approaching the Moon the service module is used to slow the spacecraft into lunar orbit. In lunar orbit, then, is the command module which has the heat shield, life support equipment, recovery equipment, and other items associated with return to the Earth and maintenance of life during the return trip, and the service module which contains the return propulsion; in addition, there is a small vehicle called a lunar excursion vehicle or the "bug." This vehicle contains necessary life support equipment, experimentation equipment, propulsion control guidance, and so forth in order to effect lunar landing. After several passes around the Moon in lunar orbit, the landing vehicle would be released from the mother craft and placed into an equaperiod orbit, equaperiod with the roughly circular orbit of the mother ship but with a much lower perigee so that from the excursion vehicle the lunar surface may be rather closely examined. The nominal altitude for the orbit of the mother craft will be approximately 65 to 100 miles. The landing vehicle would be placed into an elliptical orbit, which would, in essence, be in a position to rendezvous without additional propulsion for several passes. That orbit would have a perigee on the order of 50,000 feet or lower. After some reconnaissance from the landing vehicle of the actual landing site itself, the landing engine would be ignited and the "bug" would be landed on the lunar surface. The landing would take place roughly in the same plane as the plane of the mother-craft orbit. There would be communication and tracking capability between the two crafts and communication between each craft and the Earth. At a suitable time, with a launch window of approximately five minutes available, the landing craft is launched from the lunar surface and rendezvous with the mother ship in lunar orbit. After rendezvous is accomplished, the astronauts would transfer back to the mother ship. The lunar excursion vehicle is ejected and left in lunar orbit, and the spacecraft returns to Earth.

The prime advantage and attraction of this particular mode turns out to be the payload to escape required. This entire mission can be launched with a single Saturn C-5 vehicle; the total payload to escape required is approximately 75,000 pounds. The total capability of the C-5 vehicle to inject into escape is on the order of 90,000 pounds. Thus, there is a fairly comfortable weight margin associated with the mission. A second advantage, which turns out to be quite strong, is the fact that the vehicle which lands on the Moon is distinctly different from the vehicle which reenters the Earth's atmosphere. Thus, this vehicle can be designed for optimum performance in the vicinity of the Moon and, in particular, for the landing operation where visibility and maneuverability are desirable. The spacecraft can also be designed for optimum performance during reentry into the Earth's atmosphere where pilot visibility is not so important.

The disadvantages of the mode relate to the complexity of achieving an orbital rendezvous in the vicinity of the Moon with somewhat less assistance from the Earth than is possible in an Earth orbit rendezvous and with a much more fatal consequence associated with lack of being able to rendezvous. The analysis of this mode has required very detailed considerations of what is necessary to achieve lunar rendezvous. The difference has been determined, for instance, between the sensor and the propulsion requirements for lunar orbit rendezvous and the same requirements for a mission in which the spacecraft is launched from the lunar surface and returns to Earth. Detailed examination shows, essentially, that in every subsystem the requirements for a lunar rendezvous in terms of accuracy, velocity, margins, and so forth are no more critical and in several cases less critical than actually performing the injection for lunar orbit and return to the Earth.

Details of Approaches. As was mentioned previously, the direct approach requires 150,000 pounds to escape. (See Fig. 5.) The launch vehicle, the Nova weighs about 9 million pounds. The first stage is composed of eight F-1 engines, which burn oxygen-kerosene fuel. These engines develop 1.5 million pounds of thrust. The engine has been under development for several years. It is already undergoing static firings. It is the same engine that would be used in the first-stage booster of the advanced Saturn vehicle; in the Nova, eight engines are used compared with five engines in the Saturn. The second stage is an oxygen-hydrogen stage, containing or powered by two M-1 engines; a new 1.2-million-pound-thrust engine which has just recently been put under contract. This M-1 engine, provides the great growth and payload capability associated with this stage. The third stage is the S-IVB stage. It is an oxygen-hydrogen stage weighing about 230,000 pounds, containing a single engine, the J-2 engine, which develops 200,000 pounds of thrust. The spacecraft itself consists of a lunar landing module. This lunar landing module is an oxygen-hydrogen stage; its cryogenics weigh approximately 100,000 pounds, and it provides the thrust or impulse necessary to land on the lunar surface. The return propulsion and much of the additional life support equipment not required for direct reentry into the Earth's atmosphere are contained in the service module. The service module will, in general, use an Earth storable type of propellant, probably a single engine rather than the multiple engine shown in Figure 5.

It will weigh approximately 38,000 or 39,000 pounds. The command module which houses three astronauts and all the life support equipment associated with the mission weighs approximately 11,000 pounds. The service module and the command module are presently under contract to North American Aviation. None of the base stages to the Nova are now under contract. It seems fairly clear from our schedule analysis that the creating and construction of the Nova launch vehicle to support the Apollo spacecraft would probably delay the overall manned lunar mission by 18 months to 2 years.

The Earth-orbit-rendezvous technique is described in Figure 6. As previously mentioned the vehicle planned for this technique is the advanced Saturn vehicle. The base stage consists of five F-1 engines each having 1.5 million pounds of thrust. It is called the S-IC stage. The S-IC is presently under contract to The Boeing Company. The direction of this contract during the initial design work comes from the Marshall Space Flight Center at Huntsville, Alabama. The second stage for this vehicle, the S-II stage, which is presently under contract to North American Aviation, weighs approximately a million pounds; it is an oxygen-hydrogen stage. The stage is powered by five J-2 engines, each having 200,000 pounds of thrust. The first launching would consist of these two stages plus a tanker vehicle which must be capable of storing approximately 200,000 pounds of liquid oxygen in earth orbit for periods up to a month. The third stage, or escape stage, of the second vehicle shown in Figure 6 is the S-IVB, an oxygen-hydrogen stage powered by a J-2 engine and built by Douglas Aircraft Company. In order to achieve reasonably balanced payloads the S-IVB would be launched with a full load of hydrogen but with none of the oxygen aboard; all the oxygen is then transferred from the tanker to the escape stage. Thus, in Earth orbit there is the escape stage, the S-IVB, the lunar landing module (which is not yet under contract), the service module, and the command module.

For the lunar-orbit-rendezvous technique (Fig. 7), there are certain simplicities in the overall mission, at least in terms of the vehicles involved. The launch vehicle is again the advanced Saturn. The first stage, second stage, and third stage are the same as the ones described for the Earth-orbit-rendezvous technique except that there is no requirement for tanking the third stage. The total weight of the spacecraft has not been completely determined, but it will be approximately 25,000 pounds. The service module described for the other two modes would require only small changes. It would require two burns, a burn to place the spacecraft in lunar orbit and a burn to escape from lunar orbit. Its total weight would increase a few thousand pounds in order to provide enough fuel. The command module would be the same as that described for the other two techniques.

Certain items are common to all the modes. Once the spacecraft is injected from the lunar orbit it is necessary to return to Earth within a reentry corridor roughly 40 miles across (Fig. 8); this corridor can be achieved by a combination of the accuracy of guidance at injection from the Moon plus an ability to make midcourse corrections based on onboard stellar operations and ground tracking from the deep space instrumentation facilities. The reentry is ballistic, the shape of the reentry vehicle being roughly the same as the shape of the Mercury capsule. The reentry velocities are higher than those for Mercury and the technology associated with heat shields is one of the items for concern in the overall program, both because of the increased heating loads for reentry at escape speed and because of the problems associated with the length of time in the space environment.

Figure 9 shows the descent phase in the atmosphere; we expect to continue to use parachutes in a manner analogous to that of Mercury. However, we are investigating in the Gemini program and have provisions to incorporate into project Apollo, if it proves useful and reliable, a paragliding technique which can provide some elements of controlled reentry.

Figure 10 shows the recovery itself. We expect, through a combination of shock mounts, to be able to land on the Earth as distinct from having to do water recovery for all the missions. This is not a significant simplification in the overall system because we still require, in order to handle the broad capabilities, some form of water recovery. It is rather difficult to predict, for instance, two weeks ahead, which is a nominal duration of the mission, precisely what the weather conditions are going to be at reentry.

These detailed analyses show that all the modes are entirely feasible, with differences between them relating more to operational problems than to the difficulties of bringing the individual elements of hardware into being. We expect to present a series of detailed recommendations concerning the selection of the mode to the Administrator soon.

We have made one other major change in the program. The present approved NASA program in terms of implementing the manned lunar landing calls for the performance of the mission by Earth orbit rendezvous, with a backup which is almost time-schedule compatible, using the Nova launch vehicle. As we have examined and considered both the feasibility and the desirability of doing a parallel program rather than a series of overlapping programs which really increase the overall national capability, we have concluded that the best approach is to select one mode, have one standard method of approach (the philosophy by which the program is run), and place all our resources behind that particular approach, and then introduce new technologies to give a capability for much larger overall operations.

Mission. One of the things that we are most concerned about is providing in the program a significant capability to do sensible scientific experimentation. We feel that the mission itself represents a large enough national investment that we ought to have more worthwhile objectives than merely placing a man on the Moon. We look to the Office of Space Sciences, to the National Academy of Sciences, and to the Working Group to tell the engineering staff in detail what experiments to make. Our schedule is planned for a lunar landing before 1970.

The reliability of the vehicles themselves is the greatest uncertainty associated with naming a specific time. It is easy to schedule developments to reach the test stage. We hope with rigorous design and test practices to schedule the time of first flight. We cannot at this time schedule, in detail, how many attempts will be required before we obtain reliabilities which warrant personnel commitment to the mission. We can perhaps plan on having flights on something like a two to four-year schedule. Problems on one flight create design changes in the next one; there is roughly a six-month or more delay between successive missions. If there are no problems, the problem of data assimilation and the definition of mission for the next flight will still take approximately 3 months. We are in a time period in which two to four of these flights can be made in a year. We can, in terms of communications, have available commercial quality TV because of the wide bandwidth available. As far as landing sites are concerned, they will be affected slightly by the mode that we select, particularly if LOR were chosen; the other two modes would take landing sites in the vicinity approximately 10° to 17° latitude of the lunar equator for the first missions. In general, for a manned program it does not make too much difference whether the craft lands on the leading edge or the trailing edge. For obvious communication reasons, the backside of the Moon is unsuitable. I think that the leading edge has certain advantages because this is an area which can be more easily supported by the unmanned program.

If possible, I think that we ought to begin to select a limited number of sites early, since we have available to us the whole equatorial belt. The main reason for doing this is to focus our attention in terms of trajectory detail on that area and let space science help somewhat in focusing some of their experiments in the same area. The total number of missions is undefined. It is more than one. The program is expected to grow and the capabilities to do missions will also tend to grow. In a direct landing all three astronauts would land on the lunar surface; in a LOR landing two of the astronauts would land on the lunar surface. Again, this does not seem to be a strong factor in terms of defining the mission one way or the other. The initial stay time on the lunar surface will probably be somewhere between 4 and 24 hours for the first mission. Thus, only a fairly limited amount of exploration would be possible at that time. We would expect through the first few missions to grow to a stay capability of somewhere between two and seven days. We would expect the program to evolve over the course of a few years from this initial capability measured in terms of hours to the possibility of a permanent base. In the first few missions, the scientific landing payload will be approximately 200 pounds, with approximately a 100-pound return capability. A mission of the LOR type where there is a reasonable amount of payload capability in excess of launch-vehicle requirements, as far as the spacecraft is concerned, might mean an increase in the weight of scientific equipment carried to approximately 1,000 to 1,500 pounds in the early phases of the development program.

Power does not appear to be a major problem. Mobility is one of the difficulties. Space suits do not permit a great deal of mobility or manual dexterity. In the early missions the combination of space suit and life support equipment would limit the radius of action to approximately 1/2 to 1 mile from the point of touchdown. For safety purposes, one astronaut would probably remain in the ship at all times which would mean, in LOR, that one astronaut would be handling operations on the lunar surface.

We have formed an ad hoc working group for definition of the Apollo mission within the Office of Space Sciences. They have been working for some time and they have made what I think is a reasonably good start in the accumulation of the kinds of operations which might be planned for the first manned lunar landing missions. A major requirement is that these operations ought to be unique to man and ought to be unique to the Moon and should not be operations capable of being performed in the unmanned program. These are generally geological types of exploration and the careful emplacement of experiments.

Finally, the data requirements fall roughly into three classes. Environmental data give primarily the meteorite and radiation type of information. We already have models for these. The question is a validation of these models and, in particular, a sensitivity to any new information which comes into the program which would change our design requirements significantly. The surface physical characteristics, roughness, slopes, and dust, are of particular interest to us. Orbiting spacecraft can supply some of these data; primarily they will come from unmanned vehicles on the lunar surface. Surface bearing strength is another important consideration. A large amount of reconnaissance and topographic data will be required in order to support the program for the preparation of geographic and geologic maps which will be of help in the overall exploration.

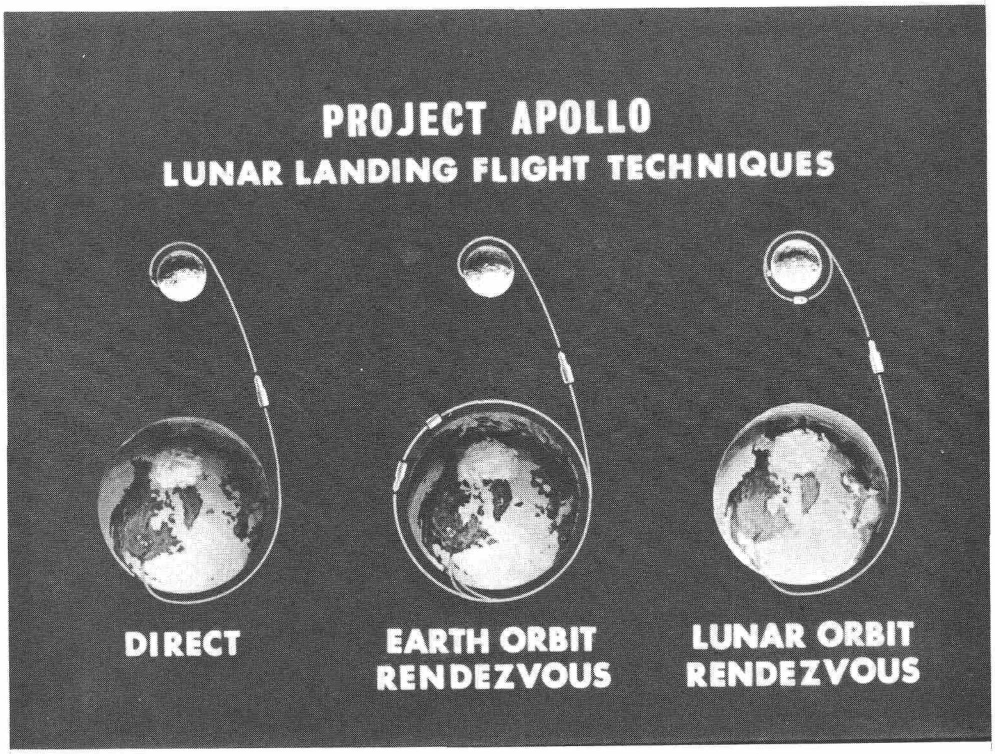


Figure 1

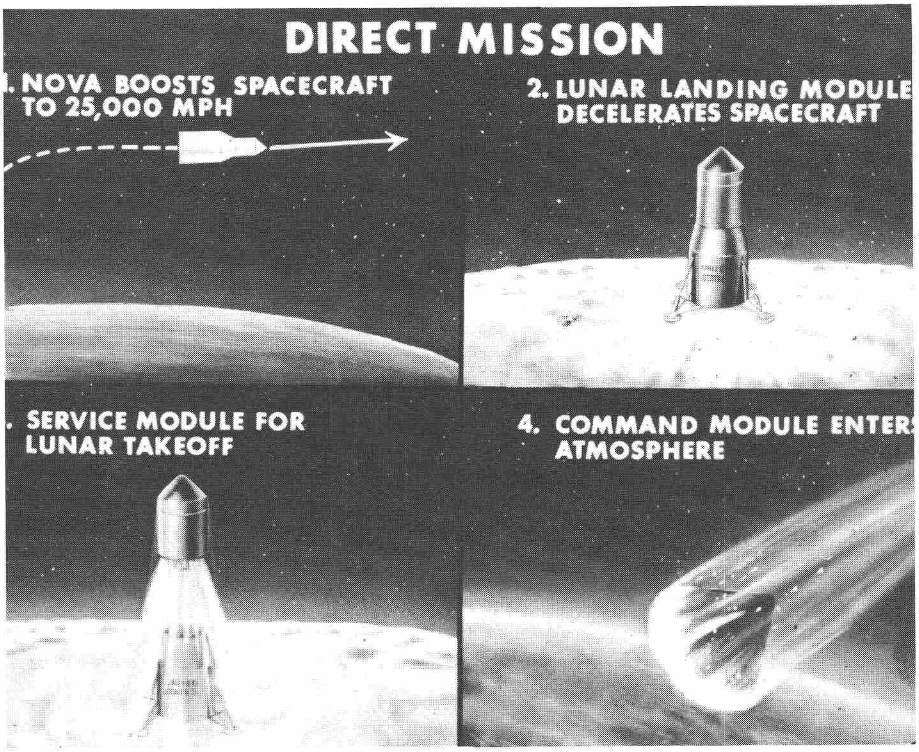


Figure 2

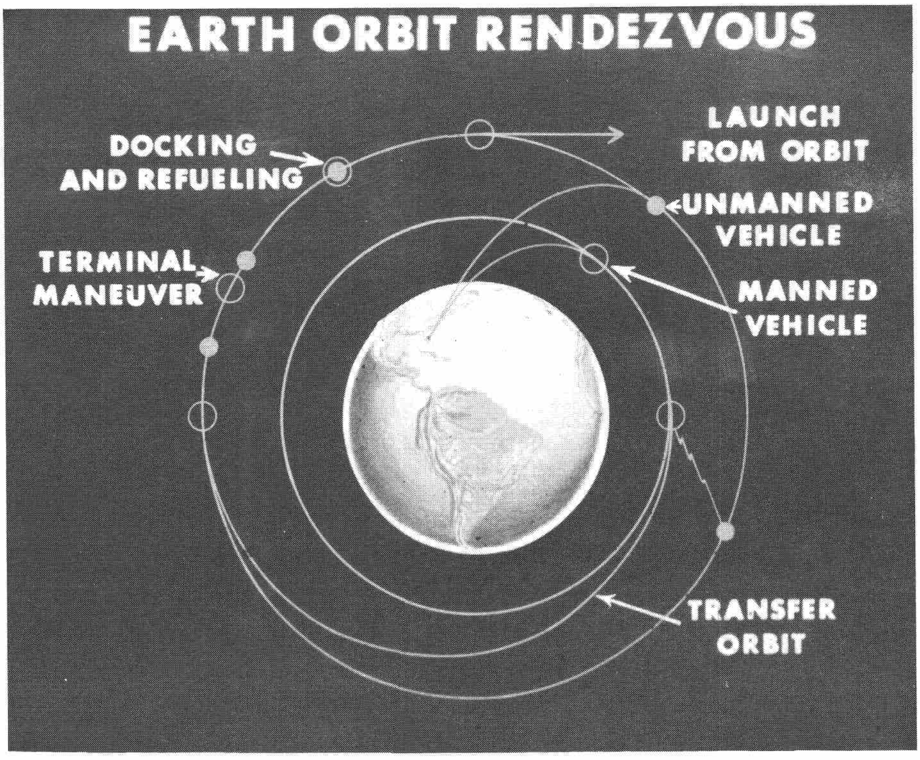


Figure 3

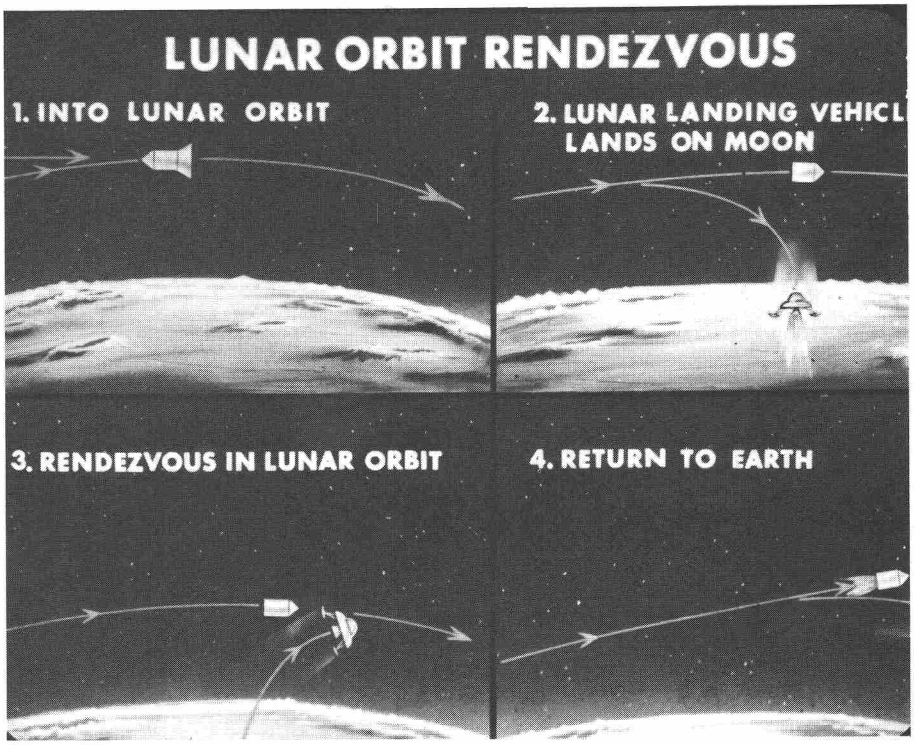


Figure 4

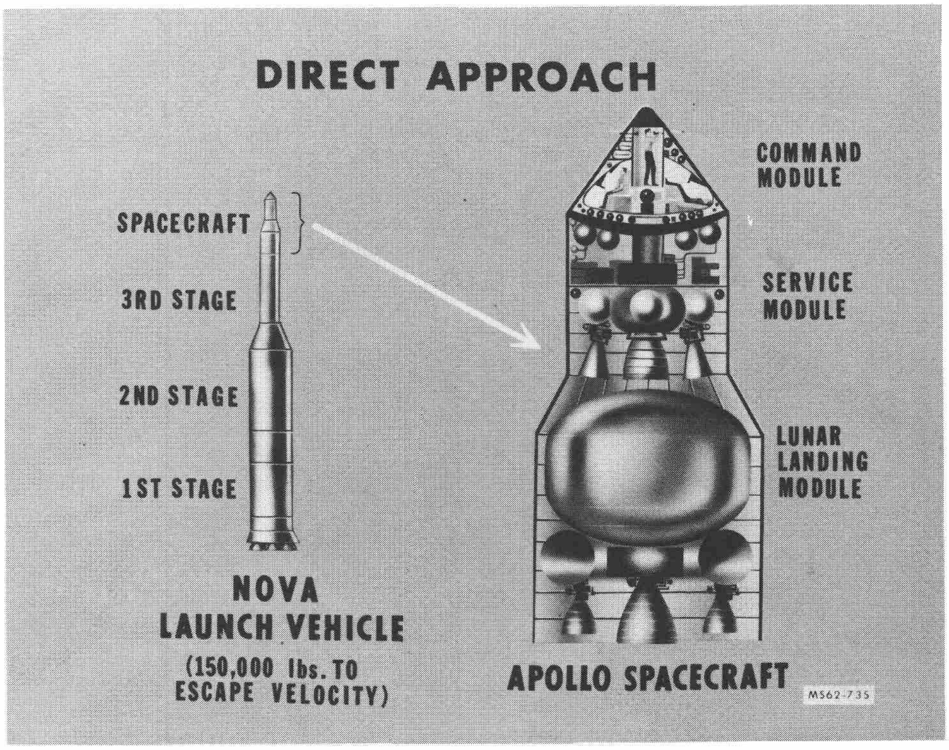


Figure 5

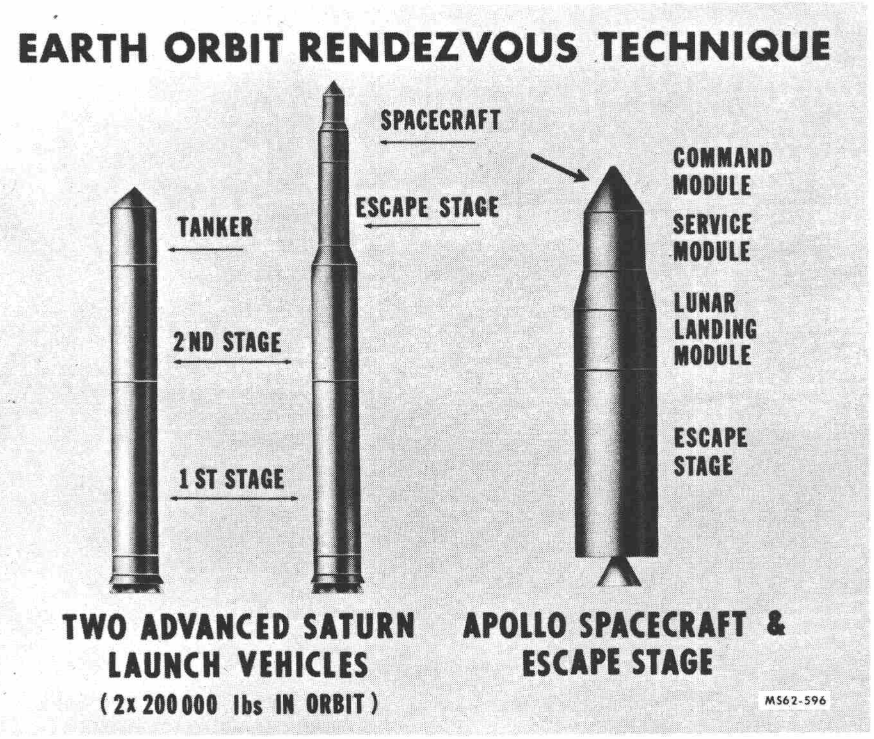


Figure 6

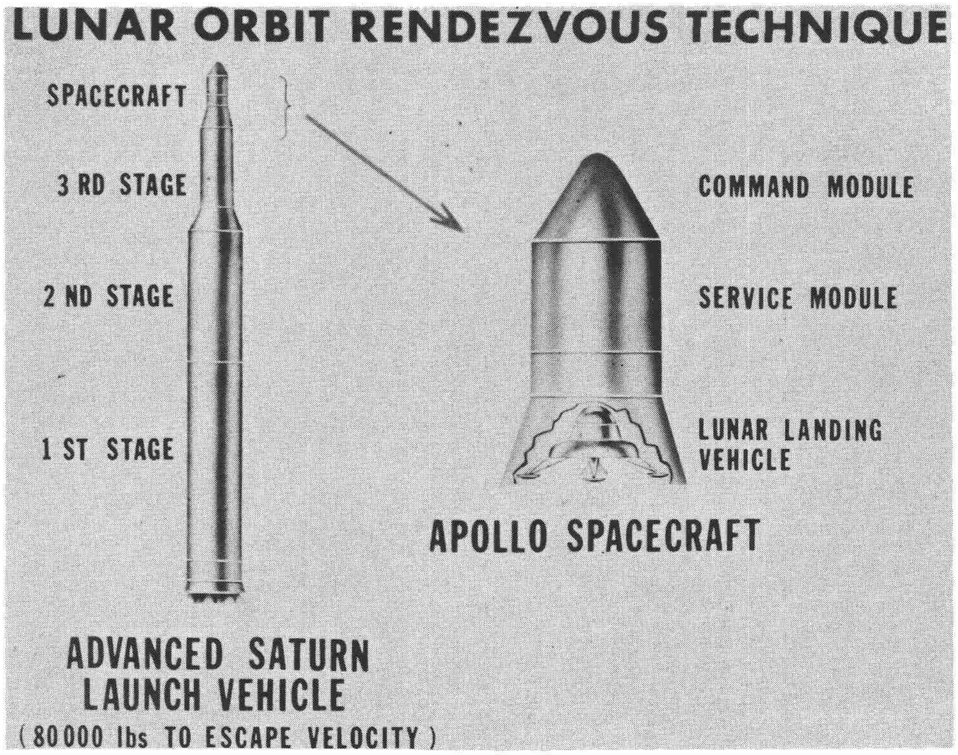


Figure 7

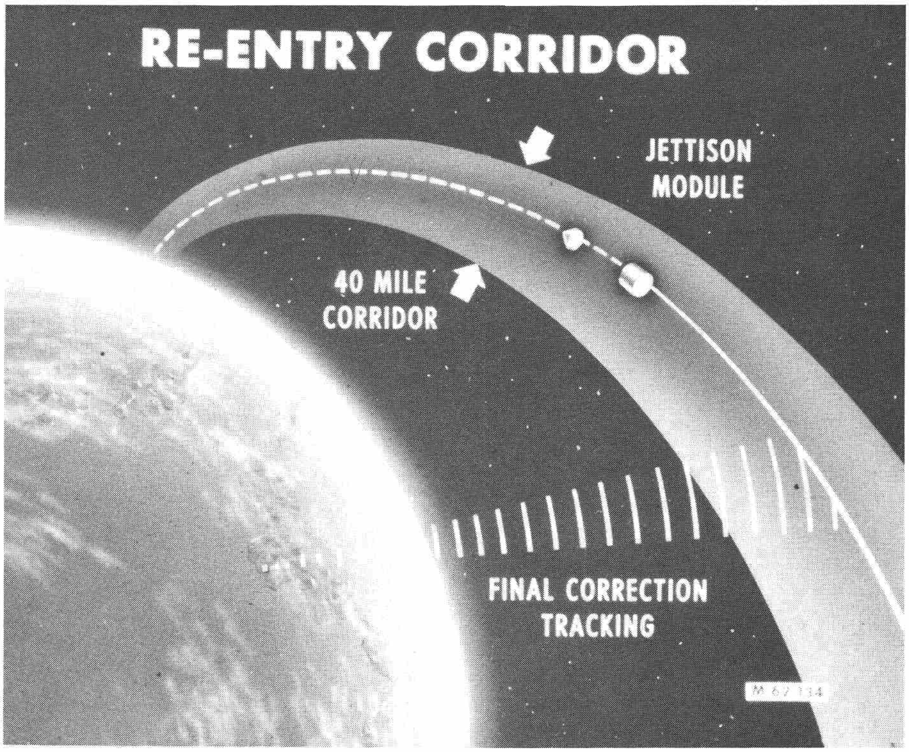


Figure 8

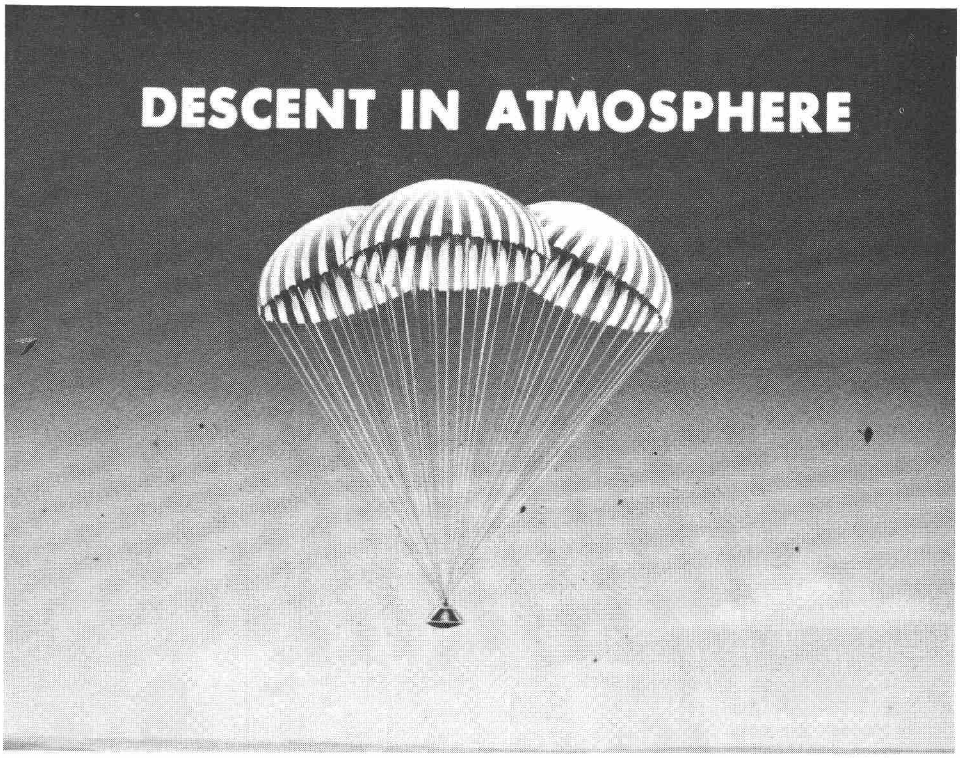


Figure 9

Figure 10

PROGRAM OF THE OFFICE OF APPLICATIONS

By Morton J. Stoller

The functional areas of the Office of Applications are: meteorology, communications, and future applications. In the Meteorological Systems program area our primary concern is with the research, development, and application of meteorological satellites and the associated command and data acquisition stations which are needed to establish a meteorological satellite system. These facilities will be used to obtain a better understanding of the weather and how to forecast it. The NASA staff works closely with the Weather Bureau, the weather services of the armed forces, and the international meteorological community in formulating the program.

The second program area is Communications Systems. This segment of the overall program of the Office of Applications includes our activities relating to satellites and ground facilities for communication satellite systems. This program area also has its international aspects, inasmuch as the relations between NASA and its counterpart agencies in other countries in establishing plans for satellite communications play an important role in our overall program.

In the Future Applications program area the effort is directed to the identification of other possible applications of spacecraft and the rapid dissemination of information on those technological developments resulting from NASA research and development activities which appear to be useful for general industrial application.

Meteorological Systems Program. Because an appropriately designed satellite gives a capability to observe the weather from above, it now becomes possible to furnish the meteorologist with maplike pictures of cloud systems over most of the world's surface. The ability to determine just where cloud systems are located, to determine the specific nature of the cloud patterns, and to estimate from successive observations approximately how fast storm systems are moving, and in what manner they are changing, has already been turned into a valuable operational tool by the meteorological services.

We are proceeding as rapidly as possible, in conjunction with the Weather Bureau, to develop a meteorological satellite system which will provide satellite observations for the meteorologist to use in his routine daily operations. Our meteorological systems program objectives reflect these considerations. In the first phase of the program we are undertaking the development of the equipment and techniques required for the establishment of an operational meteorological satellite system and for the general support of meteorological research. These two end uses may lead to somewhat different satellite configurations; for example, the operational systems will require equipment with a very high degree of reliability. Therefore, it is probable that successive operational units will have the same configuration so that the performance can be improved by repetitive use of the same system elements. On the other hand equipment for the meteorological research satellite is necessarily somewhat more experimental in nature and will be flown in various configurations in the course of development. In the second phase of the program, there are satellite launchings for the continued development of sensors, spacecraft, and systems. We expect that these developmental launchings will continue to support the interim operational use of the cloud coverage pictures by the weather services. The third phase of the program calls for the establishment, with the Weather Bureau, of an operational meteorological satellite system. This system will eventually make it possible for the Weather Bureau and other weather data users to use satellite observations much as they now use ground and balloon observations. Finally, we have an interest in those specialized sounding rocket techniques which are directly adapted to meteorology. Our interest includes the development of specialized meteorological sensors which can be used in small sounding rockets in regular support of meteorological activities. In the development of this particular phase of our program we are working closely with Weather Bureau and Department of Defense (DOD) groups doing research in the upper atmosphere and in aeronomy.

Figure 1 shows the three meteorological satellites with which we are currently concerned. Four Tiros satellites, of the design shown on the left, have already been orbited. The Nimbus, which we now have under development and construction, is a meteorological satellite with much improved capabilities. The Aeros satellite is shown in a conceptual form on the right, inasmuch as we have not yet initiated a flight program for this satellite. Each of these satellites and some of their system elements will be discussed in somewhat more detail subsequently.

The types of sensors used in Tiros are shown in Figure 2. The primary sensor is the TV camera which, with the wide-angle lens used in all Tiros satellites, covers an area on the ground about 800 to 1,000 miles across when looking straight down from the orbital altitude of 450 miles. One set of infrared radiation sensors is incorporated into an instrument called a multichannel scanning radiometer which scans the Earth and atmosphere as a result of the rotation and orbital motion of the satellite. The channels of this instrument are so designed that data are obtained corresponding to:

a. The temperature of the uppermost layers of water vapor in the atmosphere (6 to 6.5 micron band).

b. Night cloud temperatures (8 to 12 micron band).

c. The total amount of reflected sunshine (0.2 to 6 micron band).

d. The total amount of heat emitted from the atmosphere (8 to 30+ micron band).

e. Low-resolution cloud coverage (0.55 to 0.75 micron band).

The second of these channels, which measures the night cloud-top temperatures, can be used for rough nighttime cloud cover mapping. It must be remembered, though, that these sensors do not produce a direct TV-like picture when collecting the data. Many independent scans, each containing the data on the temperature variation along one line drawn across the surface of the Earth, are recorded. Analysis and processing are rather lengthy procedures as yet and additional study and development on high-speed data processing methods are required before routine inclusion of such infrared data in daily forecasting can take place. Of the possible applications of the infrared data, only the cloud cover and temperature studies have been investigated. Yet, it is entirely possible that infrared radiation data may ultimately turn out to be even more valuable to the meteorologist than the cloud pictures.

Tiros has carried two additional sets of radiometers. Their major difference is the size of the area viewed at any instant. One sees an area about 400 miles on a side. Although the other can see the entire area from horizon to horizon, it is particularly sensitive to energy in a circle about 1,000 miles in diameter centered just below the satellite. Each set contains both a white and a black sensor. The white sensor measures total emitted or outgoing radiation. The black sensor measures this plus that part of the incoming solar energy reflected by the Earth, the sea, and especially clouds. By subtraction, we can separate the reflected energy and, by again subtracting this from the energy known to be coming from the Sun, we can determine how much solar energy was retained to drive the atmosphere and create the weather. It is hoped that meteorologists can in time use these data to develop improved long-range forecasting techniques.

Figure 3 indicates a typical orbital path of a Tiros satellite. Tiros is injected into an inclined orbit. The figure indicates the maximum limits of latitude (about 50° north and south) to which Tiros normally has been providing observations because of the inclination of the orbit. We are now planning on increasing the inclination to about 58°. This should give coverage to a latitude of about 65°.

Tiros has enabled us to make specialized observations of storms and hurricanes. Figure 4 is a photograph of hurricane Betsy which was picked up by Tiros in the western Atlantic Ocean in September 1961. This satellite photograph of the hurricane corresponds to the hurricane pattern as seen on radar and to a plotting symbol the meteorologists have used for many years. The ability to identify hurricane cloud patterns, to locate them with respect to land masses, and to follow their motions are some of the major benefits of the meteorological satellite.

Figure 5 shows in an abbreviated form the Tiros global cloud analysis for September 11, 1961. This figure portrays a map of the world from a point above the North Pole. Each finger-shaped area shows in a very condensed way the cloud information from a sequence of Tiros photographs all taken on this one day. The dark shading represents clouds; the light shading, clear areas as seen by the satellite. For this type of work the photographs are converted to a graphical notation by meteorologists at the data readout stations and the cloud cover maps are sent by wirephoto to the U. S. Weather Bureau's National Meteorological Center at Suitland, Maryland. At that Center they are combined on maps such as this one and further distributed to weather stations all over the world. The major storm and hurricane centers which could be identified in this global analysis are indicated in Figure 5. Six distinct tropical storm centers, which were located by the detailed analysis of the data used to make this map, are indicated.

Figure 6 indicates one type of data that the meteorologist derives from Tiros infrared observations. The top map shows temperature contours; each line represents a condition of constant temperature as observed by the particular infrared sensor in the Tiros satellite which gives cloud-top and Earth surface temperatures. The letters W and C are placed to show warm and cold locations, respectively. Below this is shown, for correlation, the cloud cover information from ground observations for the same time period at which the infrared data were recorded. The lines show the altitudes, in thousands of feet, at which the cloud layers were located. Note how the cold areas in the upper chart correspond to the cloud (shaded) areas in the bottom one, whereas the warm areas are clear. Also, the coldest areas are those where the clouds extend to the highest altitudes. This correspondence shows how we could use such data to observe general cloud coverage at night when we are able to process the data without delay.

Our program calls for three additional Tiros launchings at approximately four to six month intervals so that we can continue to supply cloud cover pictures and infrared data to meteorologists both here and in foreign countries. The cloud pictures can be used for support of regular meteorological operations while both types of data will be available for meteorological research investigations. These Tiros satellite launchings are scheduled to continue until the time when the first Nimbus satellite is expected to be ready to support meteorological operations.

In view of the limited coverage of Tiros, it was apparent at the start of that program that the next step forward would be a satellite whose viewing axis was stabilized in such a manner that the various sensing systems, especially the TV camera, always point toward the earth. Furthermore, to provide full global coverage, we will use a nearly polar orbit. It is also helpful to have the observations always made at the same time of day, at noon, for example, when the satellite is on the illuminated side of the Earth where the present TV cameras can work. Fortunately, analysis shows that it is possible to do this. With the expected accuracy, we should be able to stay reasonably close to this condition for the estimated useful satellite lifetime.

Figure 7 shows Nimbus in near polar orbit. Because Nimbus is Earth stabilized rather than space stabilized, the TV camera which is mounted on the bottom ring will always be looking at the Earth. Inasmuch as a full hemisphere is always illuminated by the Sun, with a properly selected initial orbital injection time we can always view the subsatellite point over half an orbital period without difficulty. It is presently planned that this will be the northward moving half of the orbit for Nimbus. In order to keep the cameras pointed at the Earth and the paddles pointed at the Sun, it is necessary to provide for paddle rotation as the satellite travels in its orbit. The two control and data acquisition station locations are indicated in the figure.

The Nimbus satellite is shown in Figure 8. It can be seen that the satellite has large solar cell paddles. There is an upper section which houses the controls; attached to this control housing are the horizon scanners. The horizon scanners are used to activate the controls which maintain the axis of the satellite pointing toward the center of the Earth. The control system achieves this by turning flywheels, which are known as inertia wheels, with small electric motors. Should these inertia wheels reach the end of their control range (that is, if their maximum speed is reached) gas jets attached to the control section are triggered. These gas jets are used to keep the satellite stabilized while the wheels are slowed down. The conical structure above the control section is simply the upper portion of the command antenna. The straight tubular structure below the control section connects it to the sensor ring; a satellite configuration with such a separation uses the gradient of the Earth's gravity field to help realize the desired Earth oriented stabilization. The circular tubes are also a part of the antenna. The sensor ring is so constructed that a large number of electronic modules of standardized sizes can be accommodated.

At the bottom of the ring are two units containing the infrared scanners and a bank of three TV cameras which are arranged to produce a field of view approximately as shown on the ground below. The three cameras cover overlapping ribbonlike swaths on the ground from pole to pole as the satellite moves along from orbit to orbit. The infrared scanners will cover a similar strip, actually looking from horizon to horizon.

Figure 9 is a photograph showing a mockup of the prototype satellite which is now being assembled. In developing Nimbus, we are making an effort to design the spacecraft so that future spacecraft, whether for the purpose of research and development or for routine operational use, can be assembled rapidly with a minimum of modifications to the basic structure although completely new sensory subsystems may be added. This theory is illustrated in Figure 10 where on the left we have shown the basic Nimbus system with its fundamental elements of power supply, controls, and sensory package. In the center it is shown as it might be modified if, in the course of the development activity, a decision were made to test fly a rainfall radar system. If a special infrared spectrometer, now being developed by the U. S. Weather Bureau to measure the vertical temperature profile of the atmosphere were desired, we might mount this in the center of the sensory package ring, where the TV cameras are now located. The spectrometer package is indicated in outline form in the sketch on the right.

The need for several data acquisition stations for the Nimbus system has already been mentioned. At whatever altitude a satellite orbits, it must be visible at a sufficient height above the horizon for data acquisition to take place. An examination of the geometric conditions that are set by the Nimbus orbital requirements leads to the conclusion that only by placing a station extremely close to the North Pole could we cover all of the regular daily orbits and so realize global coverage. Alaska is a practical location, which can be used and which is fairly close to the North Pole. To complete the coverage, a station diametrically opposite to Alaska at the other side of the North Pole would satisfy the simple geometrical condition. Such a location would be impractical in an engineering sense, even if it were available politically, as there are no reasonable cost communication channels available for the transmission of the data to the central data processing location. For this reason, a second station on the east coast of the North American Continent and as far north as it is practicable to provide the necessary communications is being considered.

The Congress appropriated $48 million to the Weather Bureau in Fiscal Year 1962 for the initiation of a meteorological satellite system, based on the Nimbus program, which is to be ultimately capable of continuous observations. NASA and the Weather Bureau have since that time drawn up an agreement covering the responsibilities of the two agencies and have initiated a project development plan for the Nimbus Operational System.

With the Nimbus development program already underway and the assembly of the prototype progressing, the addition of the Weather Bureau's funding will result in the procurement of hardware for additional Nimbus satellites of the basic design. We then plan to be prepared to launch the Nimbus units about twice as frequently as was planned when the original research effort was scheduled. If all goes well, starting sometime in 1963 we would hope to have at least one working Nimbus in orbit nearly all the time.

The Aeros concept is illustrated in Figure 11. Aeros is conceived as a satellite which will be placed in equatorial orbit at an altitude of 22,300 miles. This will result in its remaining stationary at a given location over the equator from which it can view tropical and temperature latitudes. Aeros will carry, in much the same way as Nimbus, solar-cell paddles for power, a controls section, and a camera section. The unusual features of the Aeros system are associated with its stationary position with respect to the Earth. With this satellite, it is planned to undertake continuous surveillance of the weather systems on the one-fourth to one-third of the Earth visible below the satellite. Thus, we will have a capability for the continuous tracking of a particular storm as it moves over the surface of the Earth. The figure indicates the two TV patterns considered for Aeros. One is obtained with a relatively wide-angle type of camera which records and transmits pictures of all major cloud systems. The other Aeros camera will be one of relatively high resolution, and very narrow angle of view, compared with the one which covers the total hemisphere. The camera will be directable on command so that any selected area can be brought into the field of view at any time. The areas will be chosen from the wide coverage data.

Why is it that we wish to have another meteorological satellite at such a high altitude, when the Nimbus satellite should give us 100 percent coverage of the Earth's surface? Our calculations indicate that 14 orbits elapse before Nimbus returns and passes in daylight over about the same area of the Earth as it did in its first orbit. Roughly the same area is covered, but in a pass in the opposite direction and at night, every seven orbits. This means that about every 12 hours we get another observation of a given spot on the Earth's surface, but are limited on alternate passages to the use of the infrared techniques, until we can develop a TV camera that can see clouds at night.

The importance of more frequent or continuous observations is illustrated in Figure 12. In this figure we have shown the typical lifetime of certain common weather systems: tornadoes, thunderstorm cells, hurricanes, and major cyclonic storms. The figure shows the general relation which exists between the lifetime of these systems and their typical size. We have also shown the time between observations which will result if one Nimbus satellite is up (about 12 hours) and if two Nimbus satellites are in orbit. In both cases we are assuming we are using the nighttime as well as the daytime observations. With one Nimbus in orbit, about half a day passes before we again see the same area. This will be a reasonably satisfactory time for the observation of such systems as cyclonic storms and hurricanes whose normal life is considerably greater than 12 hours. But, if we attempt to observe thunderstorm cells or tornadoes, systems whose life is appreciably less than 12 hours and often less than 6 hours, they will usually form, move, and die out without ever being detected. It is for this reason that the Aeros satellite, with its capability for focusing on small short-lived storm systems and tracking them continuously, is considered to be an integral component of the eventual operational system.

Another aspect of the meteorological research and development program is reflected in the effort devoted to the continuous improvement of meteorological sensors, electronic and mechanical components, and systems configurations. These activities are coordinated with similar program studies arising in other areas of interest in order to so direct the work that more than one flight development program can benefit. New types of infrared sensors for the measurement of quantities of meteorological interest, and new data storage devices for use in the satellites are typical of the items covered in the advanced research program.

Meteorological observations made with sounding rockets supplement standard ground-station observations, balloon sounding, and the satellite data. In particular, we have been working with a method known as "grenade sounding." In these tests, a series of small explosions is produced at different altitudes by ejecting grenades from a sounding rocket. The time of arrival of the sound wave from each explosion is measured at a group of microphone stations on the ground. The location of the grenade at the time of firing is also determined with radar and/or optical instruments. The use of appropriate data reduction procedures then enables us to determine both the winds and the temperature in the upper atmosphere up to the altitudes at which the last grenade was exploded. Figure 13 shows the general arrangement for such a sounding. The facilities include a launcher for the sounding rocket, the Dovap system, which tracks the rocket so as to locate it in space at the instant of an explosion, and microphones, represented by the numbered locations in the vicinity of the Dovap stations. The photograph in the upper right corner shows a typical grenade head for the sounding rocket.

Two typical results which have been extracted from data for many sounding rocket flights at Fort Churchill, Canada, at a latitude of about 59° north are shown in Figure 14. Note that the winds change appreciably from the winter season to the summer season. This phenomenon could not have been determined without the rocket soundings because only the lower altitude conditions can be measured with conventional balloon soundings. Furthermore, we know of no satisfactory techniques for observing these altitudes from satellites. The transition point at which the wind direction shifts is where the balloons start to reach their limit and the data shown from 100,000 feet up have all been acquired with sounding rockets.

Communications System Program. It has been obvious for some time that the communications satellite has a tremendous potential for improving our ability to communicate with the other nations of the world and for making this capability available to all. The improvements possible in channel capacity and in cost per channel may lead to the substitution of satellites for the undersea cables on which we now depend for transoceanic telephone communications. Economic studies which have already been undertaken indicate that with the development of the communications satellite it will be possible to provide services closely comparable in performance to those of the underseas cables at lower costs per channel than we now experience with the undersea cables, and that we will be able to expand the system to a capacity well beyond that which would be economic with cables alone. Related studies on the number of channels that are predicted as being required for overseas communications indicate that in a decade or so the addition of communication satellites will be almost a necessity, if we are to keep up with the demands of service to other countries.

Although the usefulness of the communications satellite and its importance in worldwide communications is clear to all, it is not immediately apparent which of the technical approaches to the problem will turn out to be the most rewarding in arriving at the design of the operational systems of the future. Accordingly, NASA is endeavoring to determine, as rapidly as possible, which of the various system designs which have been proposed should be used in the establishment of operational communications satellite systems.

The objectives of our communications systems program are given as follows: (1) the demonstration of the feasibility of both the active and passive communications satellite techniques; (2) the establishment of the operational system; and (3) the support of operational communications satellite systems through continued research, development, and flight tests.

Three major communications satellite systems offer sufficient promise to warrant continued detailed investigation. The first of these is the system using low- or intermediate-altitude passive reflectors. The second uses low- or intermediate-altitude active repeaters, and the third depends on high-altitude, synchronous, active repeaters. The passive, or reflector satellite does not carry with it any power supply, receiver, or transmitter. It is used simply to reflect the radio energy from one terminal of the communications satellite system to another. Active repeaters draw their name from the fact that they carry receivers, transmitters, and sufficient power supply, so that the message to be transmitted is received, amplified, and retransmitted to the far terminal. By low or intermediate altitudes, we mean from several thousand miles to as much as twelve thousand or so miles. By high altitude, synchronous is meant the 22,300-mile orbital condition already mentioned in connection with Aeros, in which the satellite apparently remains fixed over a point on the Earth's equator.

Figure 15 shows pictorially the elements of a low-altitude active communications satellite system. It is characterized by terminal stations with large antennas and by a number of satellites orbiting at low altitude. The reason for the large number of satellites indicated on the chart is that the time any one of these satellites is visible to both of the terminal stations is limited when the satellites are in orbit at low altitude. Consequently, to get continuity of transmission between the two terminals, a number of satellites must be put into orbit, and these must be so distributed in space that at all times at least one can be used as a communication link between the terminal stations.

A realistic appraisal indicates that unless fairly complex provisions are included for controlling the position of the individual satellite in its orbit, it must be expected that the satellites will come after a period of time to an essentially random set of spacing.

An example of the results of calculations on the number of satellites that are required for the maintenance of communications at several orbital altitudes is given in Figure 16. Here we see how many satellites are needed for substantially continuous service between ground terminals located some 3,000 miles apart. Note that if we were to orbit the satellites at an altitude of only 1,000 miles as many as 400 randomly distributed satellites would be required. If we plan to use the 5,000-mile-altitude range, then we can immediately cut the number of satellites required to 40. Suppose we plan on using the 22,300-mile altitude, (which is about as high as we would wish to go for reasons I will mention in a moment), 19 satellites are shown to be required by the analysis. In actuality, if we were to orbit the satellites at 22,300 miles, and it were possible to exert effective precision control of their orbital velocity, we would not use randomly distributed satellites at all but would establish what is known as a synchronous satellite system.

The synchronous satellite system is illustrated in Figure 17. It is of great interest in communications work, because at an altitude of 22,300 miles the spacecraft will remain fixed over a point on the equator and it is only necessary to have three units in orbit to provide for basic worldwide coverage. As it is by no means certain how long it will take to arrive at a control configuration of adequate reliability and precision, research on the synchronous system is being conducted parallel with that on low-altitude systems. The DOD's Project Advent is directed to the development of a military communications satellite system of the synchronous-orbit type.

I have already mentioned that a communications system with the satellites at low altitude will require a number of satellites in orbit simultaneously. In addition, spacecraft and launching costs are likely to accumulate rapidly with such a system. We are studying techniques whereby we can launch a number of satellites into orbit from a single launch vehicle and have initiated work in this area which we expect will lead to a flight experiment within the next few years. The multilaunch concept, which is illustrated in Figure 18, calls for the insertion of a number of satellites into orbit from a single launch vehicle. Three are shown in this figure. The launch vehicle leaves the launching pad and goes into an elliptical orbit. This puts the spacecraft, which is carried by the launch vehicle and which has within it the three communications satellites, also into the elliptical orbit. When the spacecraft carrying the three communication satellites reaches its apogee, the first of the communication satellites is ejected and an additional rocket propulsion unit attached to the satellite is fired. This satellite is therefore put into a circular orbit about the Earth because of the additional velocity that has been given to it. The next satellite is ejected at apogee of the second orbit and the remaining satellite is ejected at apogee of the third orbit. These satellites are also given sufficient additional velocity by appropriate rockets to maintain circular orbits. So the spacecraft has, in the course of three of its elliptical orbits, put three communications satellites into circular orbits. A procedure for multiple satellite injection seems to be a necessity, if we are to proceed with operational low-altitude communications satellite systems.

Basically, the simplest of all the communications satellite techniques is the passive reflector—the radio mirror in the sky. At the present time NASA is investigating four of these configurations, as they appear to have the most promise for immediate application. These four are shown in Figure 19. The first is the simple reflecting sphere. This has been tried already in Project Echo. As a follow-on to the simple sphere with a continuous surface, there are two varieties of surface structure which promise better overall performance so far as the passive reflector communicatons system is concerned. One of these is a sphere which has been lightened by etching holes in the metallic foil which constitutes the reflecting surface. This reduces the weight and makes it possible to orbit a larger satellite, which will have correspondingly improved performance, with the same launch vehicle. An alternative is to make the sphere of an appropriately sized wire mesh. Inasmuch as we wish to make the sphere very much larger insofar as its reflection characteristics are concerned, we are also looking into the possibilities of effectively accomplishing this without actually orbiting the whole sphere. In normal circumstances only the bottom face of the sphere, the portion facing the Earth, is actually used in reflecting the signal from one station to another. For this reason it seems rather unnecessary to orbit the upper portion, which serves no purpose in the communications satellite system. The problem then becomes one of erecting, and then stabilizing, the spherical segment that is placed in orbit to hold the reflecting face towards the Earth. Studies of the spherical-segment reflector and of the stabilizer will therefore be carried on together because, without the second, the first is of little use.

The passive satellite program of NASA includes several flight tests. The properties of the sphere as a communications reflector are being evaluated in these flight tests. The first of these tests was the Echo I project. The left side of Figure 20 shows that Echo was, at launch, 100 feet in diameter, weighed 135 pounds, and was constructed of mylar plastic film 5 ten thousandths of an inch in thickness. This is approximately 500 millionths of an inch in thickness, about one-quarter of the thickness of a human hair. The mylar film was made reflective to radio waves by evaporating an aluminum film onto the plastic in a vacuum chamber. After the Echo sphere had been in orbit for some time it exhibited a certain amount of wrinkling and there were small changes in its shape which are associated with the loss of the original volume of inflation gas. As the sphere wrinkled, it became less acceptable as a communications reflector, since a smooth surface is the best reflector. Accordingly, we have now built a new version, Echo II, which is shown on the right (Fig. 20), a sphere which will maintain a fairly smooth surface characteristic. In designing this reflector, we took advantage of the availability of a large booster vehicle to make the sphere larger. It will now be 135 feet in diameter, and will weigh about 500 pounds. In this case the sphere is made of a laminate, a combination of two layers of aluminum foil, and one of mylar. The total thickness will be 750 millionths of an inch. This is 50 percent thicker than Echo I, but is appreciably stiffer because of the presence of the two layers of aluminum foil. When this sphere is inflated (Fig. 21) and somewhat overpressured, it takes on a permanent set, and does not tend to resume its earlier wrinkled condition when the gas pressure drops. The major objective of the Echo II project is to show that a rigidized structure of this type is a practical one as a passive reflector.

The specific versions in the NASA active communications systems program are as follow.

The Relay developmental spacecraft is shown in Figure 22. This is the test model which has been built by the Radio Corporation of America, the prime contractor for Relay. An artist's conception of the Relay satellite in orbit is shown in Figure 23. The shaded area indicates the antenna patterns of the Relay satellite. Notice that it is doughnutlike in form so that even though Relay spins on its long axis, some of its energy will be radiated toward the Earth. A magnetic attitude coil similar to that used in Tiros will be used periodically to adjust the orientation of this axis to a favorable position with respect to the ground stations cooperating in the Relay experiment.

NASA is cooperating with the American Telephone and Telegraph Company on the Telstar project (Fig. 24). This project was initiated by AT&T and has as its objective the investigation, in orbital flight, of the technical and operational problems of transmission of wide-band communications by an active communications satellite. The Telstar satellite which is being built by the Bell Telephone Laboratories is of a different configuration and has a number of technical details which differ from those of the Relay satellite. The division of responsibilities for the work that is being carried on by NASA and AT&T for Telstar is as follows: NASA is responsible for the establishment of the environmental test specifications of the spacecraft, will procure the launch vehicle, launch the spacecraft, and will undertake the orbital data acquisition necessary for the operation of the ground tracking stations; AT&T is responsible for both spacecraft and ground station development. Both NASA and AT&T will undertake data analysis and processing so as to support the development of future spacecraft and ground systems. The data resulting from the project will be exchanged without any constraints between the two organizations and will also be available to others in this country and in the international community.

Both Relay and Telstar have already given rise to a great deal of international interest and NASA has entered into agreements for experimental work with the Relay satellite with Great Britain, France, Germany, Italy, and Brazil. Discussions with other countries have taken place, and it is expected that additional agreements will result.

The third active satellite project in our present program is Syncom, which is being built by the Hughes Aircraft Company. This is NASA's initial effort directed to the development of the synchronous satellite. Its objectives are to provide experience in using communications satellites in a 24-hour orbit at the earliest possible time; to develop the capability of launching satellites into the 24-hour orbit using existing launch vehicles plus additional "apogee kick" rockets; and to test the life of communications satellites components at the 24-hour-orbit altitude. An artist's conception of the Syncom spacecraft is shown in Figure 25. Of interest is the addition to the spacecraft of the apogee rocket motor, the control jets for orientation and positioning, and the solar sensors. The traveling wave tube is used in the transmitter of all three active satellites because of its excellent performance characteristics.

It is particularly useful when a wide range of frequencies is to be amplified as in this case. The initial injection of the Syncom satellite will be from Cape Canaveral into a very highly elliptic inclined orbit with an apogee of 22,300 miles. (See Fig. 26.) As a result, at apogee it will be at the desired altitude for synchronous rotation with the Earth. However, it will not have had enough energy imparted to it to stay at that point, and the internal apogee rocket must be fired to add the energy required. When the satellite is separated from the lower stages of the Delta launch vehicle, it is spun about its axis and so it is rotating much as a gyroscope would rotate as it comes up to altitude. When the apogee rocket is fired, the satellite will be injected into a circular orbit. At this time the attitude control jet in the end of the satellite can be actuated by ground control to provide a force to turn the satellite. The result of this operation is indicated in Figure 26. The satellite then appears to be rotating on its axis like a wheel as it moves in its orbit.

Figure 27 shows the satellite in its orbit above the earth. It is more than likely that the satellite's speed in orbit will be somewhat too fast or too slow for an exact match with the Earth's rotation and it will tend to progress or retrogress around the Earth. A result of the inclination of the orbit is to make the point below the satellite trace a path resembling a series of connected figure eights as shown in Figure 27. An additional element of ground control is then available in the positioning gas jet in the side of the satellite. This force will slow the satellite down or speed it up in its orbit depending upon the direction in which the jet is pointed when it is operated. This controllable jet can be used to reposition the satellite to the location where we would like to have it for communications experiments.

The final position of the Syncom satellite is indicated in Figure 28. It will still be rotating about its axis and will be moving in a figure-eight pattern about 30° above and below the equator. The antenna pattern of Syncom is indicated on the right in Figure 28. It resembles that of Relay in that it is uniform around the satellite axis and so the rotation of the satellite will not affect its performance.

NASA and the Department of Defense both have responsibilities in the Syncom project. NASA is responsible for the spacecraft development and launching and for the orbital data acquisition and processing. The Department of Defense is responsible for the development of transportable ground stations for use with Syncom. Much experience has already been accumulated within the Department of Defense on the Advent ground station design for this type of service. It was possible to take advantage of this experience by arranging with the Department of Defense for a supporting effort for the Syncom project. Both NASA and DOD will undertake data analysis and processing to support further research and development on both the spacecraft and the ground systems for synchronous satellites.

We are now formulating and establishing the advanced research and development for the communication systems which will follow the first phase of the passive and active satellite project developments just discussed. Several major areas in the general category of advanced research and development are: studies of ground systems components, satellite systems components, attitude stabilization, and an evaluation of the effects of the orbital environmental radiation. In considering ground systems components, detector units, receiver circuits, transmitters, modulators and antennas are of primary concern. Insofar as the satellite systems are concerned, it is obvious that the testing of particular receiving and transmitting systems and power supply arrangements will be required. Studies of the configurations needed for redundancies in space borne systems in order to develop the reliability required for long operational life will have to be continued.

Attitude stabilization for passive satellites is vitally important if we are to get away from the sphere as the only effective reflective satellite structure. Both the synchronous satellite and the low-altitude satellite will also show a great deal of performance improvement if we can develop a stabilization system which will eliminate the need for satellite spin and which will hold the satellite in a fixed attitude relative to the Earth. We could then use an antenna on the satellite which will focus in the preferred direction to improve the power transfer both from the Earth to the satellite and from the satellite back to the ground. Accordingly, from the very first we are trying to concentrate on attitude stabilization system designs which have inherent simplicity and reliability.

Low-altitude satellites will generally spend a fair percentage of their life passing through the Van Allen radiation belts. For this reason, we already have planned an investigation of the environmental radiation of these belts with both the Relay and Telstar satellites. Both of these will carry radiation monitoring instrumentation and radiation damage detectors. These radiation investigations will continue until we have sufficient engineering experience and an adequate statistical basis to determine just what protection the various electronic and power supply components must be given in the operational communications satellites.

In the advanced satellite flight program which will follow on beyond Relay and Syncom, performance improvement of the active satellite systems is the primary objective. The first step in this direction is to take advantage of the growth that can come about as we move to larger launch vehicles (see Table I.)

TABLE I

Systems	Launch Vehicle	Weight lb.	Orbit Statute Miles	Orbit Shape	Channel	Stabilization	Per cent of time available	Number of stations
Low altitude								
Relay	Delta	150	700 to 3,000	Elliptical	1 television	Spin stabilized	10	2
Advanced satellite	Atlas-Agena B	600	Up to 12,000	Circular	4 television	Earth oriented	25	Many
Synchronous								
Syncom	Delta	55	22,300	Inclined	1 telephone	Spin stabilized	75	2
Advanced satellite	Atlas-Agena B	500	22,300	Equatorial	4 television		100	Many

Future Applications

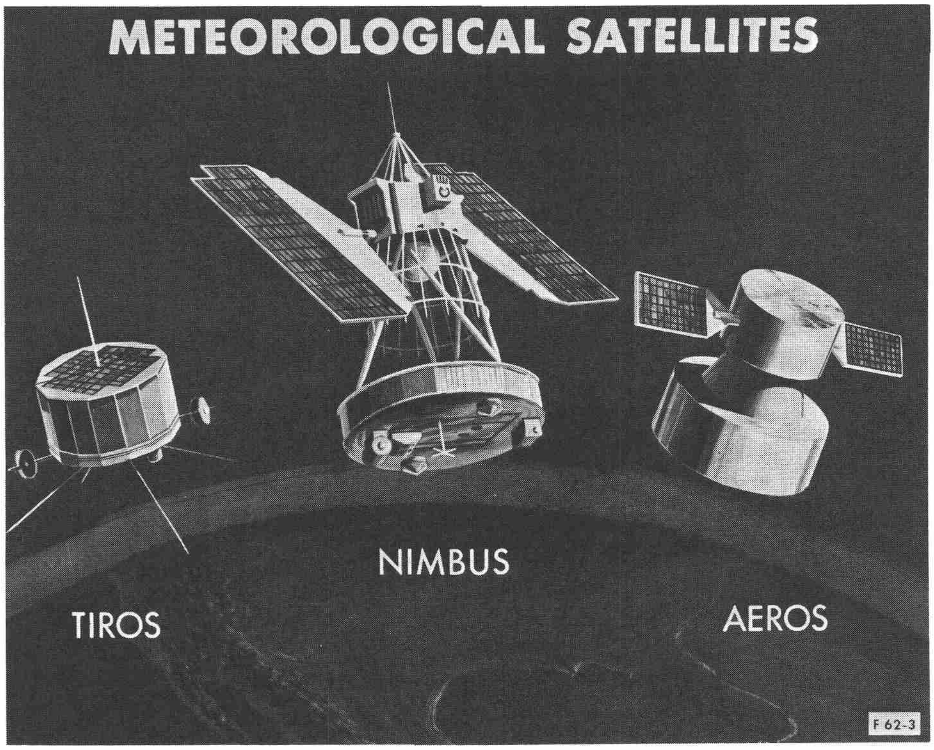
There are two segments into which we can divide Future Applications. One of these is other satellite applications. Typical of this sort of application might be a civilian navigational satellite effort. Navigational satellites are presently under development by the Navy in Project Transit and this program is coming along quite rapidly from a military point of view. There may be an area of civilian application for Transit and it is the intention of the NASA to work with the Department of Defense and the Navy to explore this possibility.

The other segment of applications to which our office is directing its attention is that of identifying within the NASA research and development program those new techniques or processes which can be used by a wide range of industrial organizations. To accelerate the process of getting these new data to potential users, we must (1) recognize a process, material, or device as having potential industrial value; (2) catalog the useful properties or values so that we can refer the new development to anyone who has an interest in a specified area; (3) take action to inform those who have potential use for the new idea so that, as rapidly as possible, the benefits of new developments are fed out to the various segments of industry which can make use of it; and (4) continuously evaluate and refine our procedures for processing applications information, so that we arrive at the most effective way of accomplishing the three activities just discussed.

Concluding Remarks

In summary, the Office of Applications has three major areas of activity in its program: meteorological systems, communications systems, and future applications. The first two of these are programs for satellite systems development. A specific line of satellite development is already underway in meteorological systems.

In communications we feel that quite probably the ultimate system may use the synchronous orbit satellite. Inasmuch as this is a difficult technical development, in the interim, efforts leading to the establishment of a communications satellite system using low- or intermediate-altitude active repeaters have been initiated. At the same time, since the ultimate value of the passive satellite in the range of possible communications satellite systems is not determined, we are continuing to investigate the construction and utility of passive satellites. The experimental work on the synchronous satellite will be used in the development and evaluation of techniques, which if proven feasible, should make it possible to realize an operational system within a reasonable period of time. In the area of future applications, our present effort is to locate within the NASA research and development program those new techniques, processes and devices which can be of benefit to the civilian economy.



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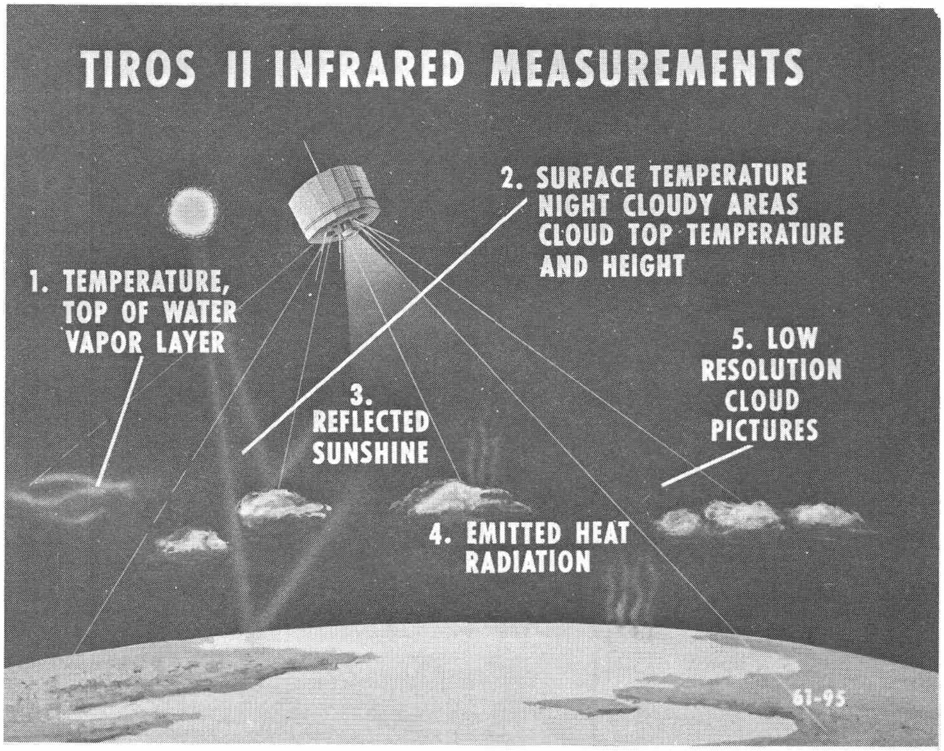

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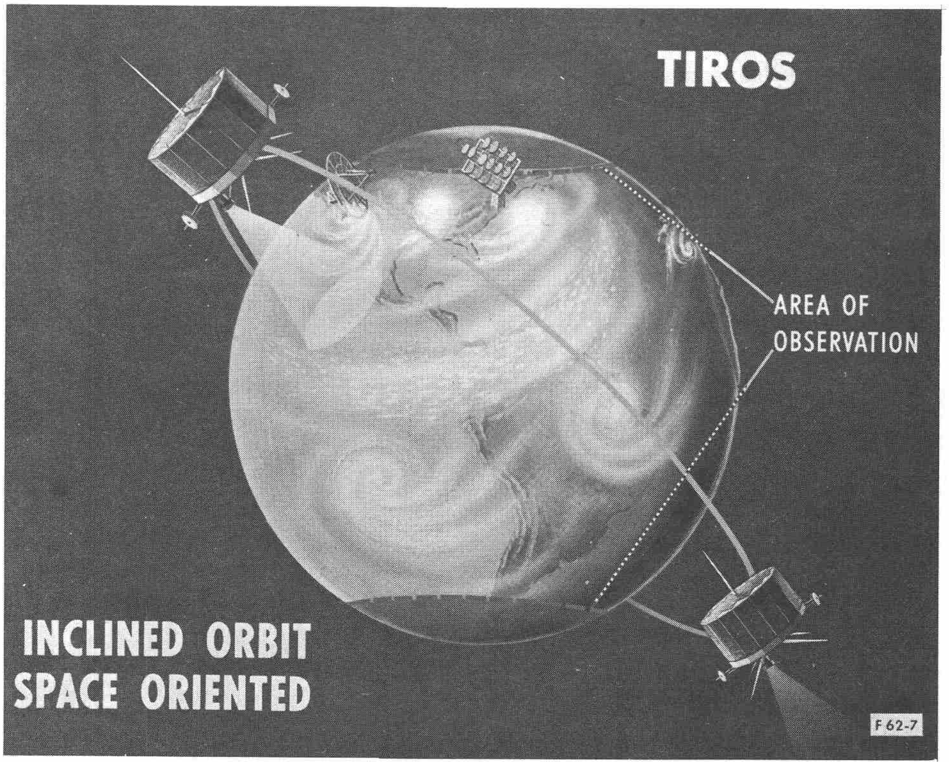

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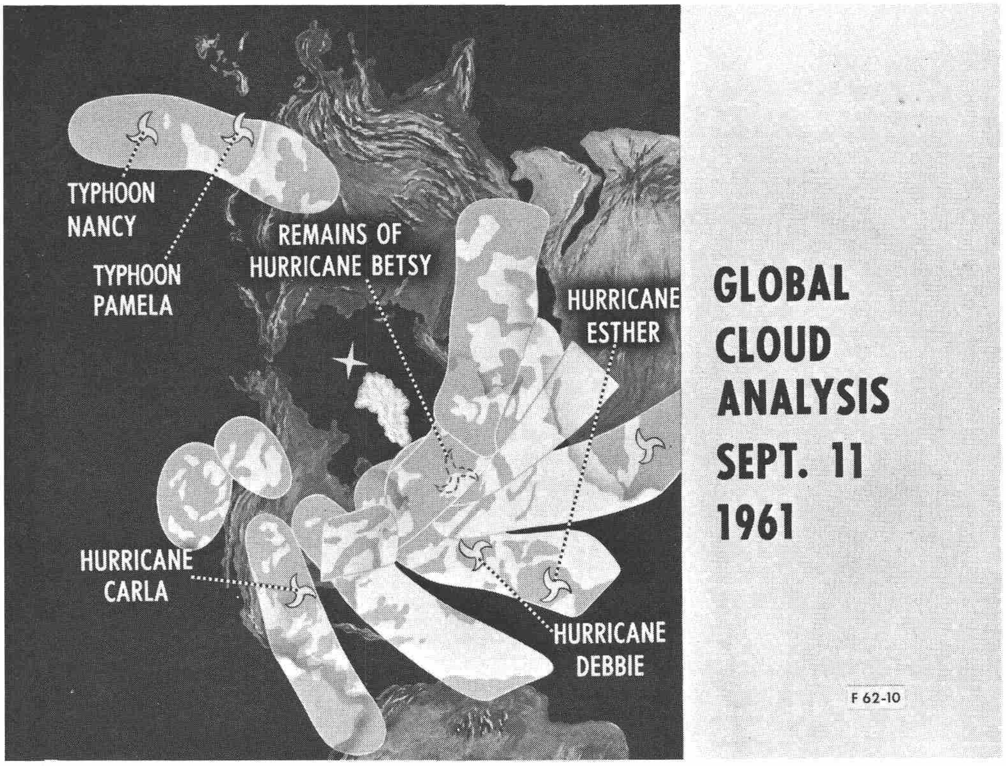
Figure 4



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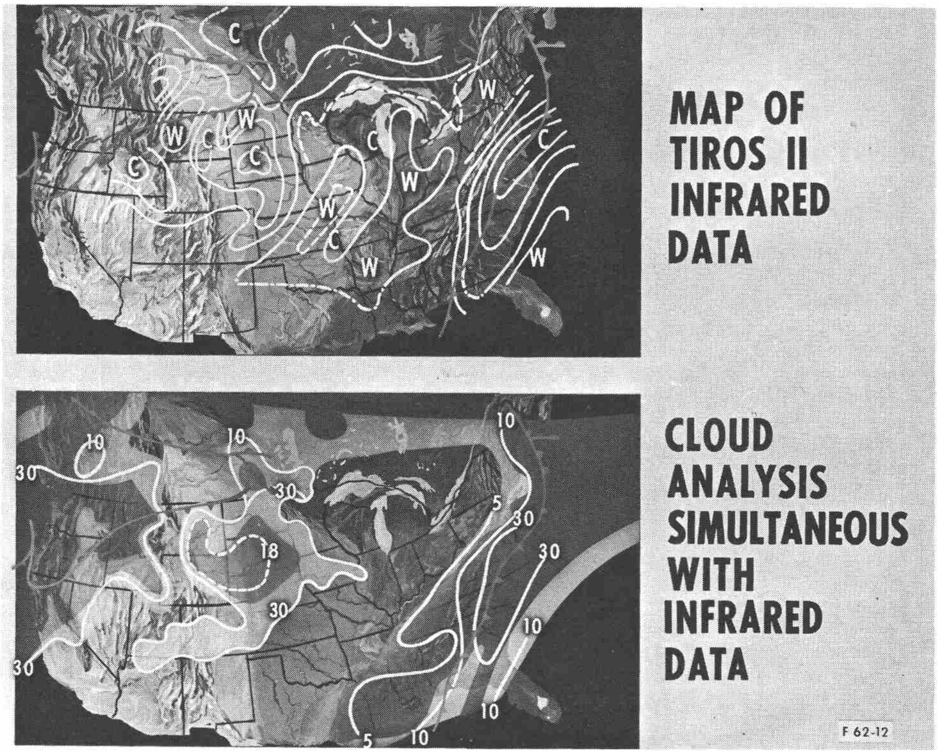

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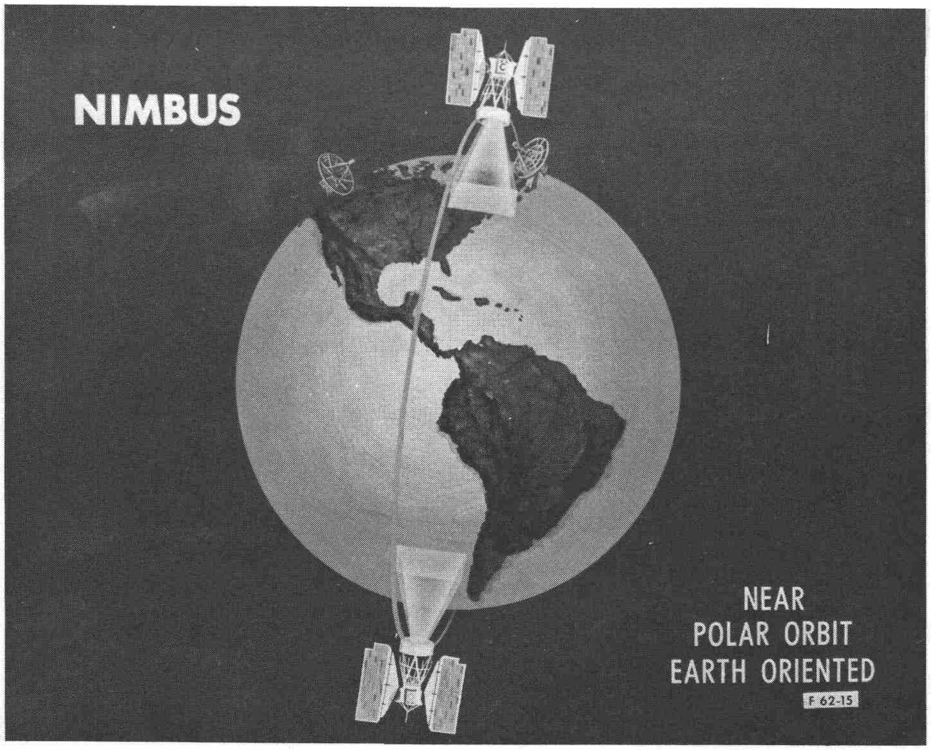

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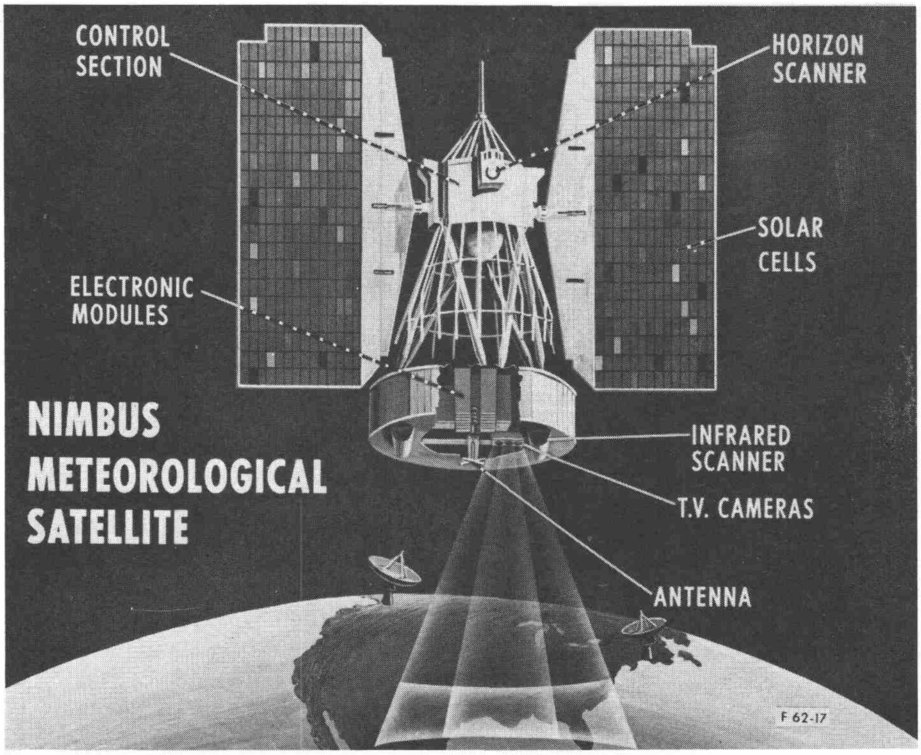

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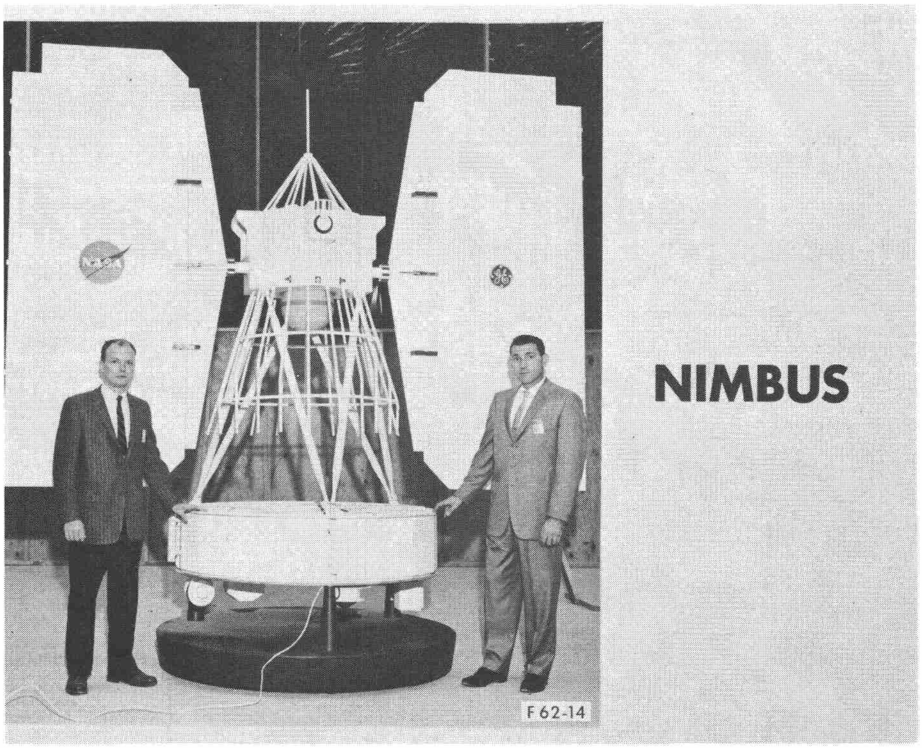

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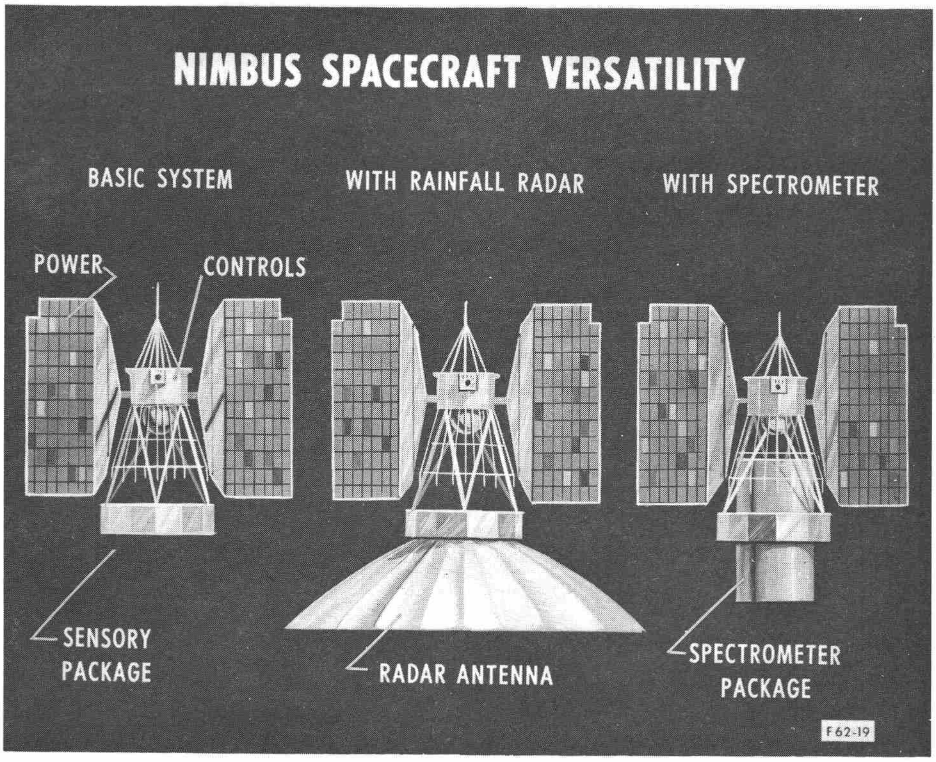

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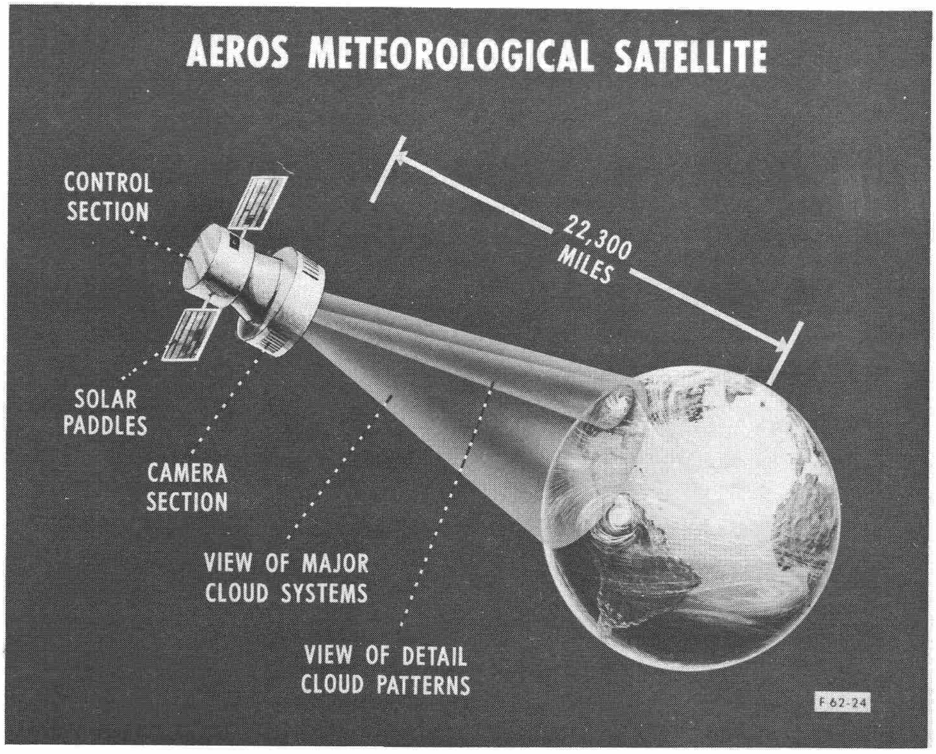

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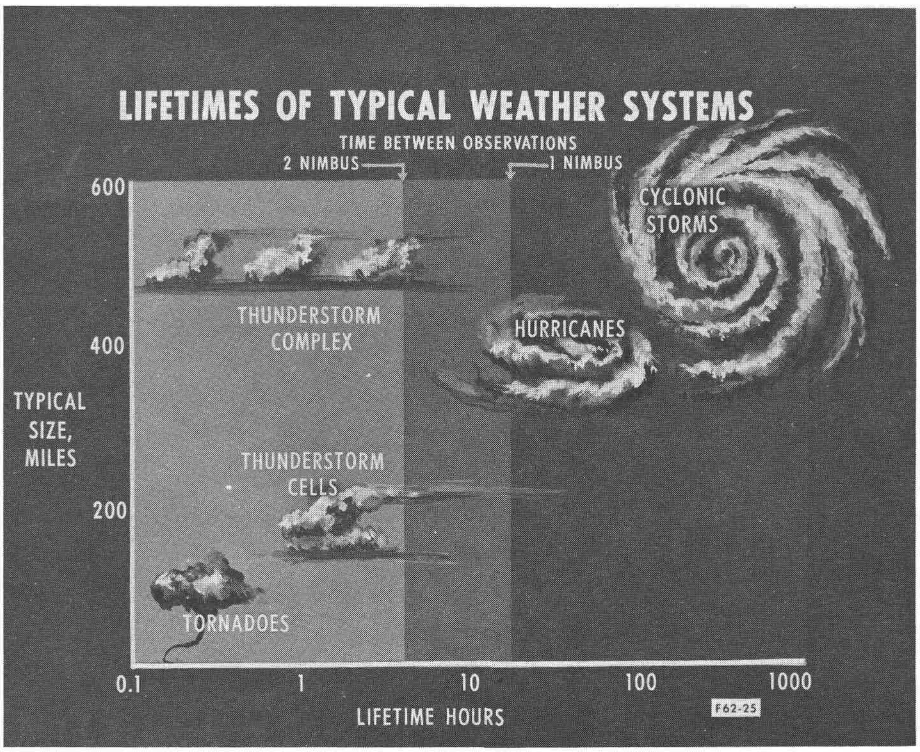

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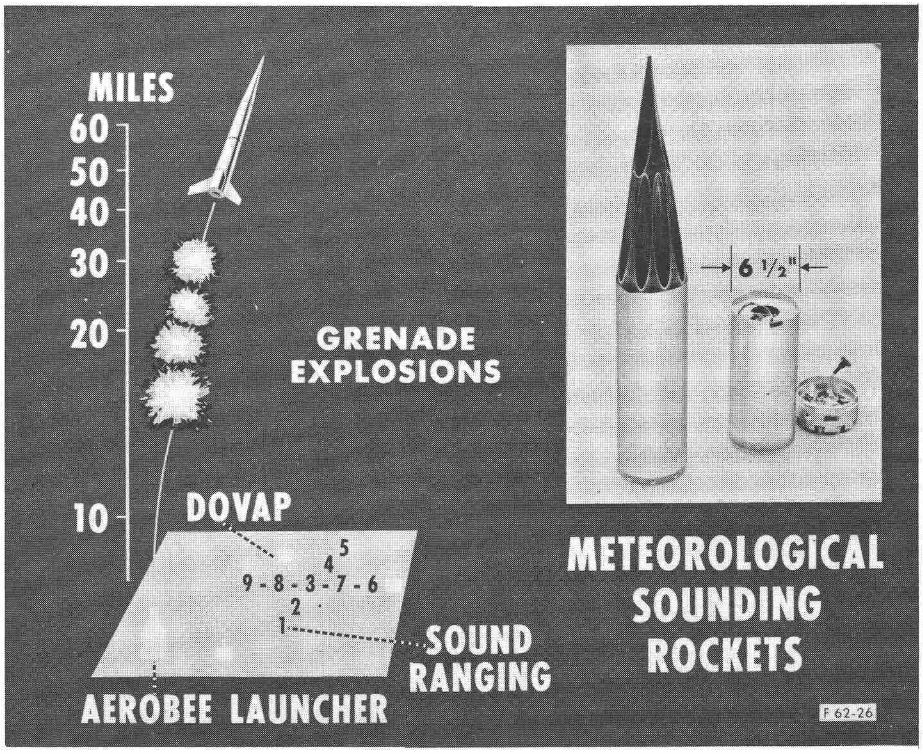

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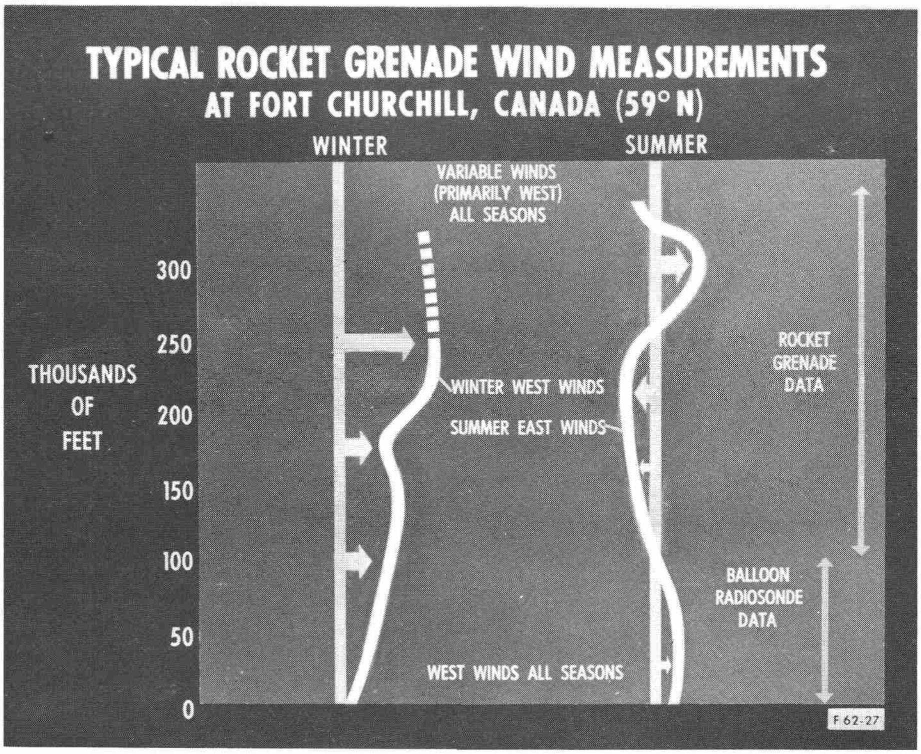

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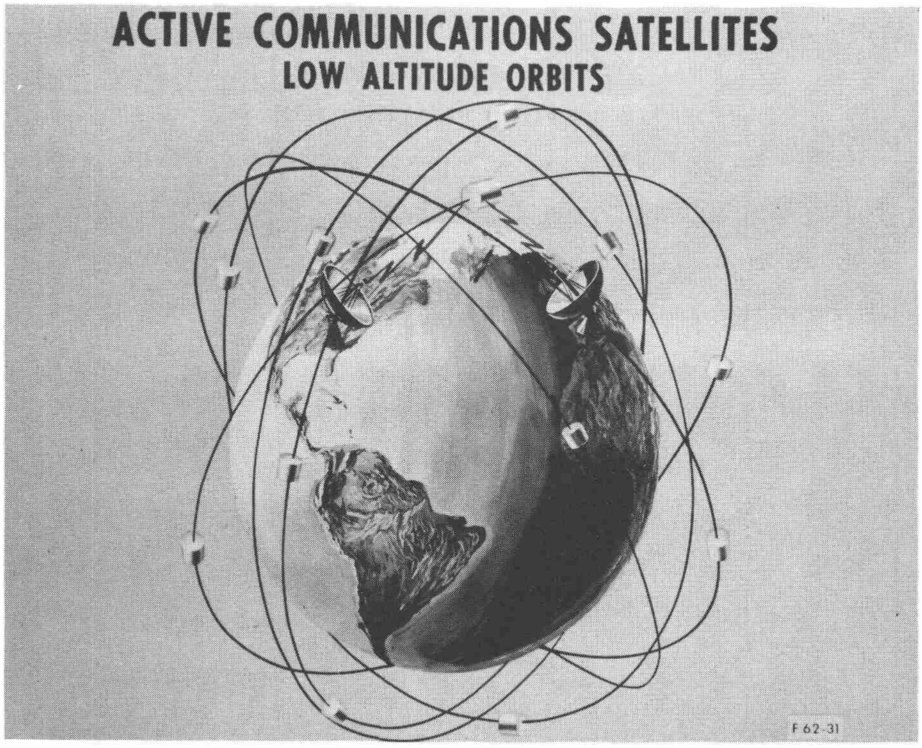

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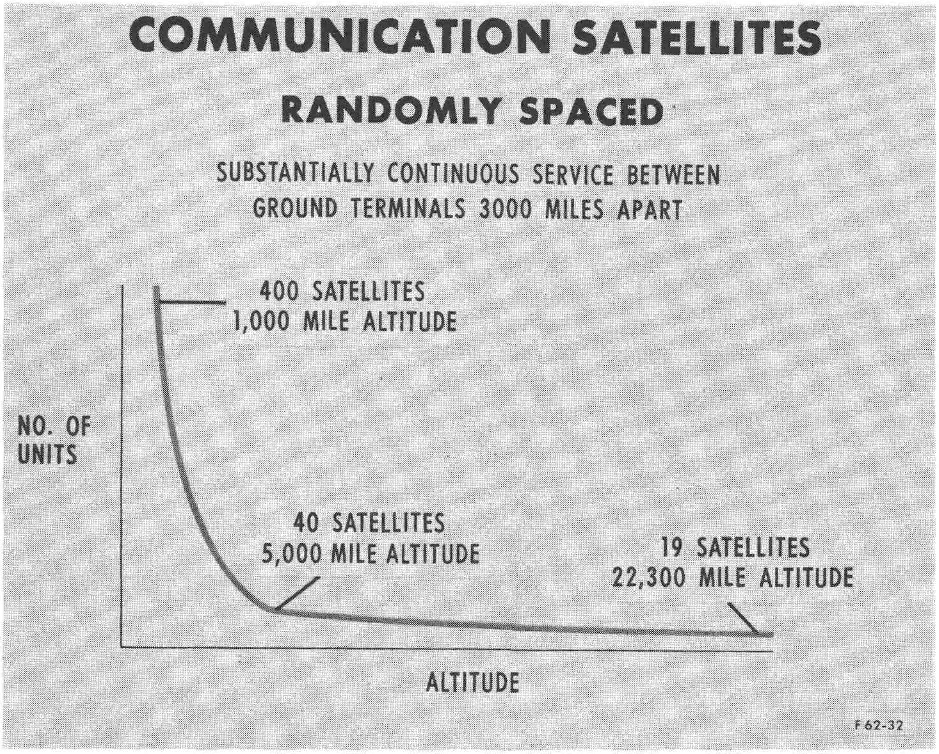


COMMUNICATION SATELLITES

RANDOMLY SPACED

SUBSTANTIALLY CONTINUOUS SERVICE BETWEEN
GROUND TERMINALS 3000 MILES APART

400 SATELLITES
1,000 MILE ALTITUDE

NO. OF
UNITS

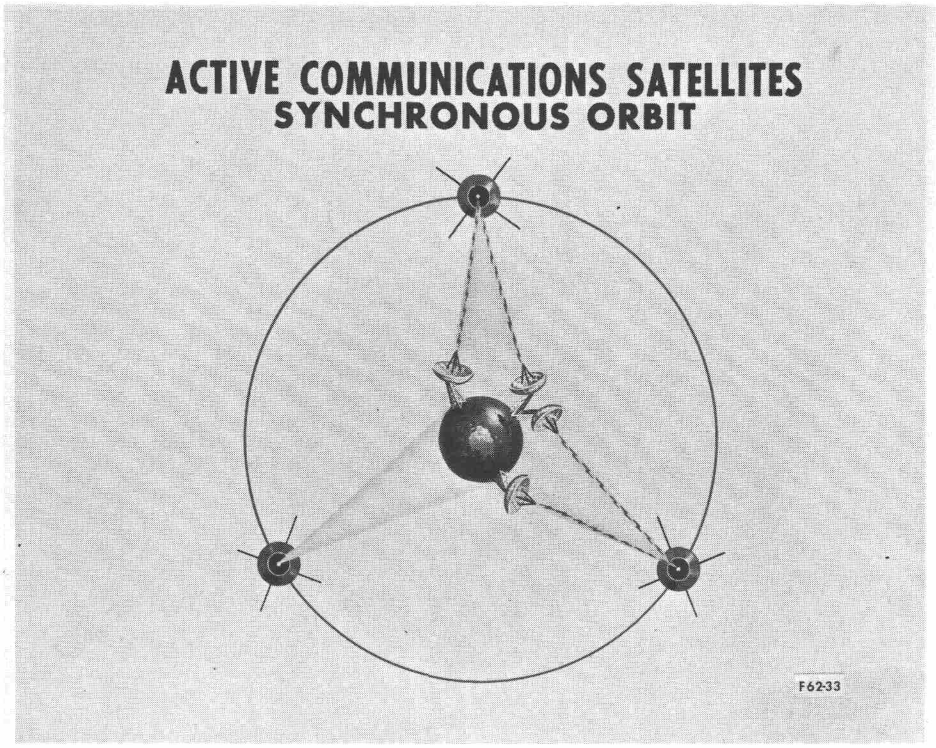
40 SATELLITES
5,000 MILE ALTITUDE

19 SATELLITES
22,300 MILE ALTITUDE

ALTITUDE

F 62-32

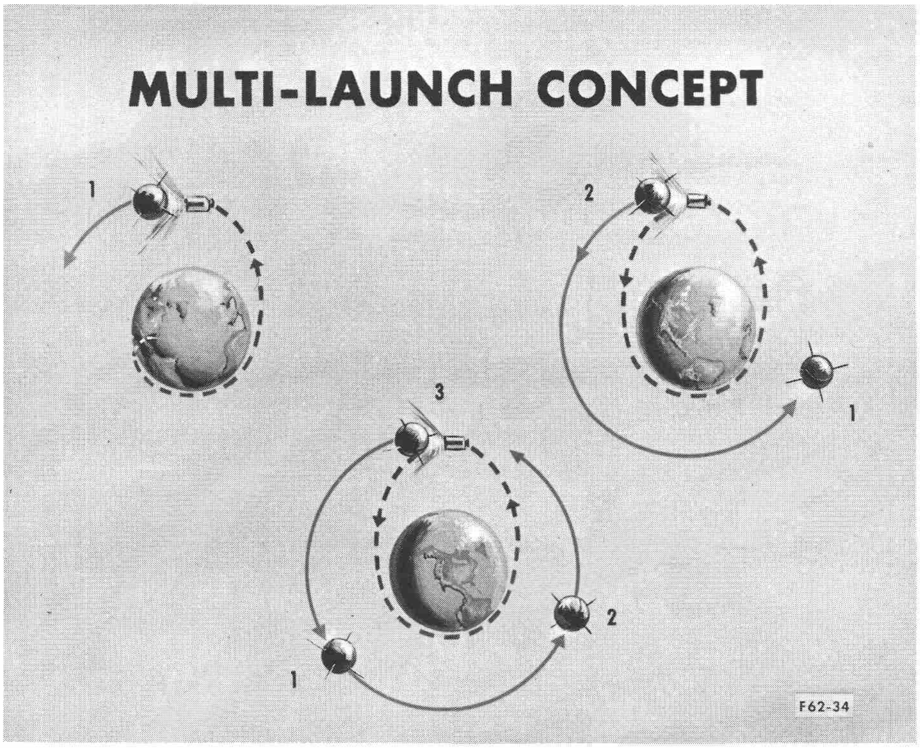
Figure 16



ACTIVE COMMUNICATIONS SATELLITES
SYNCHRONOUS ORBIT

F62-33

Figure 17



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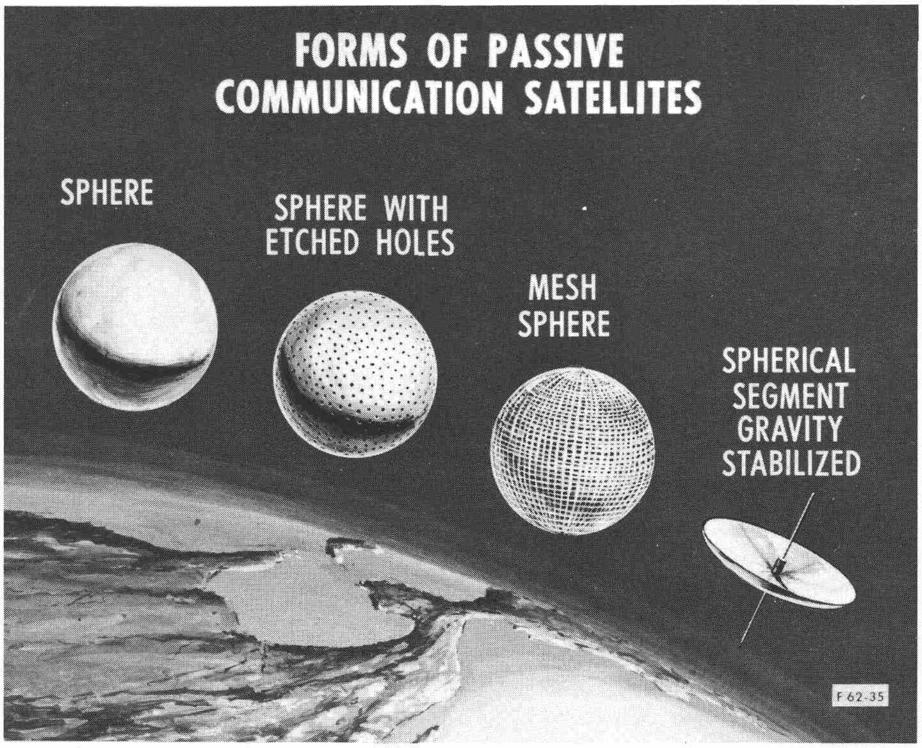

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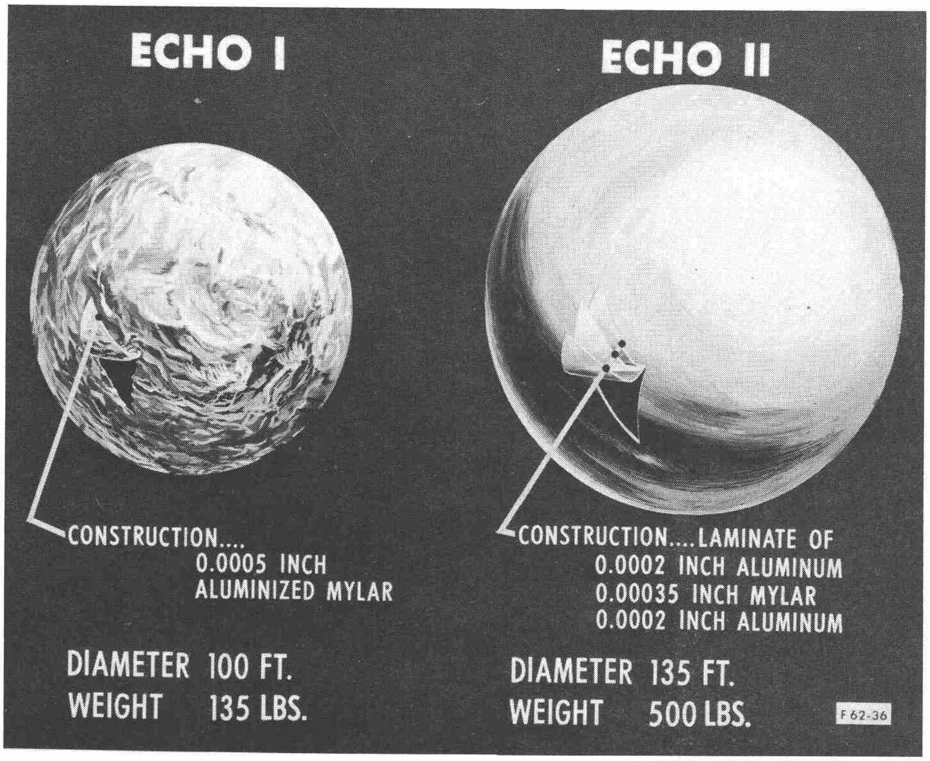

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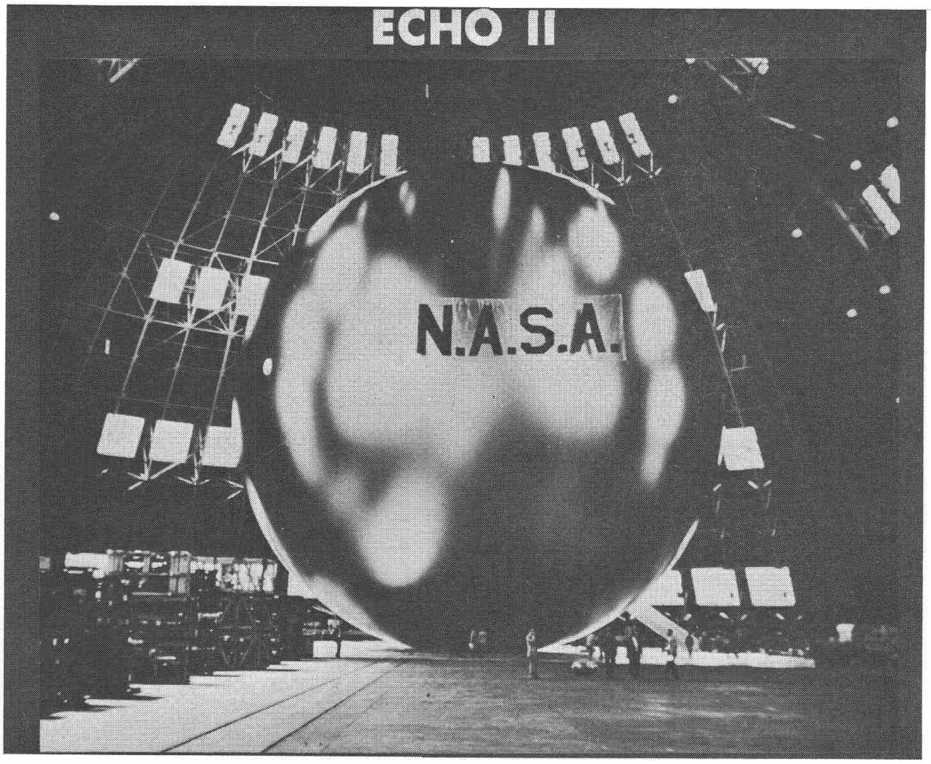

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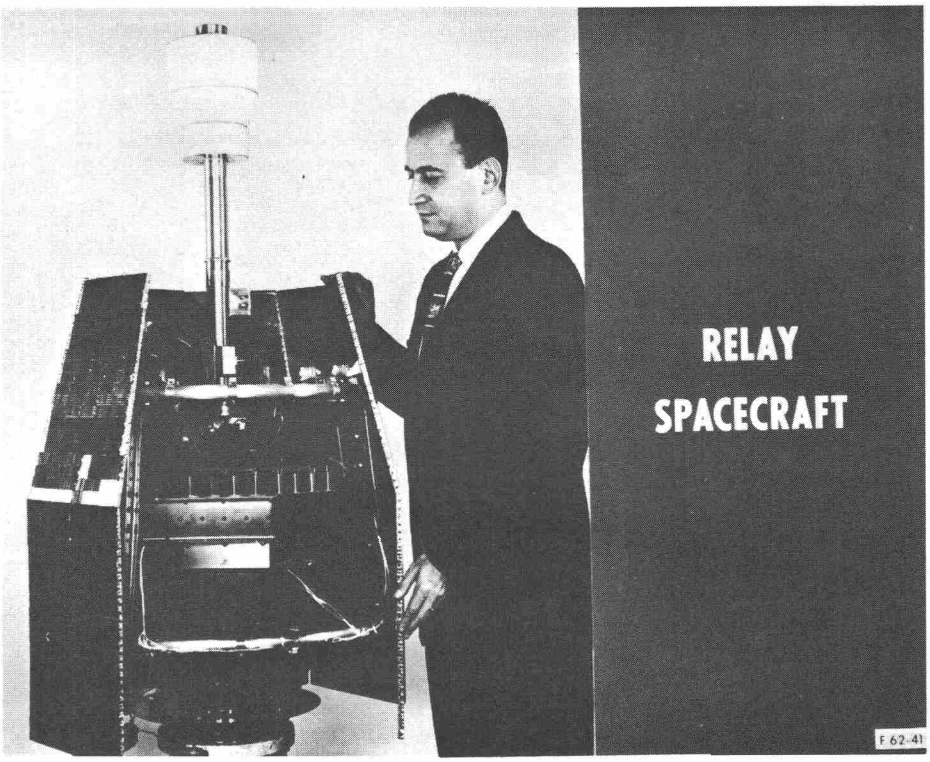

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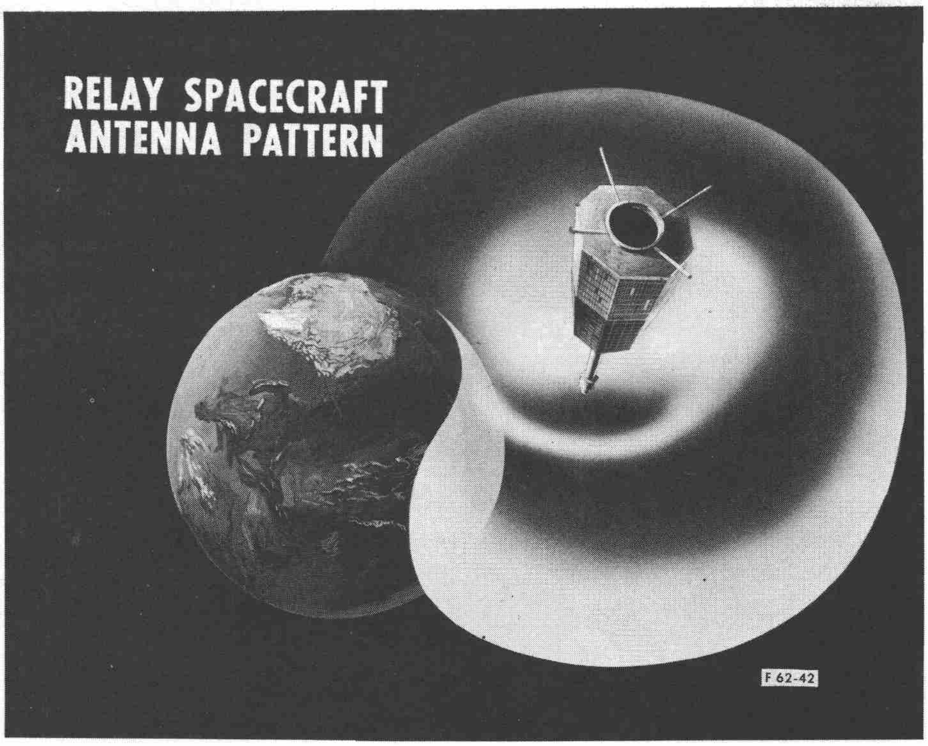

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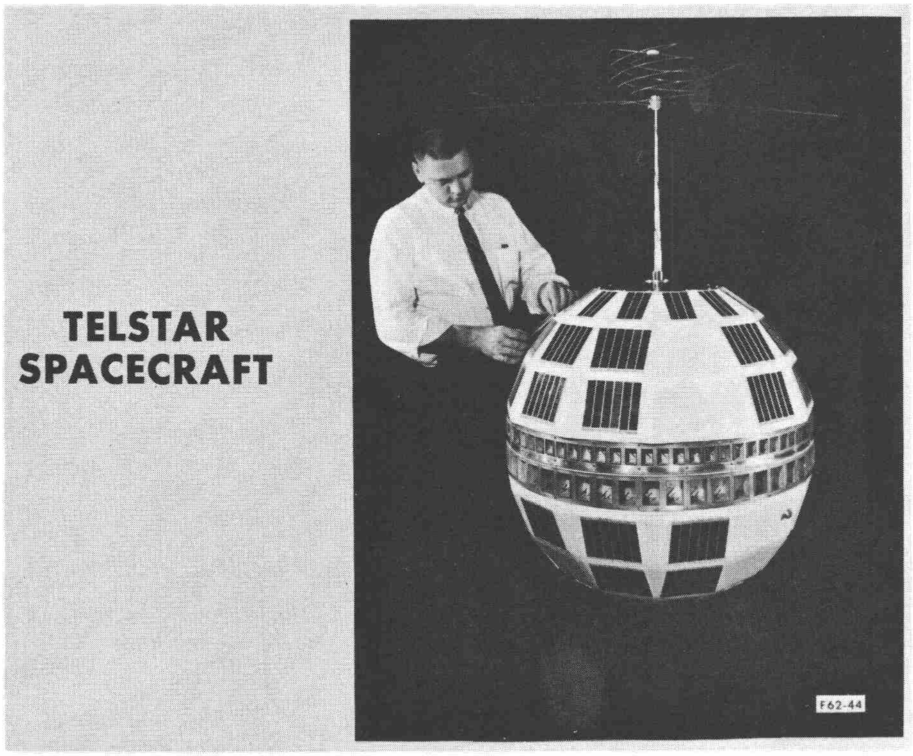

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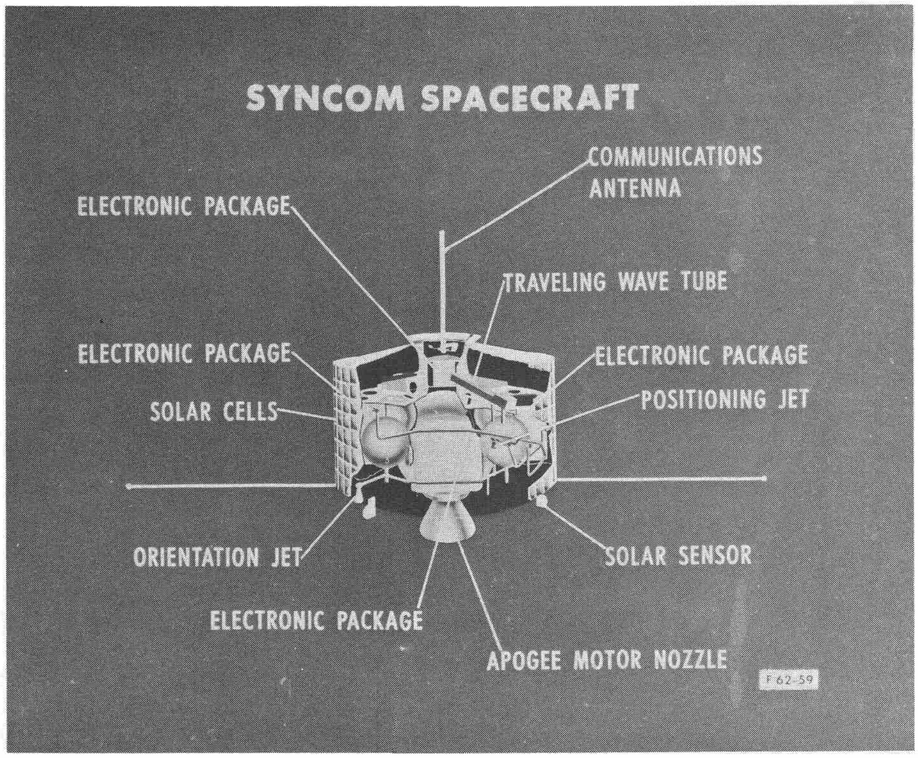

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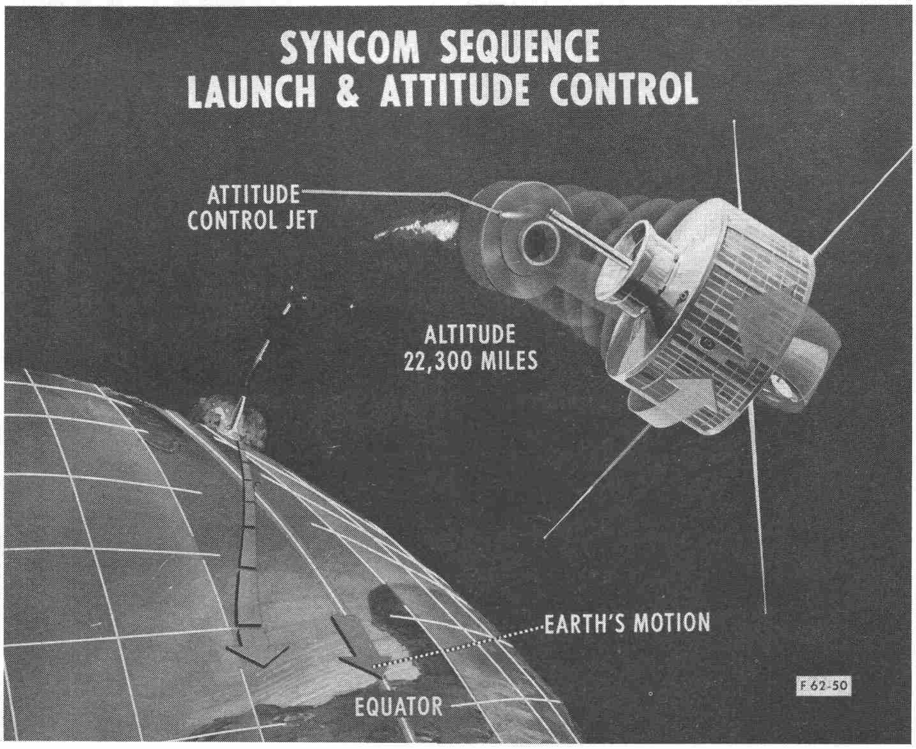

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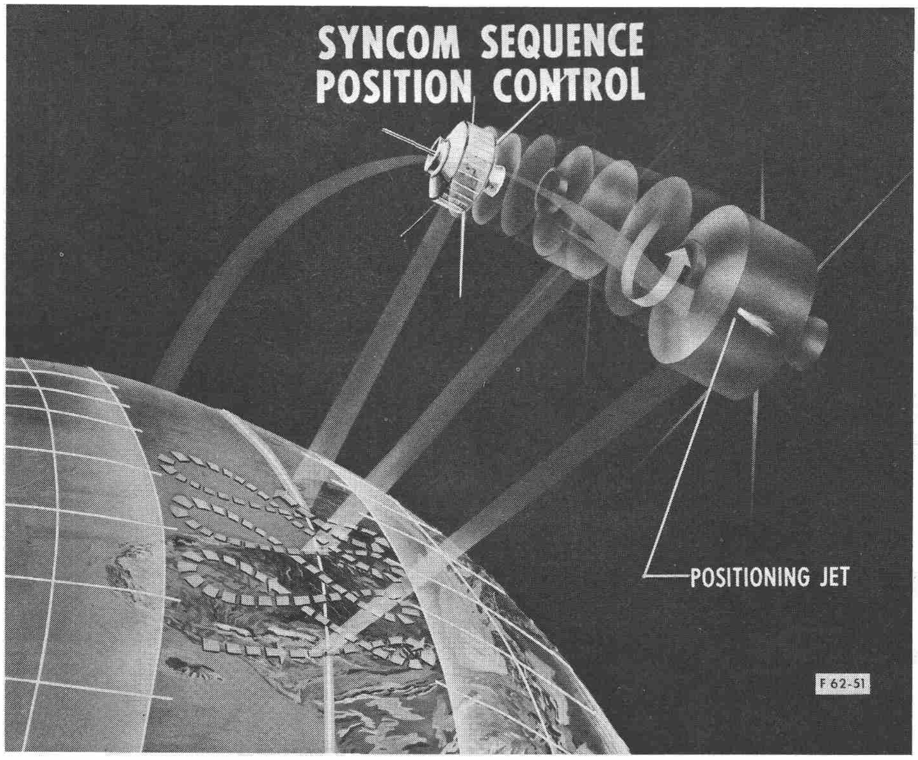

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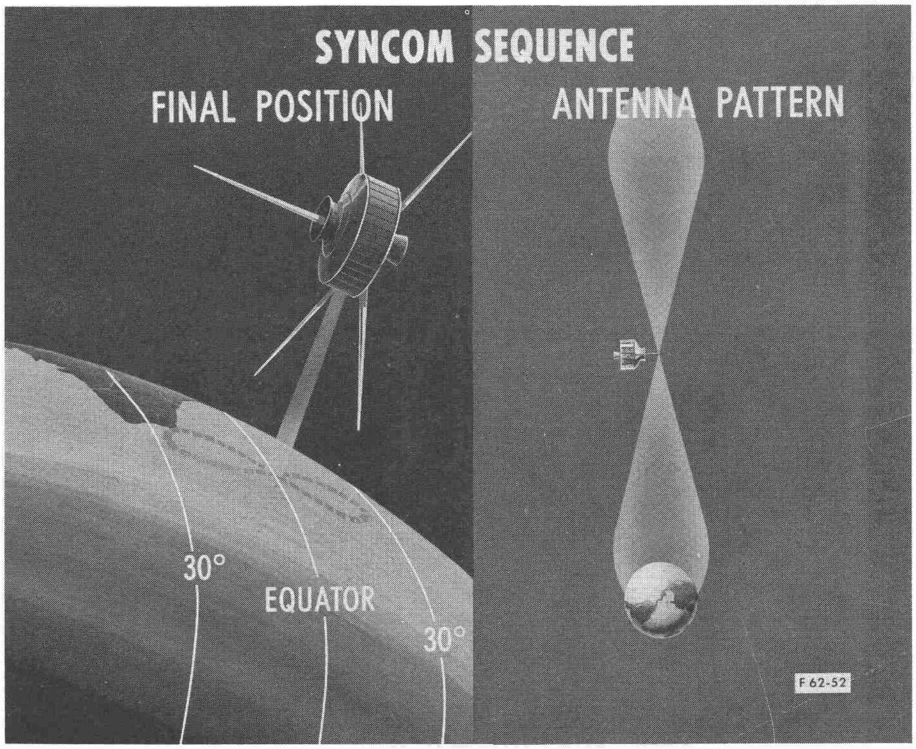
SYNCOM SEQUENCE
FINAL POSITION
ANTENNA PATTERN
30°
EQUATOR
30°
F 62-52

Figure 28

PROGRAM OF THE OFFICE OF ADVANCED RESEARCH AND TECHNOLOGY

By Alfred Gessow

Introduction

The objectives of the programs of the Office of Advanced Research and Technology (OART) for research into and outside the Earth's atmosphere, are broadly stated as follows:

1. To discover new ideas, materials, methods, and devices for space exploration and aeronautics utilization.

2. To develop and define feasibility and limitations of promising ideas.

3. To improve quality and performance of components and systems for space and aeronautical use.

Unlike the mission of the Office of Space Sciences, we are not trying to do research in the environment of space, but, conversely, we are trying to determine the effect of the environment on space vehicles.

Our mission allows us to have a broad range of interest, covering the spectrum of support of basic research to discover and develop new ideas in almost all the physical sciences at one end to the development of advanced flight hardware which are in direct support of NASA's advanced missions at the other end.

Organizationally, OART is composed of a nuclear systems and electric propulsion group, a chemical propulsion and space generation group, an electronics and control group, a space-vehicles group, an aeronautics group, a human-factors group, and, finally, a basic research group concerned with fluid and electrophysics, materials, and applied mathematics. Aspects of the entire program which would be of most interest to the space sciences are: fluid physics, materials, electronics and control, vehicle environment, and space power generation.

The fluid physics and materials areas are oriented towards basic research, whereas the electronics and control, vehicle environment, and space power generation lean more to technology. In many cases, however, the distinction is fine and sometimes nonexistent. The work in these areas ranges from hundreds of small-size investigations carried on by one or two research professionals to costly development and flight studies involving many men and a great deal of money. The work itself is carried on within the various NASA Centers and also under contract with universities and industry.

Fluid Physics Program

High-Temperature Gases

An important part of the program deals with the properties of fluids and, in particular, the properties of high temperature gases. Examples of work being supported in gas-gas and gas-surface interactions are:

1. Atomic and molecular properties of gases:
    - Collision cross sections
    - Radiation transition probabilities
    - Radiation spectra
    - Reaction rates

2. Bulk properties of gases:
    - Thermodynamic properties
    - Viscosity
    - Thermal conductivity
    - Diffusion
    - Electrical conductivity

3. Gas-surface interactions:
    - Sputtering phenomena
    - Accommodation coefficients
    - Catalytic effects

Collision Cross Sections. Collision cross sections between ions and molecules are being measured in the energy range from 100 to 8,000 electron-volts. N_2+ and $N+$ are used in combination with the primary constituents of the Earth's atmosphere. Also, several atomic and molecular beam studies with hydrogen, nitrogen, and oxygen are in being and are planned to study ionization phenomena in stopping gases of the same chemical species.

Radiation Transition Probabilities. Approximate theoretical methods of evaluating radiation transition probabilities for light atoms are being studied. Also being studied are line-absorption profiles for the experimental determination of transition probabilities. At present, measurements are confined to the region of the visible spectrum. It is planned to extend this work to the ultraviolet spectrum for atoms and to include both visible and infrared molecular spectra.

Radiation Spectra. Radiation spectra of air and constituents of other planetary atmospheres are desired up to $60,000^{o}K$. In addition to a small amount of theoretical work, shock-tube programs, as well as ballistic range measurements of gas-cap radiation from the nose of ballistic models, are being conducted. There is also a flight program underway to measure radiation in front of reentry bodies at speeds up to escape velocity.

Reaction Rates. Reaction rates for dissociation, ionization, and combustion are studied theoretically and experimentally in shock tubes. These reaction rates will be used to predict and describe the nonequilibrium processes that occur in the upper atmosphere, in the shock-heated gases about space vehicles entering the atmosphere, and in propulsion motors.

Bulk Properties of Gases. The thermodynamic properties of gases in equilibrium may be determined accurately to very high temperatures from theory. Reliable estimates of transport coefficients can also be made once accurate tabulations of all the important collision cross sections become available. In the interim, transport coefficients of gases are deduced from measurements of bulk gas properties in the shock tube. Future work will include calculations of thermodynamic and transport properties of gases which are believed major constituents of some of the planetary atmospheres.

Sputtering Phenomena. By using an ion accelerator, it has been possible to obtain reliable quantitative data on the sputtering process. The effects of angle of incidence, target temperature, and surface films caused by the neutral background gas have been investigated. Future plans include investigation of additional target materials and the measurement of sputtering at lower energies and with neutralized beams of particles.

Accommodation Coefficients. The ratio of momentum transferred to a surface by a bombarding molecule is related to the temperature of the surface and the extent to which the energy of the molecule comes to equilibrium with the solid before reflection. The accommodation coefficient is a measure of the degree of approach to this equilibrium. It can be determined by measuring the angular scattering distribution function of charged and neutralized beams after reflection from a target surface. Several programs of this type are underway.

Catalytic Effects. Steady-state heat-transfer studies of various coatings are being considered to determine surface effects on recombination processes. The use of catalytic surfaces could be effective in greatly reducing heat transfer if the materials problem could be solved.

Hypervelocity Studies

Some examples of hypervelocity studies which are being conducted in the fluids area are listed as follows:

1. Reentry heat transfer
2. Meteoritic heat transfer
3. Aerodynamics of tektites
4. Satellite drag

Reentry Heat Transfer. Convection and radiation heat-transfer studies on reentry-type bodies are being conducted both theoretically and in a large number of advanced ground facilities and also by means of flight tests with rocket-boosted models. Speeds up to and beyond escape velocities have been obtained on the ground, with goals of 50,000 fps in mind for gases involving oxygen, nitrogen, and other mixtures.

Meteoritic Heat Transfer. It has been shown that observations of tracking data and the luminous intensity of large meteors can yield heat-transfer information for ablating bodies at velocities greater than can now be obtained from ground facilities. Analyses of large fireballs now being made will yield flight heat-transfer data from 23,000 to 80,000 fps.

Aerodynamics of Tektites. Experiments with natural and synthetic tektite glass have been made in arc jets. In this way aerodynamic heating rates similar to those encountered during a hypervelocity entry in the Earth's atmosphere were produced in a laboratory. Study of the ablated laboratory shapes, as well as of large numbers of natural tektites, using aerodynamic theory, gives an indication of the direction and velocity of entry.

Satellite Drag. Theoretical studies of the electrostatic and electromagnetic drag forces on spherical bodies are being carried out, as well as an experimental investigation in a plasma-beam facility.

Plasmas

The dynamics of plasmas, encompassing conventional fluid mechanics, atomic and molecular physics, and electromagnetic field theory, is of interest to NASA because of its relation to reentry phenomena, to an understanding of the Earth, the magnetosphere, and to various technological possibilities, such as magnetohydrodynamics propulsion and power generating devices, hypervelocity simulators, control of hypervelocity flows, and to space communications. The plasma-physics program coverage is listed as follows:

1. Plasma generation
2. Waves and oscillations
3. Plasma acceleration
4. Diagnostics
5. MHD flows
6. Solar physics

Plasma Generation and Properties. Basic to the study and utilization of all laboratory plasmas are methods for the production of plasmas. Various methods are being studied including induction heating, arc discharges, electron bombardment of neutrals, and the heating of initially ionized plasmas by ion-cyclotron resonance techniques.

Waves and Oscillations. Projects in this category include studies of dispersion and damping of oscillations in a thermal plasma, electromotive force wave propagation in magnetoplasmas, electron diffusion in a turbulent plasma, and studies of one-dimensional surface waves with liquid mercury and sodium-potassium mixtures.

Plasma Acceleration. A sizable effort is being made in plasma acceleration because such studies provide fundamental knowledge in plasma areas not yet well explored and because of the promising technological applications. Studies are being conducted on various steady-state and pulsed devices, including coaxial configurations, traveling wave accelerators, and rectangular crossed-field geometries.

Diagnostics. A prime difficulty in the plasma physics and MHD programs is that of making meaningful quantitative measurements of the plasma. Coincident with the research and development effort, therefore, is the development and use of electrostatic and electromagnetic probes, radiofrequency and microwave spectroscopy techniques, and so forth to measure macroscopic and microscopic characteristics of the plasma.

MHD Flows. Two factors have recently emerged which make flight applications of external MHD flows of renewed interest: (1) The consideration of speeds in the escape velocity range, meaning increased ionization and conductivity of the gas flowing around a vehicle, and (2) the advent of high-field superconducting magnets, with lower weights and lack of the heat generation problem. Investigations are planned to assess the use of magnetic fields to decelerate, control, and modify the heating of lunar reentry vehicles.

Solar Physics. Theoretical calculations of the interaction between solar wind and the geomagnetic field are being made and experiments to simulate the phenomenon by shooting an ion beam into a magnetic field are planned. Also, a group is working on a theory of statistical patterns of solar flare events by correlating radiometer and magnetometer data. (Object is to use the demonstrated correlation so that long-observed magnetometer data can be used in solar-flare predictions.)

Materials Program

Materials research extends from the study of pure physical principles to applications in the space program. As in many other aspects of interplanetary travel and reentry problems, the main concern is with extreme environments of low pressures and temperatures, with radiation damage from atomic particles, as well as with structural damage from micrometeoroids. (See Fig. 1.) The materials program in OART can be illustrated with some examples.

Effect of Vacuum on a Light Metal. OART is supporting a number of investigations to determine the effect of ultrahigh vacuum on material properties. An example of one of the tests is shown in Figure 2 which shows the result of a 2-hour exposure of a magnesium sample to a pressure of approximately 10^{-6} mm of Hg at moderate temperatures. Note the marked pitting produced by the vacuum in the magnesium sample. Magnesium was chosen to illustrate the effect, since it has the worst vapor pressure of any of the metals.

Radiation Damage to Electronic Materials. Research on damage to electronic materials resulting from onboard nuclear-reactor radiation, as well as from external radiation from the space environment, has been in progress and it is proposed to increase the effort in these areas. Figure 3 shows, for example, that laboratory simulation of radiation bombardment similar to that which occurs in the inner Van Allen belt drops the output of a typical semiconductor by 50 percent after a 10-day period. A comprehensive investigation has been initiated to check present electronic materials for radiation damage and to determine what new materials or techniques are required to produce radiation-resistant electronic items. Complete electronic components will also be checked for radiation damage in ground simulators.

Erosion by Atomic Particles. Space-particle damage is not limited to radiation effects or structural damage. The collision of atomic particles with satellite material can alter surface characteristics—such as emissivity and absorbtivity—to the extent of seriously affecting the heat balance of the spacecraft. These erosion effects are illustrated in Figure 4 which shows the result of subjecting an aluminum sample to metallic ions for a few hours. Flight experiments are underway to subject similar samples to the actual space environment.

Meteoroid Impact. The impact of micrometeoroids colliding with spacecraft presents another materials problem. The current ground research program is aimed at studying the effectiveness of various energy-absorbing methods as well as at understanding the mechanism of penetration of solids at extreme speeds. The program involves investigations in the firing ranges of NASA laboratories, using helium and hydrogen guns. Figure 5 shows the crater developed in an infinitely thick copper target by the impact of a steel pellet traveling at 26,000 fps. Unlike the situation at lower speeds, the depth of penetration and the diameter of the crater produced are not proportional to the projectile speed and diameter. Current theories do not satisfactorily explain such results. Facilities with higher speed capability and the ability to fire projectiles of smaller size are being proposed.

Melting Points of Some Refractory Materials. An important aspect of the materials program for space application deals with investigations to produce materials which can withstand high stress at high temperatures. For this purpose, it is necessary to start with materials having high boiling points. The melting points of some refractory materials having boiling points above 3,500° F are shown in Table I. Note the absence of the more common structural materials such as steel, aluminum, and copper. Instead, metals such as tungsten, tantalum, molybdenum, and niobium are the object of research studies, as well as alloys involving oxides, carbides, borides, and nitrides.

Tungsten-Alloy Research. An example of research on refractory materials is shown in Figure 6, which illustrates the progress that has been made in increasing the high-temperature strength of tungsten during the last few years by alloying it with tantalum. At 3,000° F, for example, the alloying process increased the tensile strength of pure tungsten from approximately 15,000 psi to about 55,000 psi. At the higher temperatures the gains are significant, although somewhat less spectacular. The dashed lines indicate what gains might be hopefully expected by 1966.

Electronics and Control Program

The Electronics and Control part of OART supports a wide variety of research and development projects of interest to the space science program. These projects fall under the general categories of guidance and navigation, control and stabilization, communications and tracking, and instrumentation and data processing. The program can be illustrated with a few examples.

Attitude Control Research. Whenever it is desired to change the flightpath of a vehicle, or to use it as a base for making measurements, it is necessary that the orientation of the vehicle be controlled with an accuracy commensurate with the precision implicit in the task. There are three general classes of devices which are under active investigation in our efforts to achieve satisfactory solution to the orientation problems imposed by a wide variety of vehicle requirements. These are illustrated in Figure 7.

Mass expulsion devices are ideally suited to applications where accuracy requirements are not extreme, vehicle motion frequent, nor flight times excessive.

Inertia wheels, spheres, fluid fly wheels, and gyros provide means for vehicle orientation with high precision. Research in this area is associated with the

TABLE I

Refractory material	Melting point, °F
C	[a]6,600
W	6,120
Ta	5,430
Mo	4,710
Nb	4,530
ThO_2	5,970
ZrO_2	4,920
Cr_2O_3	4,410
4 TaC:1 ZrC	7,110
HfC	7,030
TaC	7,020
ZrC	6,380
NbC	6,330
TiC	5,710
WC	5,190
W_2C	5,170
VC	5,120
Mo_2C	4,870
[a]SiC	4,710
HfB_2	5,880
ZrB_2	5,500
TaB_2	5,430
TiB_2	5,400
NbB_2	5,250
WB	5,180
ThB_4	4,530
HfN	5,990
TaN	5,600
ZrN	5,400
TiN	5,310

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[a] Sublimes.

development of electrostatically supported reaction spheres, optimization of gyro stabilizers, and the investigation of systems combining inertia devices with mass expulsion techniques.

There has been limited use to date of forces inherent in the space environment for vehicle orientation control. Research indicates that the Earth's magnetic field can be used for attitude control of orbital vehicles, that solar pressure can be used for orienting deep space vehicles, and that gravity-gradient techniques have potential applicability to many planetary orbital vehicles.

Horizon Scanner Sensors. Horizon scanners of limited performance are used in a number of current space programs, and devices of higher performance are required for many advanced space missions. These devices are used for determining the vertical direction from the spacecraft to the center of the reference body which, depending on the particular mission, may be the Earth, Moon, or another planet.

As shown in Figure 8 by detecting the Earth-space boundary at opposite horizons, the angles between each sensor and the spacecraft can be determined and signals can be furnished to the spacecraft to adjust its attitude to make the angles equal, thereby maintaining the spacecraft perpendicular to the center of the Earth.

A major problem in designing accurate horizon scanners is to obtain detailed information on the actual characteristics of the horizon as seen by various types of sensing devices. Towards this end, a flight research program is being initiated using various horizon sensor-filter combinations onboard rocket vehicles at altitudes of up to 1,500 miles and onboard the X-15 airplane within its altitude capability. The sensors to be investigated cover the infrared, ultraviolet, and visible spectral regions.

Ground Antenna Technology. A major problem in communications with spacecraft at very great distances from the Earth is to develop new antenna systems with extremely large effective apertures.

The current approach involves the building of very large diameter antennas which are difficult to engineer and are quite costly. Research is being undertaken to determine the feasibility of obtaining large effective antenna apertures by arraying a number of independently steerable smaller antennas in which the output signal would be the coherent summation of the outputs from the entire group to produce a result equivalent to that of a large antenna, as illustrated in Figure 9.

Advantages of the array include (1) reduction in cost, (2) greater tracking flexibility, and (3) greater reliability and versatility.

Problems involved in building an effective array include complexity of the electronic system and associated computing and data handling, and development of suitable new components for the electronic systems.

Space Erectable Antennas. A program is underway to study various types of antennas which could be efficiently erected on a space vehicle for data transmission over large distances or when high data rates are necessary. As illustrated in Figure 10, research in antenna design has resulted in the development of arrays which are erected by the expansion of compressed foam after launch and injection. A series of tolerance studies on a four-element disk array has indicated that many of the dimensional parameters are not critical and that for the most critical dimensions the tolerances are of the same order as those required from parabolic dishes of equivalent gain.

Work on helix arrays is going on using full-scale models, and work is also being done on erectable parabolas using techniques developed for spin casting plastic erectable paraboloids. Extensions of this work to erectable horn structures, feeds, and electronic beam aiming techniques are planned.

Rocket Effects on Electromagnetics. Attenuation of radio and radar signals caused by transmission through rocket exhaust gases and propellant compositions has been of much concern in past and present programs. The ability of guidance and navigation electromagnetic devices to perform properly in situations wherein such attenuations occur is subject to question. Prior knowledge of these effects is not only desirable but may well be required to optimize designs and parameter selections to minimize such effects.

In respect to lunar landing devices such as a radar altimeter or Doppler navigator and, to a lesser degree, communications during lunar landing missions, additional discontinuities will exist in reception of altimeter signals from the landing-site surface due to exhaust disturbances to the lunar surface. The electromagnetic devices in this application are required to function in both a rocket plume and a dense dust particle environment. It is planned that lunar surface simulations, in conjunction with rocket environments, be performed to measure the effects of these transmission discontinuities.

Figure 11 depicts a study of this problem for the Surveyor spacecraft which is to be performed in a 41-foot vacuum sphere.

Reentry Communications Blackout (Fig. 12). One of the important current experimental NASA programs is to investigate the interference of the ionized flow field produced by high reentry temperatures with radio transmission, communications, and radar tracking for vehicles reentering the atmosphere at velocities from 10,000 to 45,000 fps and to develop practical methods of eliminating this data blackout phenomenon.

Among the techniques for eliminating or reducing the blackout problem during reentry, several promising approaches are being considered involving the use of higher telemetry frequencies, cooling the air in the vicinity of the antenna to provide a local data transmission window by injecting droplets of liquid, and creating a "window" in the plasma by means of a magnetic field to polarize the plasma. Also various aerodynamic configurations are being considered which tend to minimize the plasma sheath.

Navigation and Guidance Technology. The angular position and rate sensors and linear acceleration sensors currently used for guidance and control of launch vehicles and spacecraft employ conventional gyroscopes and accelerometers. Many advanced concepts exist for sensors having potential for greater reliability and longer life as well as much higher performance in the space environment. These more sensitive devices will have particular application for the low thrust levels over long periods of time produced by the electric propulsion systems being developed for future missions. The more promising techniques for future gyros and accelerometers include the electrostatic and cryogenic gyros and accelerometers depicted in Figure 13. These devices will utilize superconducting magnetic and electrostatic fields to support a rotor in the case of a gyro and a proof mass in the case of an accelerometer. Such concepts are currently being investigated but will require increased effort to achieve anticipated advances in the gyro and accelerometer state-of-the-art.

Vehicle Environment Program

An obviously important part of the work of OART is that dealing with environmental effects and technology. Some of the major problem areas in this regard are listed as follows:

1. High-energy radiation effects and shielding
2. Meteoroids
3. Temperature environment
4. High-vacuum technology
5. Zero-g effects in fluid systems

The big problem in regard to high-energy radiation and meteoroid damage is that of providing adequate protection to the spacecraft and its contents. This problem will be discussed in greater detail subsequently.

Temperature Environment. The space environment characteristics affecting heat balance and temperature distribution of a spacecraft are: solar radiation, albedo, infrared thermal radiation (from Moon and planets), and cold-space background. Work is underway to regulate external and internal spacecraft temperature control by means of coatings and mechanical means, (i.e., by passive and active systems) and by simulating solar radiation in laboratory chambers by using sources which can match closely the Sun's rays in intensity, uniformity, parallelism of rays, and energy distribution.

High-Vacuum Technology. The principal problem in high-vacuum technology involves the further improvement of high-vacuum techniques to obtain lower pressures and of the means to measure these low pressures accurately. It is most desirable that high vacuum be combined with other space environments in test facilities.

Zero-Gravity Effects on Fluid Systems. A fairly comprehensive program is underway to study the fundamental behavior of fluids under zero-gravity conditions. The experimental part of the program is carried out in a ground facility (i.e., with a drop tower giving several seconds of zero gravity) and by flight experiments in aircraft, with rockets in ballistic trajectories, and in satellites, wherein the range of zero gravity goes from approximately 1 minute on up.

High-Energy Space Radiation. The OART program dealing with the problem of the effect of the space radiation environment on space vehicles and their contents will include studies in the following areas along with the development (where necessary) of basic data needed as input into these studies:

1. Radiation data
   a. Collation
   b. Analysis (from space vehicle design viewpoint)

2. Radiation effects
   a. Space vehicle materials (glasses, coatings, etc.)
   b. Sensitive electronic components (solar cells, transistors, etc.)

3. Shielding
    a. Shielding effectiveness of materials (primary radiation secondary production)
    b. Methods for design of optimized shields
    c. Advanced concepts (electromagnetic, electrostatic)

4. Facility development (for use in the above studies)

Radiation data: Radiation data are collected from various sources and analyzed from the standpoint of the space vehicle designer. Analysis of the data covers the determination of the integrated radiation intensities to which vehicles will be subjected. This must include consideration of solar flare probabilities and shielding effect of the magnetic field.

Radiation effects: The chief problem area resulting from high-energy radiation incident on space vehicles is the materials and components exposed on the outside of the vehicle. Structural materials should not present much of a problem over reasonable exposure times. As previously mentioned, semiconductor electronic devices present one of the greatest problems. OART has an experimental program on the study of the effects of high-energy radiation on solar cells.

Shielding: Both theoretical and experimental studies are being carried out on the determination of shielding effectiveness. The largest uncertainty here is in the production of secondaries and the biological effect of the particles (both primary and secondary). It appears that optimum shields will involve laminated materials. Also, the placement of materials needed for other purposes in the vehicle is of great importance. Work in this area is quite dependent upon the answers to the problem of secondary production. Electronic shielding does not look promising at this time. Electromagnetic shielding appears to have some promise if superconductors are used. Here it appears that such techniques may be useful where large volumes are to be shielded from very high energy particles (as possibly in the case of space stations). There are many engineering problems and, consequently, this type of shielding constitutes a long-range problem. The present studies are conceptual in nature.

Facilities: The facilities used for the radiation studies are linear and cyclotron accelerators. Thus far, facilities existing at various universities and laboratories throughout the country have been used for these engineering-type tests.

<u>Meteoroid Penetration Problem</u>. The state-of-the-art knowledge of the meteoroid environment is shown in Figure 14. Presented are the expected penetration rates for different periods of time for the area noted as a function of material thickness based on two estimates. The upper curve indicates the distribution of meteoroids as proposed by Whipple together with the penetration criteria of Summers. The lower curve indicates the distribution of meteoroids as proposed by Watson together with the penetration criteria of Bjork.

The actual penetration rates, as yet to be determined, are expected to lie between the two curves. Superimposed on the chart are the ranges of material thickness exposed to the environment by the S-55 satellite as well as the proposed S-65 satellite. In addition to narrowing substantially the uncertainty that currently exists, these projects will provide definitive information over the material thickness range shown. These data in themselves will not provide all the information required for the efficient design of spacecraft, and other flight experiments need to be programed.

Micrometeoroid Satellite (S-55). The S-55 micrometeroid satellite is shown in Figure 15. The S-55A, launched in the summer of 1961 with a Scout launch vehicle, was in orbit for only 2-1/2 days and recorded no penetrations. A duplicate experiment is the S-55B, planned for launching during late summer 1962, with the main instrumentation again being the single-shot pressure cans, wire cards, and grid detectors.

Three more satellites of this type are proposed, having the same shape and weight and again using the Scout launch vehicle. In the proposed S-55C, the wire cards and grids will be replaced by a new mylar capacitor sensor which can record multiple hits (i.e., it is reusable). The pressure cells would be of stainless steel. In the prepared S-55D and S-55E satellites, the entire exposed usable surface would consist of the capacitor-type sensors, bonded onto different thicknesses of aluminum sheets.

Also proposed and under study is the S-65 satellite, which would use the nylon capacitor sensors in conjunction with varying thicknesses of stainless steel. Thicknesses up to the order of 0.010 inch can be tested, which are an order of magnitude greater than those tested in the S-55. The data obtained from both series of tests would provide a basis for extrapolation to greater thicknesses. The proposed S-65 satellites would also have an order-of-magnitude increase in exposed area over the S-55 series (about 500 square feet compared with 25 square feet) and would require much larger boosters.

Meteoroid Flight Experiments. Also, included in the proposed series of flight experiments is the recoverable probe shown on the left in Figure 16. (The satellite shown on the right is a generalized configuration of the S-65 type just discussed.) The paraglider device will be shot up in a ballistic trajectory to a peak altitude of approximately 700,000 or 800,000 feet with an Aerobee rocket. On the way up it will unfold, proceed into space, and then drop down and glide to Earth. The glider will have a large surface area that is made up of alternate layers of nylon and aluminum acting as a condenser for detecting penetrations. The recoverable feature of the vehicle will permit examination of actual micrometeoroids that become embedded in the material.

Space Power Generation Program

One of the prime requirements of any unmanned or manned space vehicle system is the availability of lightweight, reliable electrical power. In the area of nonpropulsive power, requirements of a few watts to several kilowatts are foreseen, with life requirements ranging from a few minutes for ballistic tests to 5 to 10 years for operational systems such as communications and navigation. To meet this broad spectrum of power levels and other technical requirements associated with them requires the development of a variety of power-system configurations. The types of spacecraft electric power systems being supported in the OART program are listed as follows:

1. Batteries
2. Fuel cells
3. Nuclear
4. Solar mechanical
5. Solar cells
6. Solar thermoelectric
7. Solar thermionic

The energy sources represented in this list are chemical systems in the form of batteries and fuel cells, nuclear systems, and solar systems in which the energy is converted mechanically, as in the Sunflower system, by solar cells, and by direct conversion of solar energy into electricity by thermoelectric and thermionic systems.

Batteries. Work has been supported to develop new and improved battery types, one of which, the silver-cadmium, has been flight tested in the Explorer XII. The possibilities of improving the 20 to 30 watt-hr/lb performance of these batteries are being investigated. Also, work on improving silver-oxide—zinc batteries is continuing.

Fuel Cells. As the durations of the missions are extended or the power levels are increased, electrochemical systems such as fuel cells become more attractive than conventional batteries because of their lighter weight and higher efficiency.

The Space Power Technology Program is supporting the development of a regenerative hydrogen-oxygen fuel cell for possible use with solar cells for energy storage. Improvements in system weight and life are potentially available. In addition, a contract has been let for the development of a flight prototype hydrogen-oxygen fuel-cell system such as may be used for the Apollo spacecraft.

Mechanical Power Conversion. The largest project sponsored by the Space Power Technology Program is the Sunflower 3-kilowatt solar dynamic power system. The Sunflower system will weight approximately 750 pounds and will have a multiple-petal solar concentrator 32 feet in diameter focusing solar energy in a mercury boiler. Mercury vapor will expand through a multistage turbine driving an alternator. The vapor will be condensed and returned to the boiler.

The major problems in this flight prototype development program are perhaps materials problems. Although sometimes referred to as a state-of-the-art system, this is certainly not the case. Such a system involves extensive development and ground testing before it will be ready for flight tests, perhaps in 1965 or 1966.

Solar Cells. The majority of spacecraft to date have been powered by silicon solar cells. Although the success in this area has been encouraging, several important problems exist in this field. Solar cells which must operate in the Van Allen radiation belt and to a large extent beyond these belts are subject to serious damage due to proton and electron radiation. As mentioned previously, the Space Power Technology Program has sponsored contracts for experimental determination of the effects of electron and proton radiation damage to solar cells, and theoretical studies to explain the damage phenomenon as a tool in designing radiation-resistant solar cells. Progress has been made in this area. The next phase of this program will include development of solar cells which show some promise of high radiation-damage resistance.

Solar-cell power systems are also very expensive. One method of reducing solar-panel costs is to make the solar cells more efficient by adding reflective surfaces to the solar panel, arranged so as to concentrate additional solar energy on the cells. A contract has been let aimed at exploiting this technique and at studying thermal problems and advanced panel structures. It is hoped that work in this area will reduce panel weights and thus improve the present designs calling for approximately two pounds per watt.

Solar Thermionic. The NASA program in thermionics related to chemical and solar power systems ranges from support of basic research to the development of a complete flight prototype thermionic power system with an output of approximately 250 watts. No flight program has been planned as yet.

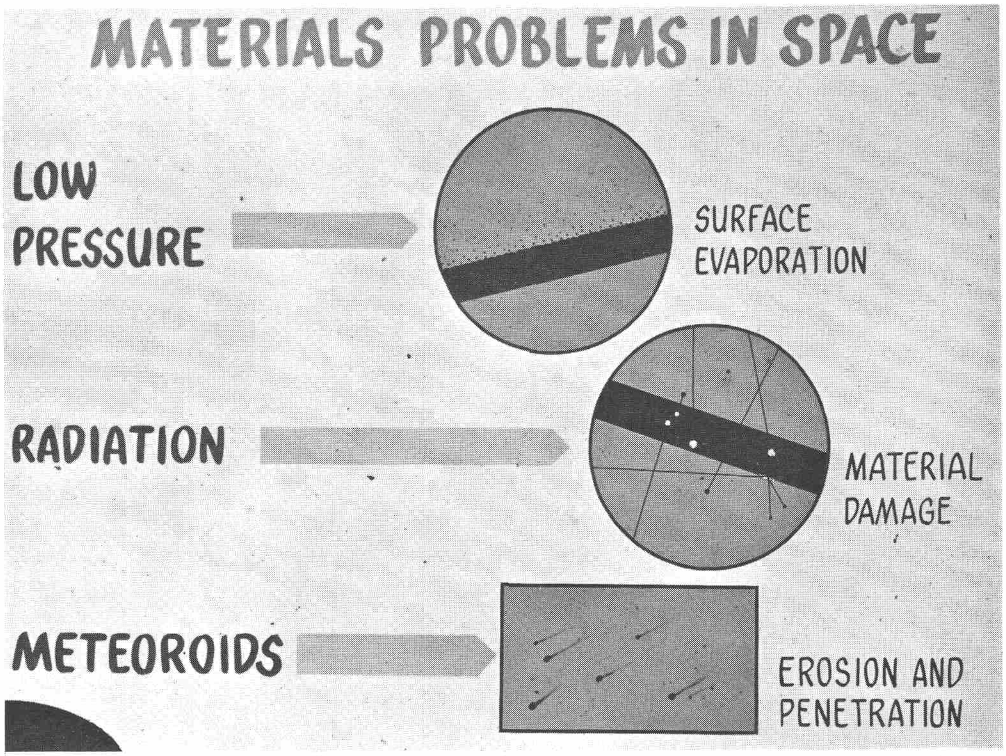
Additional diode development work is planned, including some work on the fabrication problems associated with flight-type hardware.

In technology closely related to thermionic power systems, NASA is supporting a research and development contract for thermal energy storage by heat of fusion of eutectic compounds. It is believed that thermionic conversion may not be applicable to orbiting vehicles unless thermal energy storage is possible to eliminate thermal cycling. It is also anticipated that attitude control requirements may be less severe if thermal energy storage is provided.

NASA is sponsoring extensive work on all-metal mirrors for solar-energy concentration. The next step in this program will be the construction of a 9- to 10-foot mirror master and fabrication of a sample mirror. This would be the largest single-piece mirror stowable in presently available nose cones.

Snap-8 and Electrical Generating System. The major advanced technical development program on nuclear electric power generation is the development of the Snap-8 system. The Snap-8 system is designed to deliver 30 kilowatts of electric power, or with two of these conversion systems operating with a single reactor, it could deliver 60 kilowatts of electric power. The major effort at the present time is the development of the 30-kilowatt system with enough work on the 60-kilowatt version to show that such a system is feasible. Testing has been conducted on all the components of this conversion system. Continued test time and design and development work is, however, still required to provide for reliable components. The first test with the reactor is scheduled for late 1963 but obviously very reliable components and nonnuclear system operation must be established before the system is mated with the reactor. The development of high reliability is undoubtedly the most difficult job in the Snap-8 program.

OART has in-house and general support capability which could be of great value to the Space Sciences Program. Although we are not standing still waiting to be told what our program should be, we are happy to receive suggestions as to ways in which we can be of help to the other program offices in NASA in specific areas of research and development.



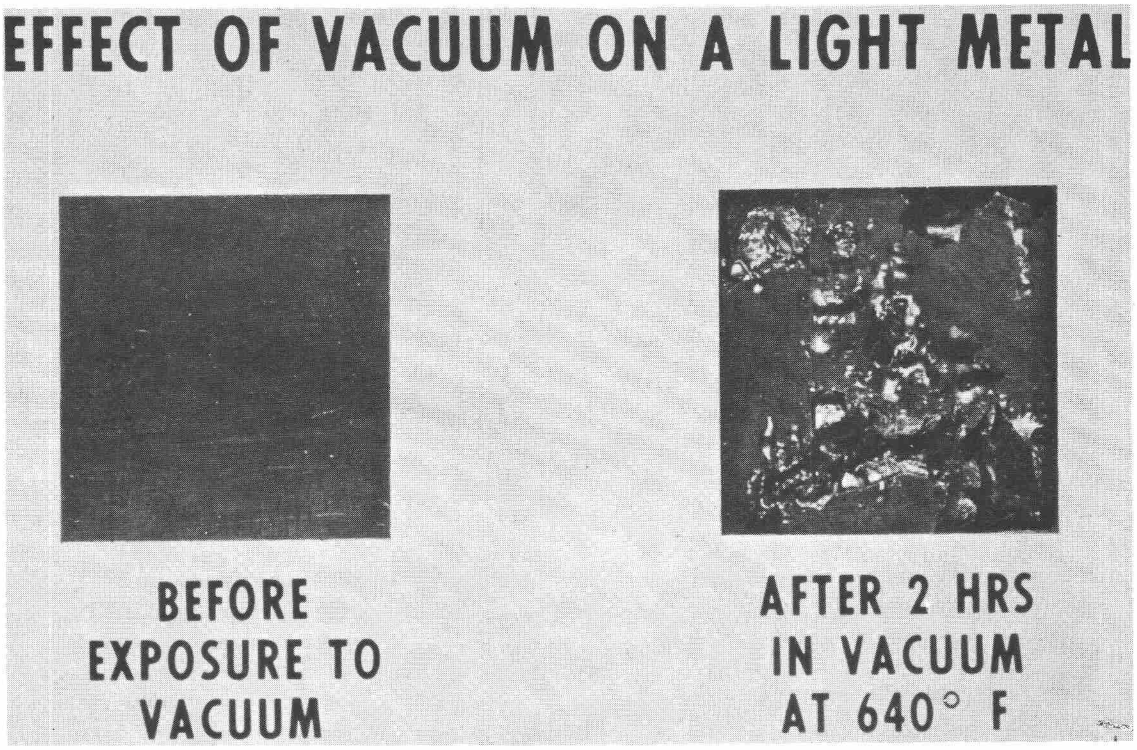
MATERIALS PROBLEMS IN SPACE

LOW PRESSURE → SURFACE EVAPORATION

RADIATION → MATERIAL DAMAGE

METEOROIDS → EROSION AND PENETRATION

Figure 1



EFFECT OF VACUUM ON A LIGHT METAL

BEFORE EXPOSURE TO VACUUM

AFTER 2 HRS IN VACUUM AT 640° F

Figure 2

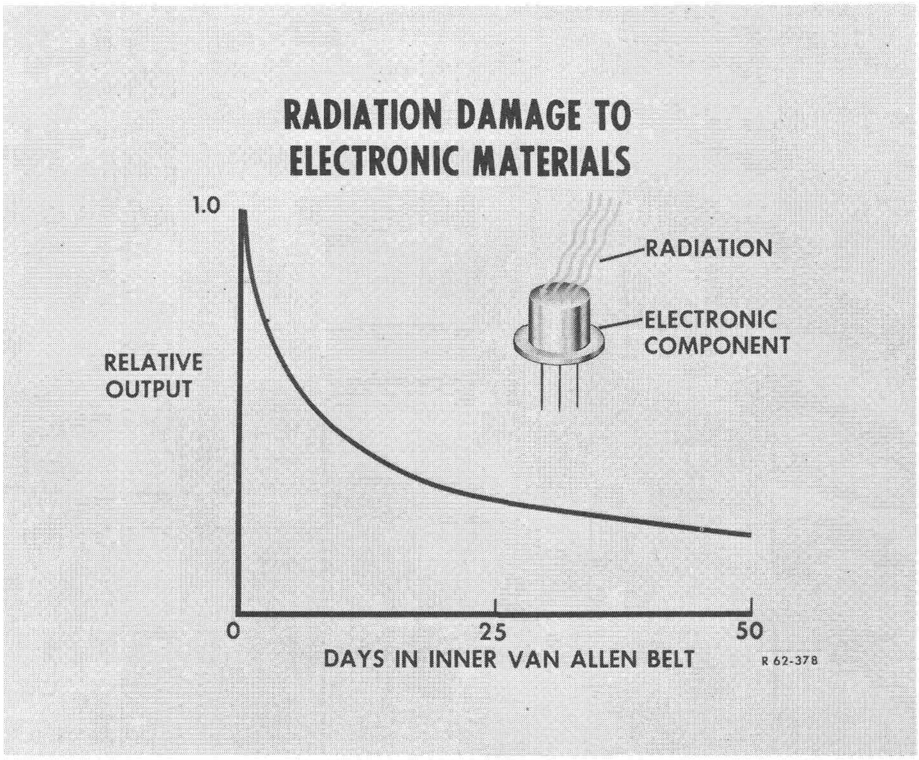


Figure 3

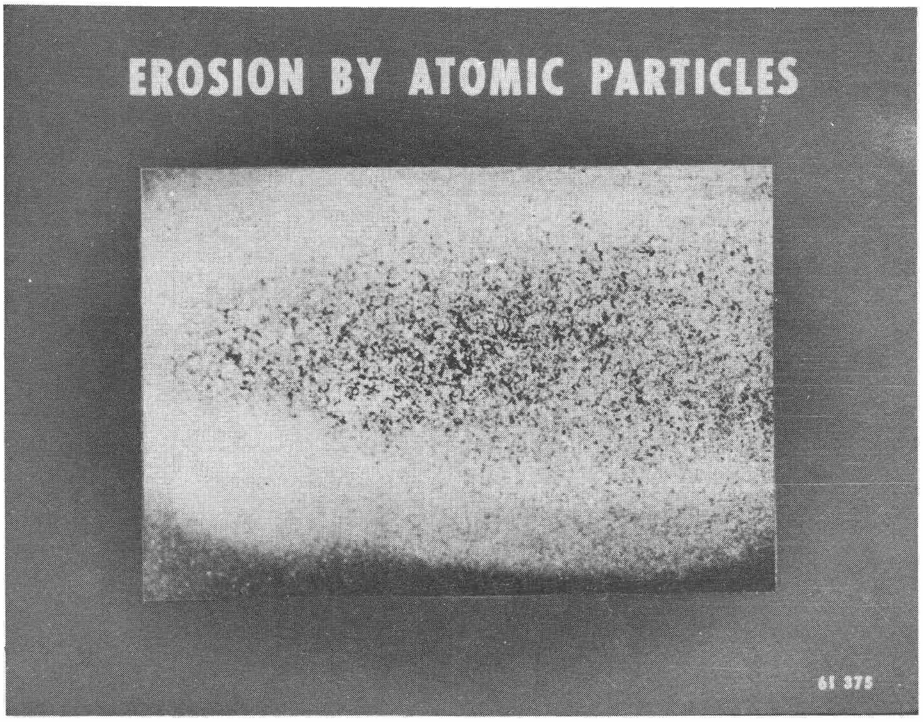


Figure 4

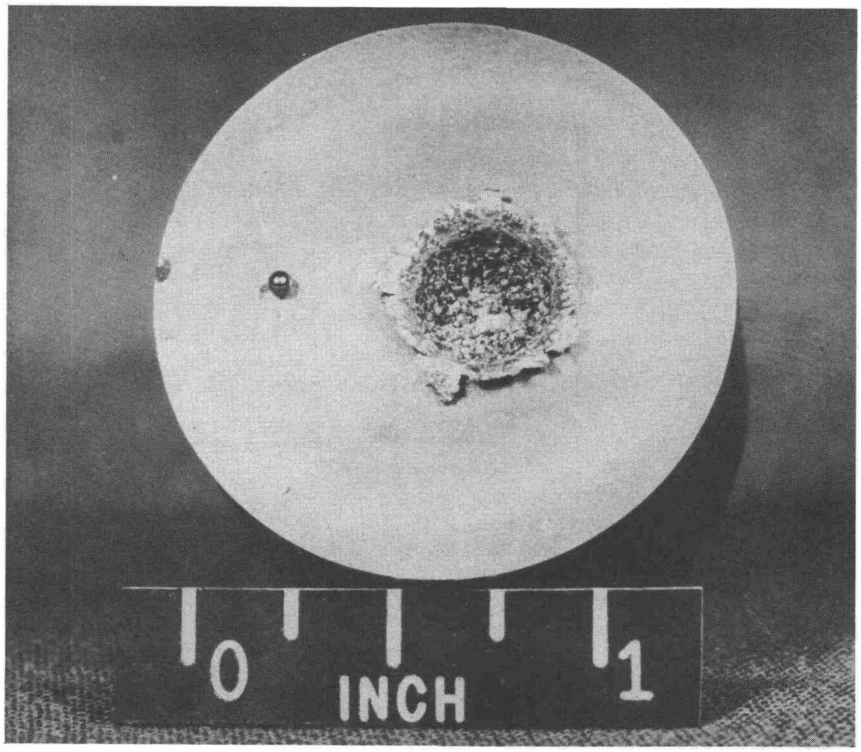


Figure 5

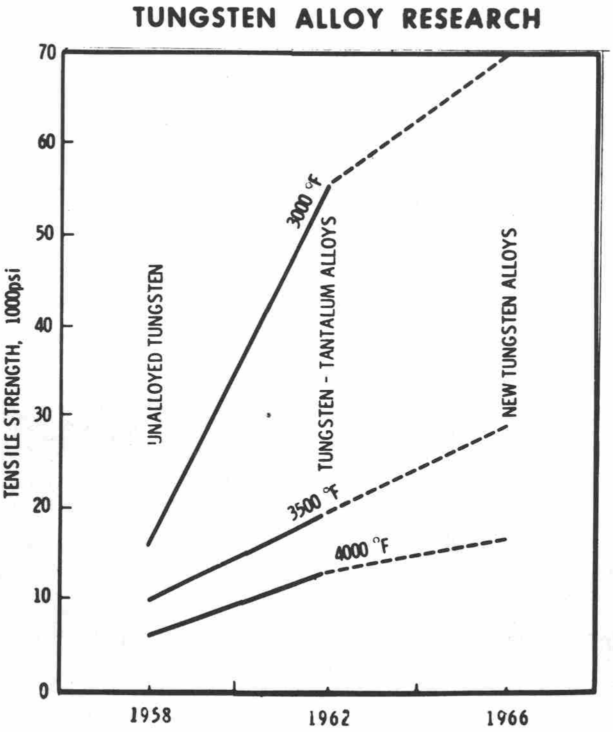


Figure 6

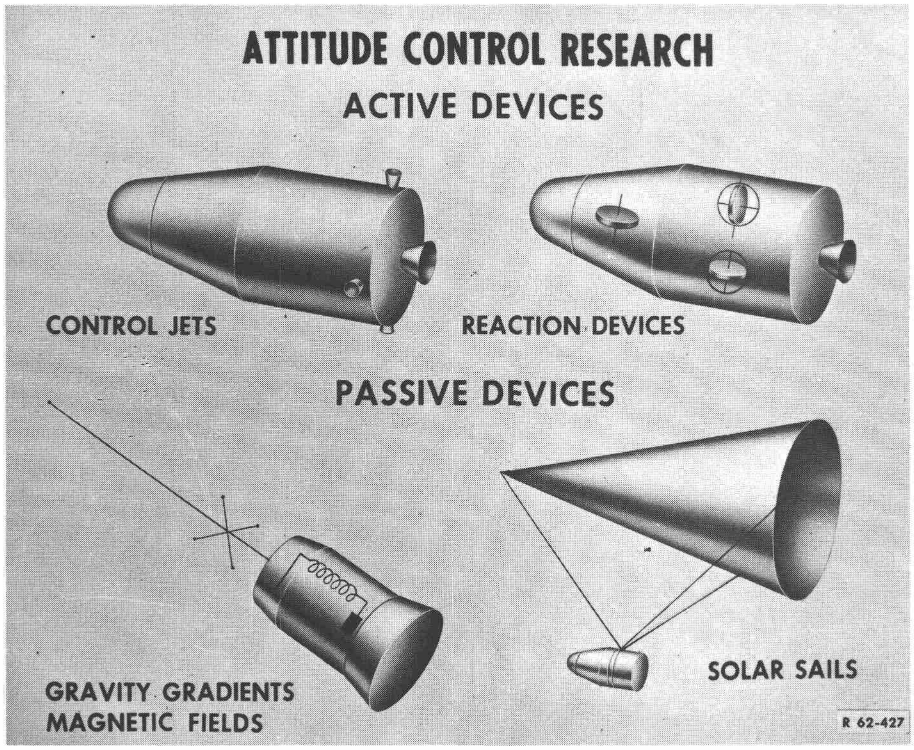


Figure 7

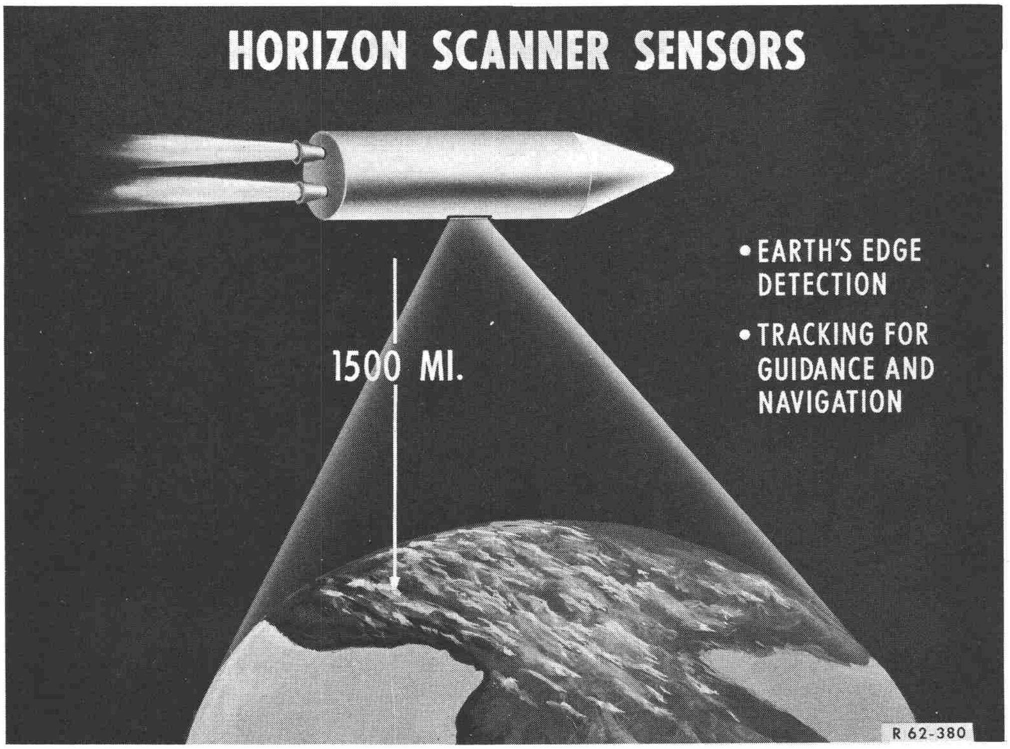


Figure 8

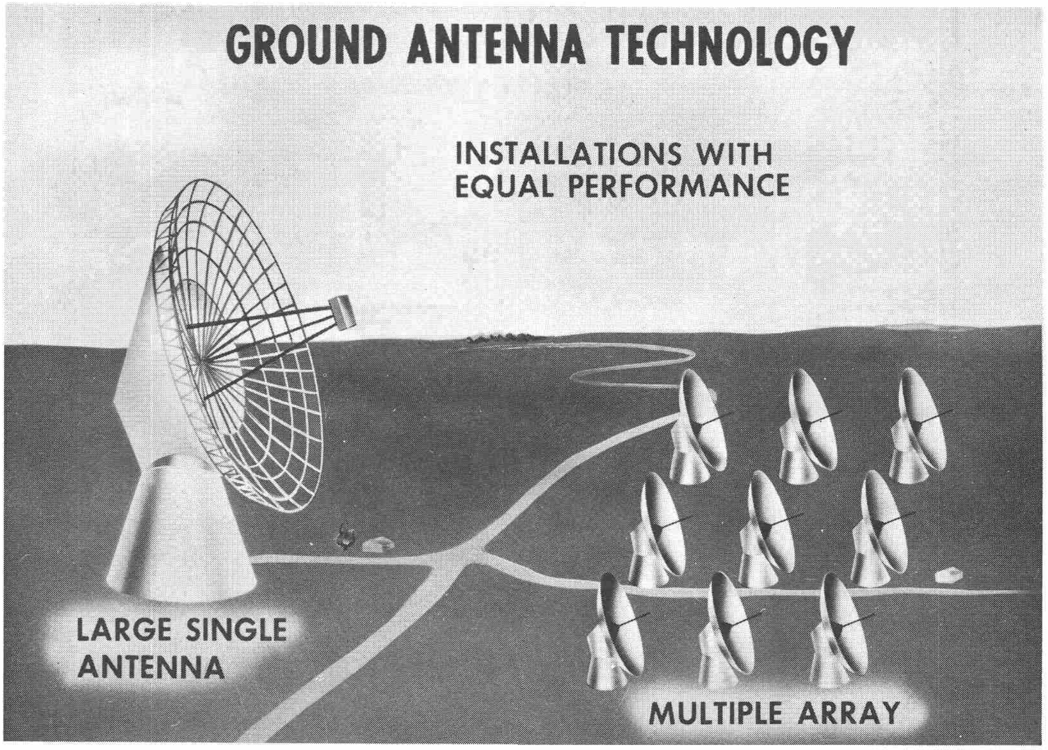


Figure 9

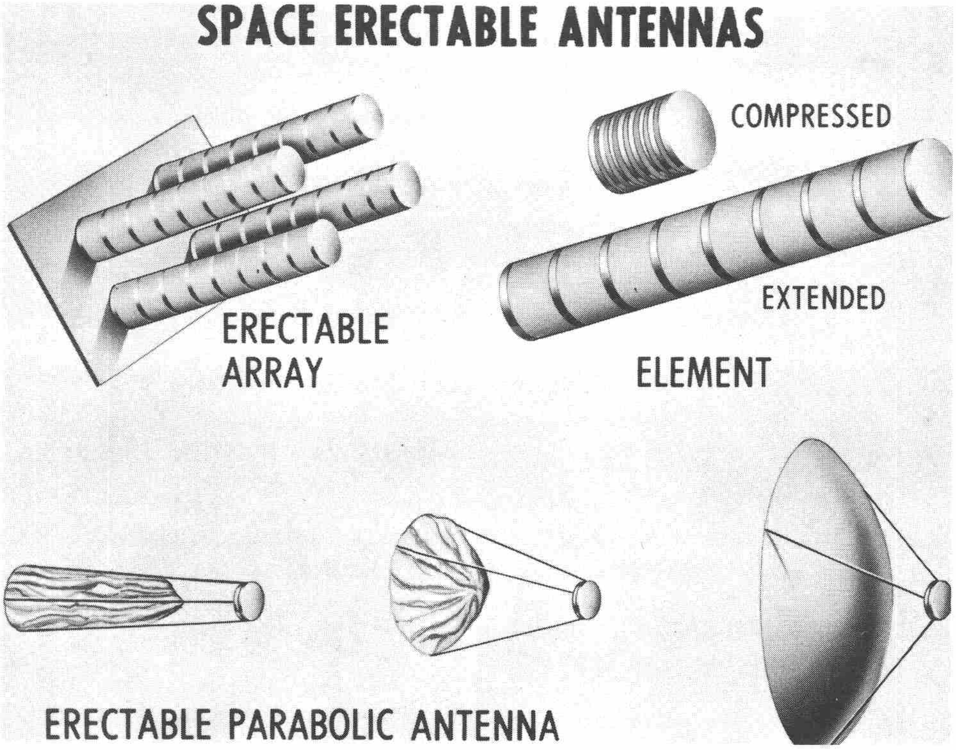


Figure 10

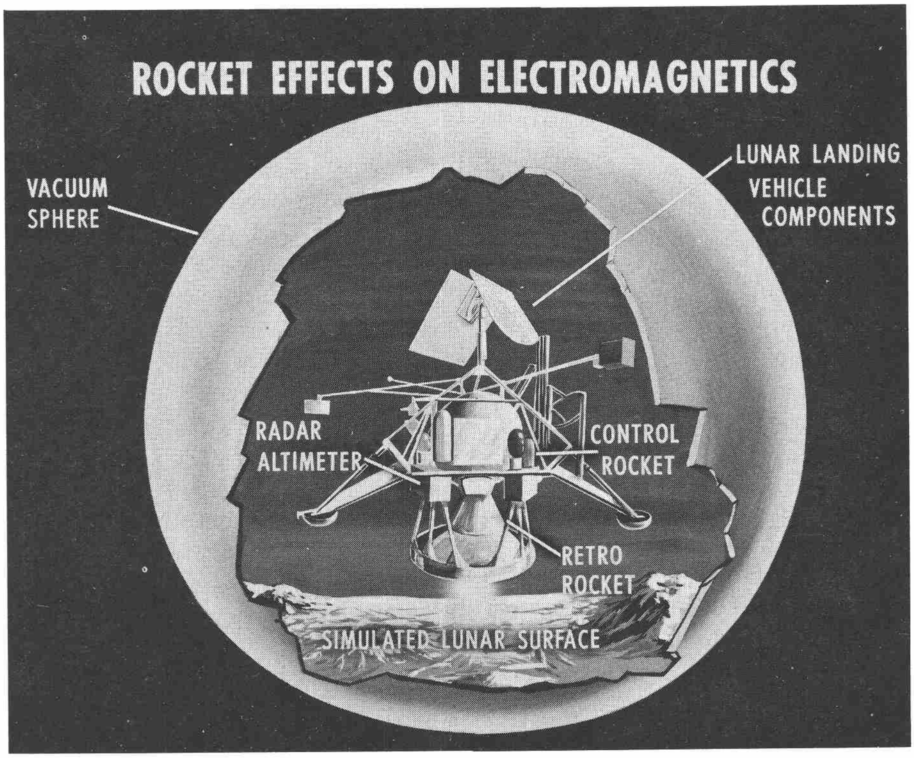


Figure 11

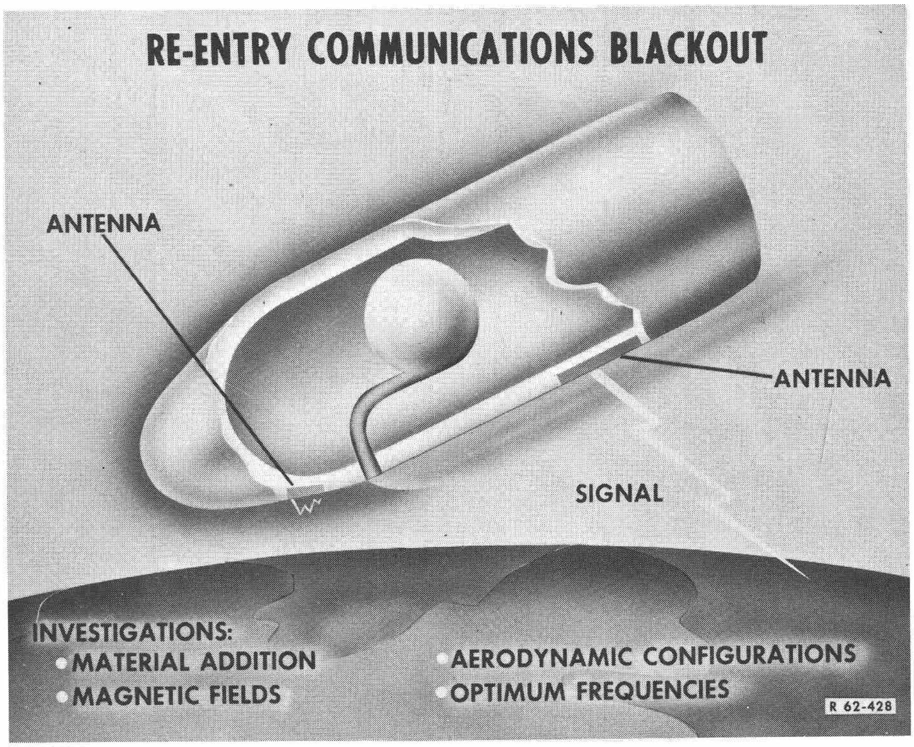


Figure 12

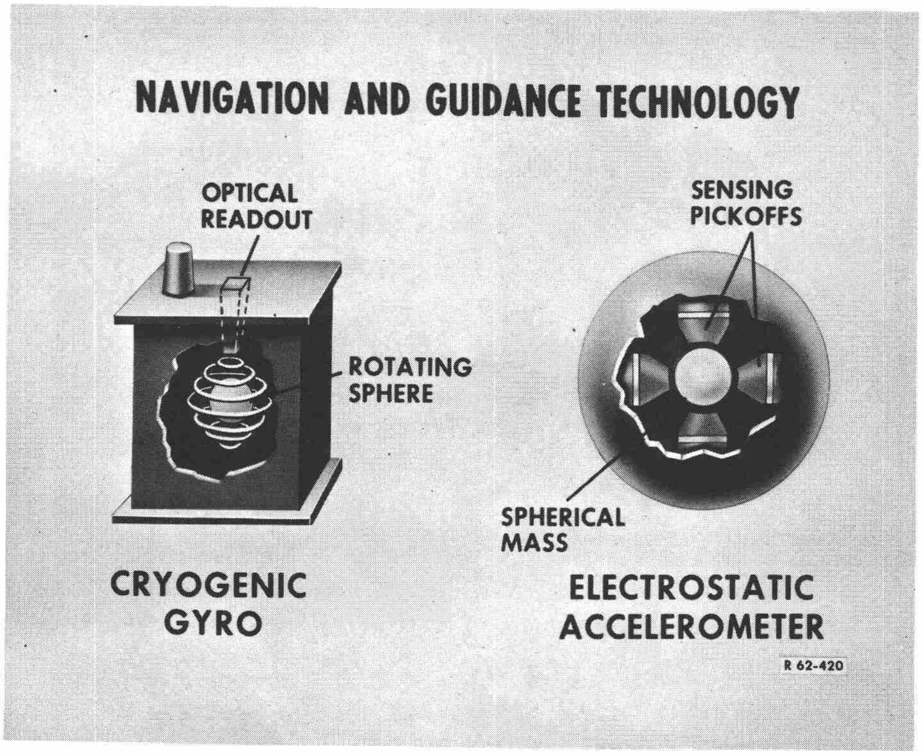


Figure 13

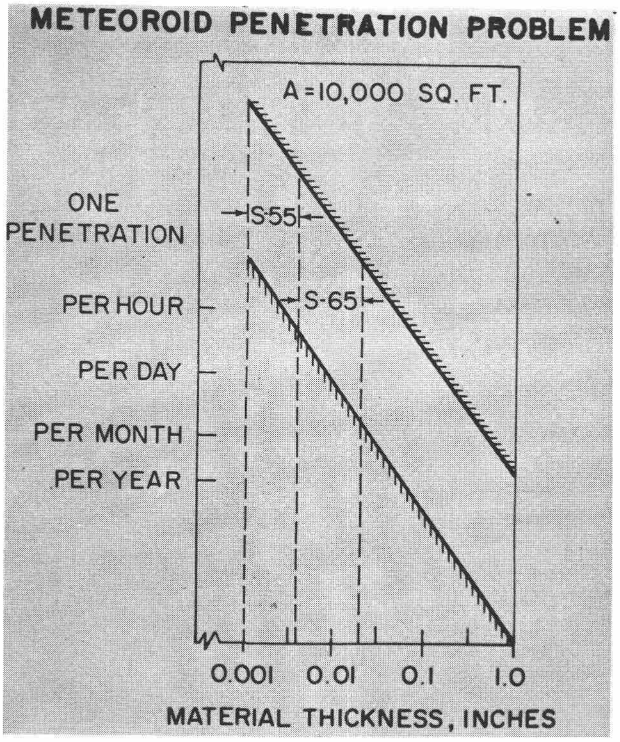


Figure 14

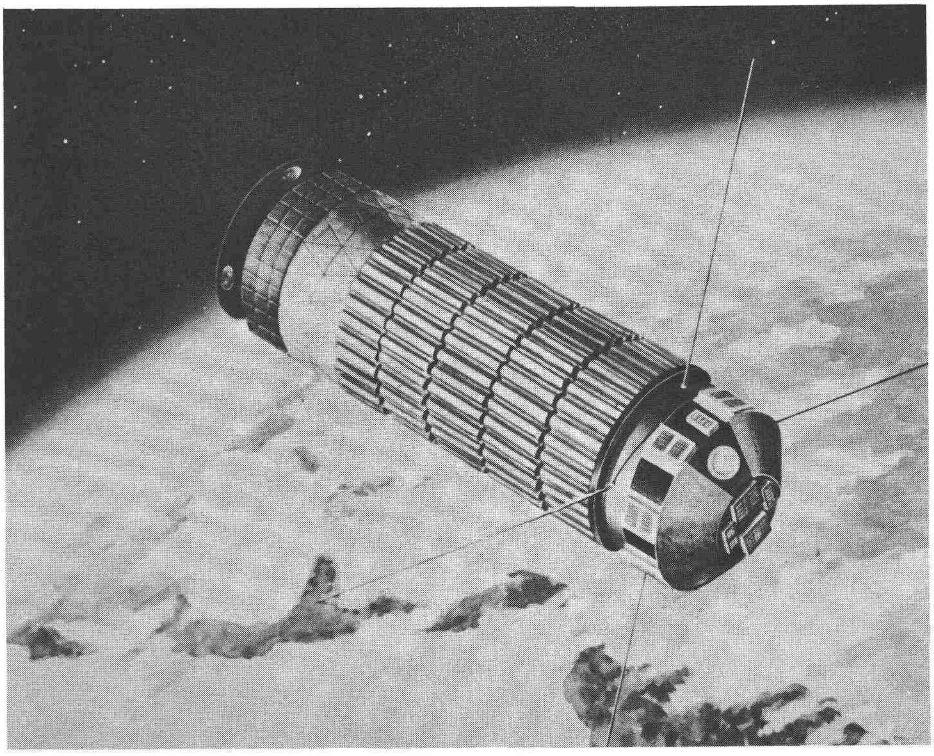


Figure 15

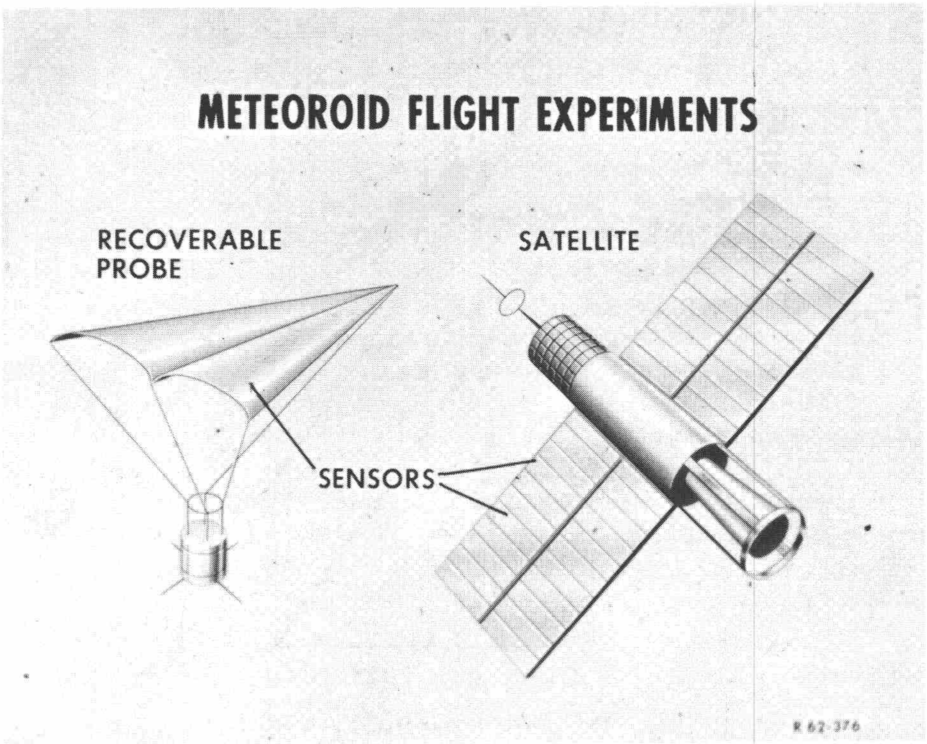


Figure 16

PROGRAM REVIEW AND RESOURCES MANAGEMENT

By John D. Nicolaides

The overall NASA Headquarters Organization is outlined in Figure 1. Mr. Webb and Dr. Dryden are appointed by the President. Dr. Seamans, a career employee, is general manager of the entire NASA operation. The purpose of this report is to describe the organization of the Office of Space Sciences and to indicate its distribution of funds and projects.

The scope of the NASA program and the moneys involved in carrying out the programs of the various offices for Fiscal Year 1963, are as follows: the Office of Manned Space Flight, $1,624 million; the Office of Space Sciences, $543 million; the Office of Advanced Research and Technology, $504 million; the Office of Tracking and Data Acquisition, $158 million; and the Office of Applications, $136 million. The total amount of money requested for research, development, and operation for NASA is $2,968 million for Fiscal Year 1963. This represents a tremendous increase. Only a few years ago the total NASA budget was a hundred million dollars or so.

Figure 2 presents the organization of the Office of Space Sciences. Of these six major groups only Biosciences, Lunar and Planetary, and Geophysics and Astronomy have space flight programs. Thus, not all the money involved is directly related immediately to obtaining scientific information from space flights. For space flights we have to develop vehicles where necessary; we also feel an obligation to have a very strong university program. This paper concerns the review and management phase, primarily resources distribution.

The distribution of funds within the Office of Space Sciences has already been pointed out in the paper by Dr. Newell. Figure 3 shows this distribution. The lunar and planetary program has $236 million, the geophysics and astronomy program has $147 million, and the vehicles development program has $106 million. It should be emphasized that the other flight program offices pay for their own vehicles. The vehicle office develops vehicles when new ones are required. The bioscience program has $31 million, the university program has $30 million, and the construction-of-facilities program has $23.1 million.

Figure 4 shows the distribution of the total OSS resources, as assigned to flight vehicles: 36.5 percent of our program is based on the Centaur vehicle; 39 percent is based on the Atlas-Agena vehicle; only 4.5 percent is based on the Delta vehicle; the Thor-Agena vehicle has 0.9 percent of our total funds applied to it; the Scout vehicle has 1.9 percent of the total funds; and sounding rockets have 1.8 percent.

The flight schedule currently under consideration for Calendar Years 1962 to 1965 is given in Table I. The Atlas-Agena B costs about $8.3 million. The Centaur costs $8.9 million; the Thor-Delta costs $2.5 million; the Scout vehicle costs

TABLE I

Project	Launches per Calendar Year 1962	1963	1964	1965	Launch Vehicle
Lunar					
Ranger	3	4	5		Atlas-Agena B
Surveyor			3	8	Centaur
Planetary					
Mariner	2		2		Atlas-Agena B
Mariner				3	Centaur
Interplanetary					
Pioneer (Quiet Solar Year)			1	2	Delta
Geophysics					
Earth					
Explorers	2	3	1	4	Scout
	2	4	4	5	Delta
	1	1			Thor-Agena B
Geoprobe			2		Scout
Orbiting Geophysical Observatory		2	1	1	Atlas-Agena B
Sun					
Orbiting Solar Observatory	1	2	2	2	Delta
Astronomy					
Orbiting Astronomical Observatory			1	1	Atlas-Agena B
Biosciences					
Pan	1	4			Thor-Agena B
Bios II	1	1	2		Scout or Delta

$1 million; and the Thor-Agena costs $6.8 million. Therefore programs which utilize launch vehicles expand costly facilities for each mission. The costs of spacecraft range all the way from $1.5 million to $21 million per spacecraft. That includes the development of the payload, supporting research associated with it, and all the other factors, except the launch vehicle. The funding of related facilities is handled by the offices operating these facilities. For example, Goddard pays for the data and tracking work. Where there are exceptional requirements, the Office of Space Sciences sometimes has to fund the related facilities. Some of these items are financed by Construction of Facilities funds. We also pay for all the data reduction by the experimenters.

The distribution of funds in the bioscience program is shown in Figure 5. The Pan program is a bioscience effort using primates, and has been assigned $14.2 million. It is the first U. S. effort of this kind. The supporting scientific work in the bioscience office requires $10.2 million. Bios is the concept of launching either a nonrecoverable satellite, or a simple recoverable satellite, containing bioscience experiments including small primates; $4.3 million has been assigned to this program. Reprograming accounts for $2.2 million and $0.2 million is allowed for "Other."

The distribution of funds in the lunar and planetary program is shown in Figure 6. The major funds of $46.7 million are planned for the Ranger effort. The Surveyor lander program has $44.9 million. The Mariner program, and in particular the two Venus shots this year, has been assigned $37.5 million. The Surveyor orbiter program planned for geodesy and reconnaissance of the Moon to meet requirements of both the Office of Manned Space Flight and the Office of Space Sciences is estimated to require $34.1 million. The Ranger follow-on program is allowed $33 million; the Mariner Mars program will require $20.2 million. The interplanetary probe is assigned $9.7 million and supporting research requires $8.9 million. The Voyager study has been allowed $1 million. A successful space flight program requires the design, fabrication, and thorough ground testing of spacecraft before flight and the conduct of selected experiments and associated ground-based research. In looking to the future, the Office of Space Sciences finds it necessary to schedule a large percentage of its annual resources on incremented payments for long lead-time items. This results in an apparent imbalance when the program is viewed in terms of space science conducted within a year.

Figure 7 shows the distribution of funds for the geophysics and astronomy program. The big item in this budget is the Orbiting Astronomical Observatory, $47.6 million; the second largest item is $44.9 million for the Orbiting Geophysical Observatory. Supporting science takes $14.5 million. The Orbiting Solar Observatory is assigned $11.5 million. The sounding rocket program is allowed $10 million; the international cooperative programs, $5.9 million; the interplanetary monitor, $4.8 million; the atmospheric structure satellite, $3.4 million; the ionosphere monitor satellite, $2.0 million; the energetic particle satellite, $1 million; the topside sounder, $0.9 million; and the geoprobe, $0.5 million. The largest projects, OAO, Ogo, and Oso, are all being flown on the Atlas-Agena vehicle.

Figure 8 shows the funding in a slightly different way; all the projects in the Office of Space Sciences are listed in order of amount of money involved. Only the Ranger, the Mariner Venus, the Explorers, and Oso are expected to result in space flights within Fiscal Year 1963.

NASA HEADQUARTERS ORGANIZATION

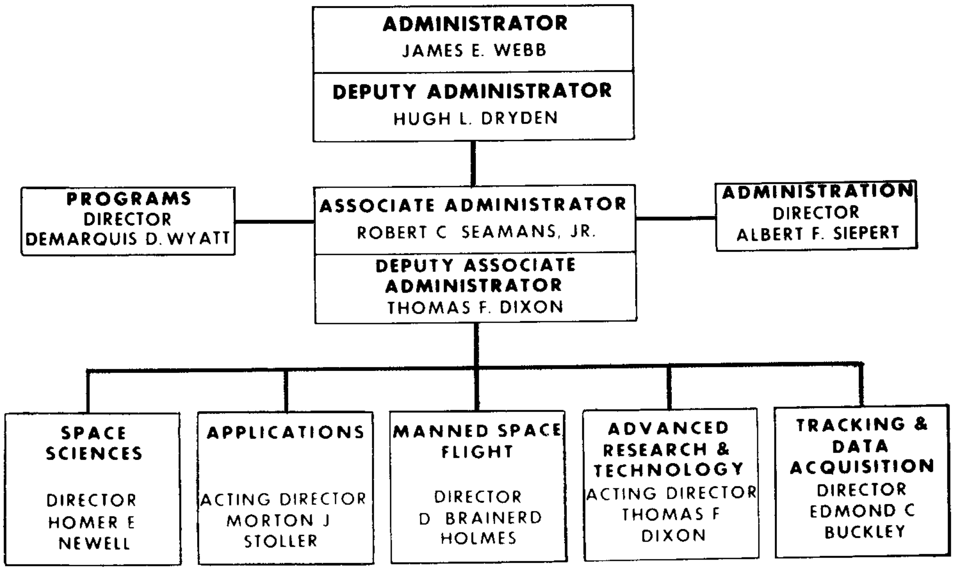


Figure 1

OFFICE OF SPACE SCIENCES

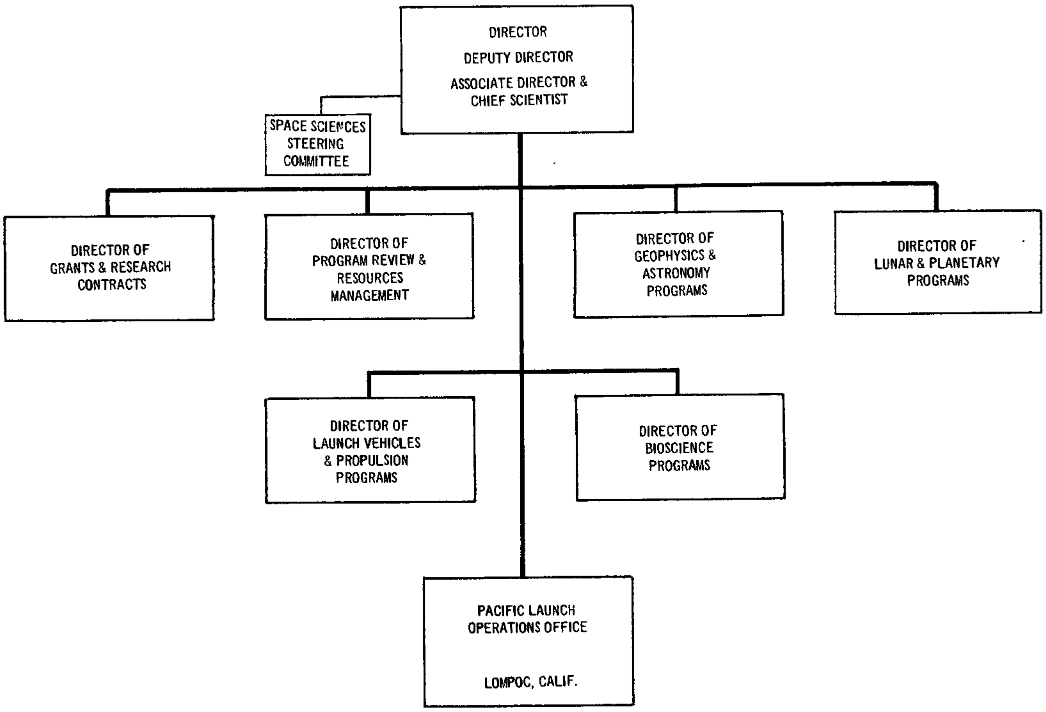


Figure 2

OSS FISCAL YEAR 1963 ESTIMATES
(TOTAL FUNDS $573,159 MILLION)

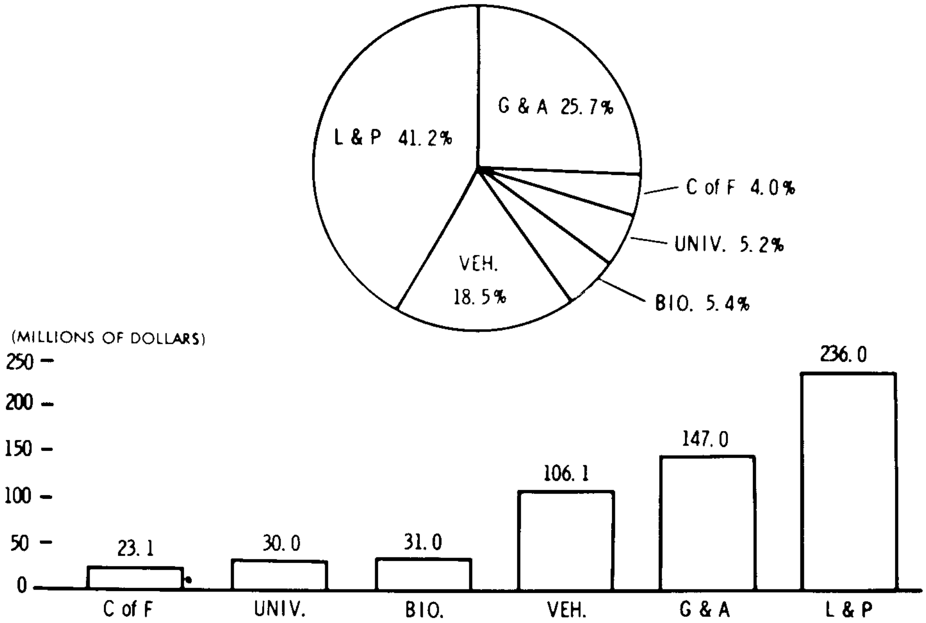


Figure 3

OSS FY '63 ESTIMATES
% PROGRAM BASED ON VEHICLES
(TOTAL FUNDS $550 MILLION)

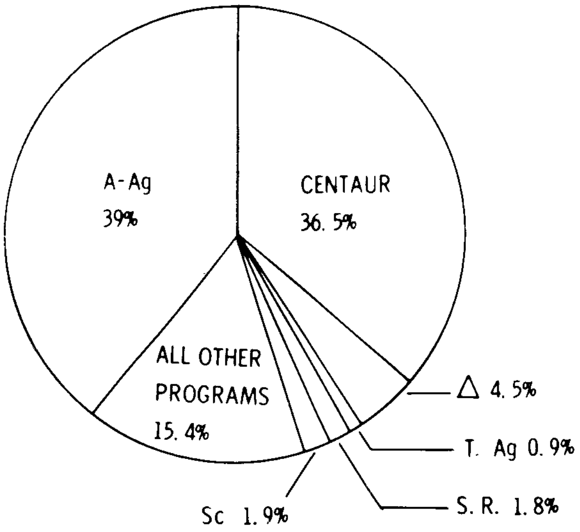


Figure 4

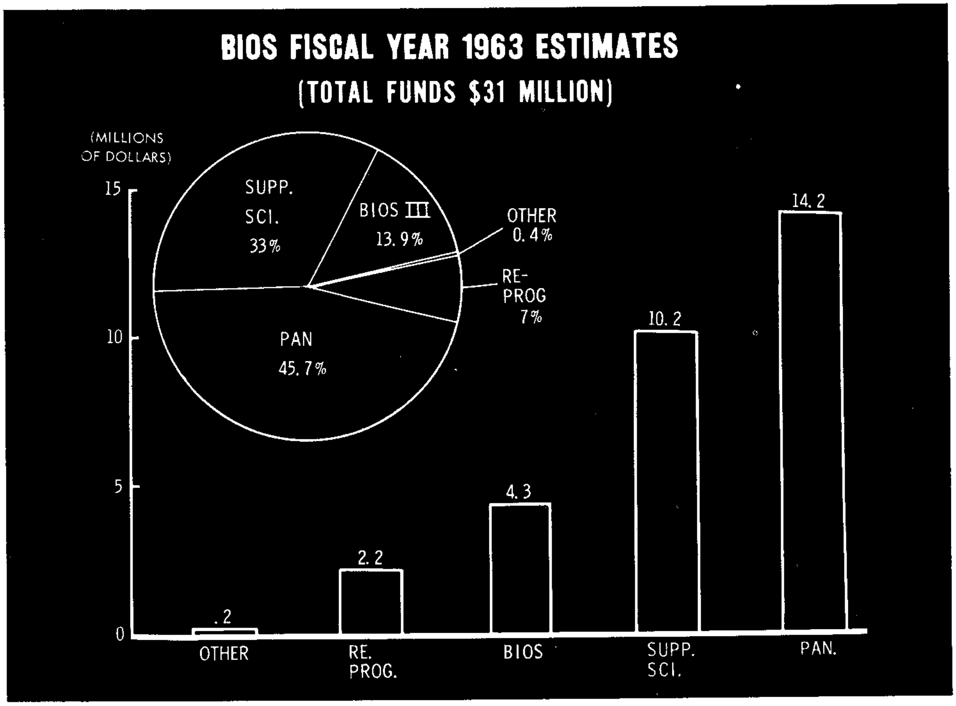
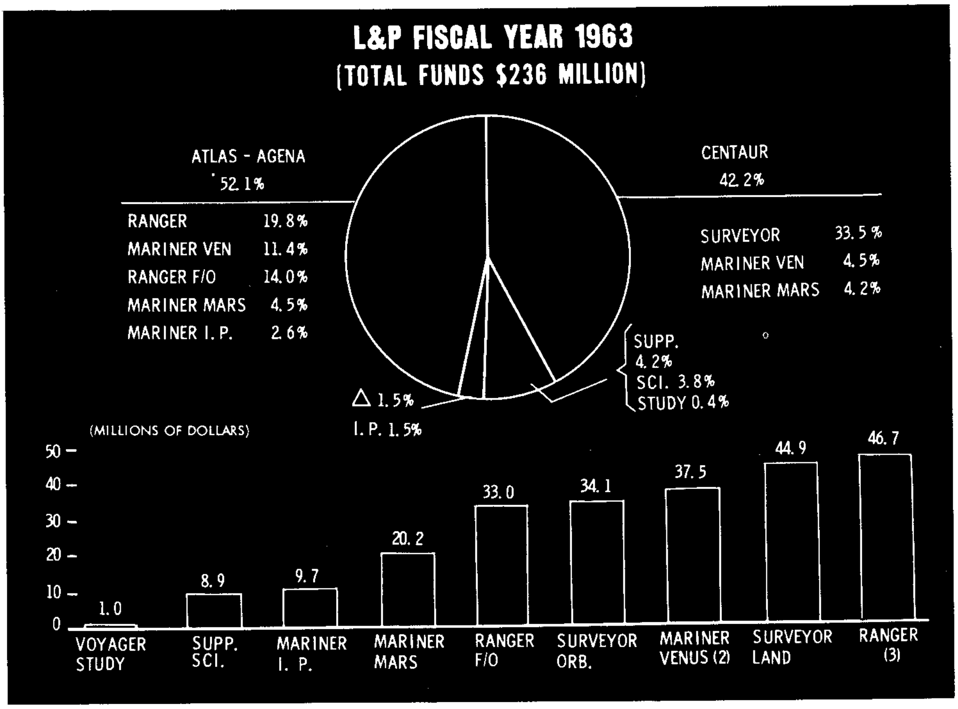


Figure 5



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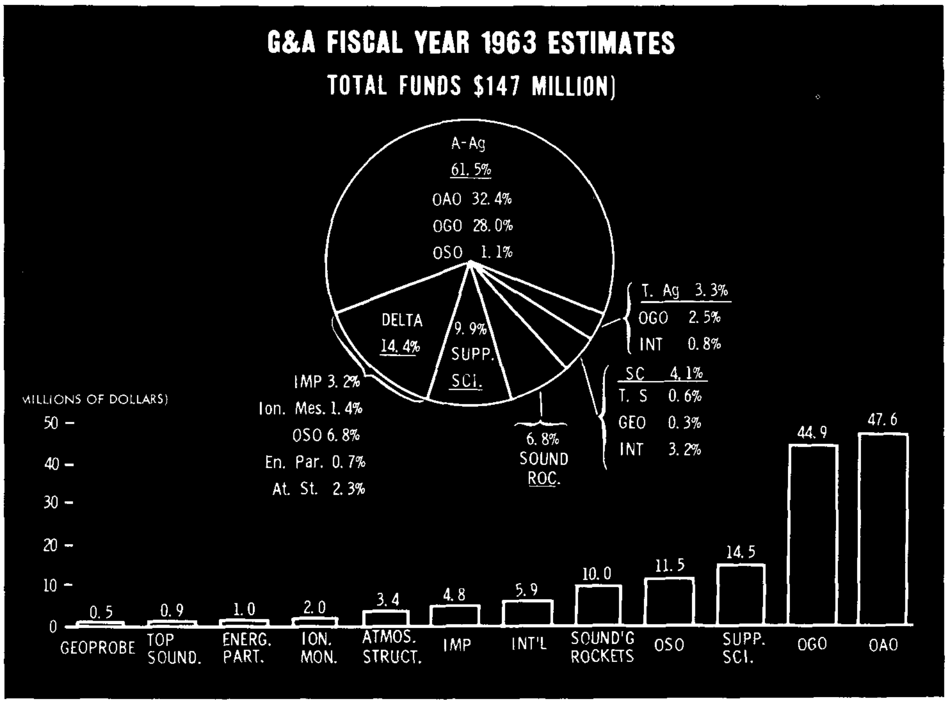


Figure 7

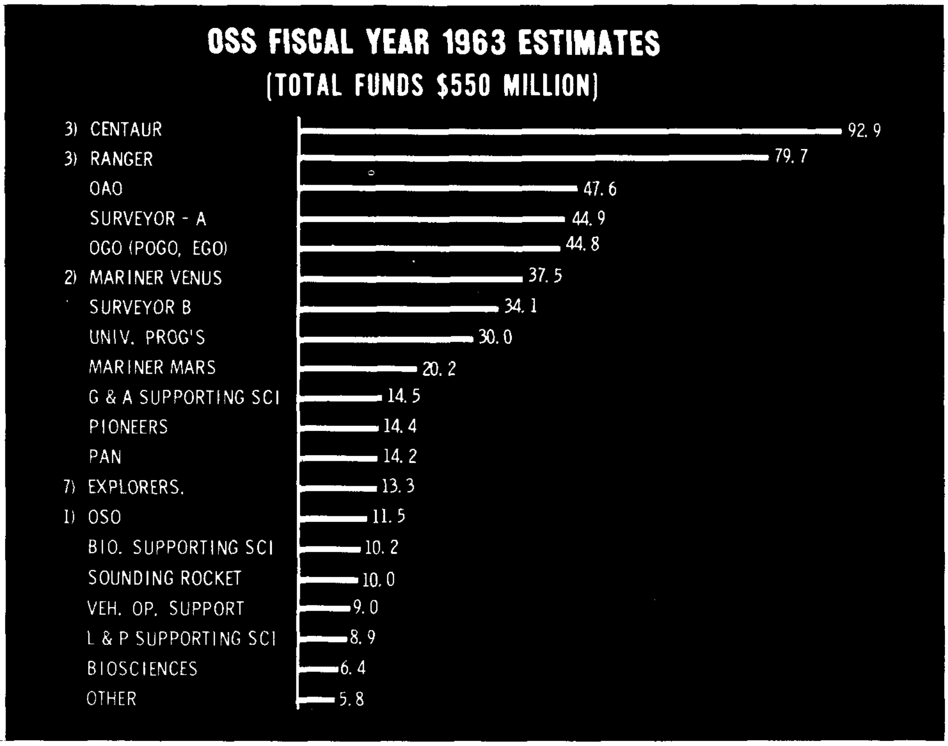


Figure 8

PROGRAM OF THE OFFICE OF GRANTS AND RESEARCH CONTRACTS

By Thomas L. K. Smull

This presentation concerns the role of the Office of Grants and Research Contracts within the NASA organization and, in particular, the manner in which NASA has been developing its relationship with universities.

When NASA was organized in 1958, it was recognized that there are special problems in dealing with educational institutions and that educational institutions at the same time have an important contribution to make in the space program. The Office of Grants and Research Contracts was established at that time. The functions of the office are: (1) to provide a central point in NASA for initiating and coordinating relationships between NASA and nonprofit scientific and educational institutions; (2) to develop policies and procedures for the negotiation and business management of grants and contracts with nonprofit scientific and educational institutions, and to establish such contractual terms as are necessary to assure uniformity in NASA's relationships with these organizations; (3) to receive, catalog, and insure the proper handling of all proposals.

In 1958, NASA started accepting unsolicited proposals from universities and other organizations and supported research through grants and contracts in a manner similar to that of other Federal agencies. This program grew as NASA grew. In the first year of operation we handled a program of some $6 million of which $3 million went into universities and the balance to nonprofit research organizations, other Government agencies, and a few industrial concerns. In the second year of operation we handled a program of about $12 million, of which $6 million went into the universities. In the third year of NASA operation the Office of Grants and Research Contracts handled a program of about $25 million, of which some $14 million went into the universities.

The decision a year ago to attempt a manned lunar landing by 1970 brought about another big step in the NASA program and an evaluation of the NASA-university relationship. We recognized that we had to take additional steps and do more than just continue support of the same sort of activity. Legislative authority which would permit NASA to provide facilities to universities and nonprofit organizations was obtained. At about the same time, NASA brought together a group of university working scientists, teachers, and administrators from all parts of the country representing the principal disciplines in which NASA has an interest to give a representative idea of the national interest in connection with the program. This group met with NASA representatives in the summer of 1961 for a general briefing and study of our activities in relation to the universities. From this study and other studies that were going on within NASA regarding ways to aid the lunar landing program (which is the focal point of NASA activity), we hoped to understand what NASA's responsibilities should be with relation to the university community. One area of concern was the educational problem—how to obtain sources of manpower needed to help achieve NASA objectives and at the same time strengthen the universities rather than weaken them. In what ways were we to continue to support the research activities in these universities—the traditional sources of knowledge? How could we continue to carry on this activity in an educational atmosphere and from this general type of activity evolve what is now called the sustaining university program. This paper concerns the answers to these questions. The university is the one contributor to the NASA program which produces new supplies of trained scientific manpower. All the rest of the activities use that manpower, but only the universities are reproductive. We have attempted and are still trying to develop our activities in such a manner as to increase the reproductivity instead of to hinder it.

The intent of the sponsored research program is to support basic and applied research projects by grants or contracts to advance the national space effort. The Office of Grants and Research Contracts serves as the focal point for the university and nonprofit activities of all the program offices. This sponsored research grew during the first three or four years to the level of some $14 million in Fiscal Year 1961. In FY 1962 approximately $25 million will go into the universities for sponsored research—activity directly related to NASA projects, both the space projects themselves and the underlying technology that is being developed.

The new program, the sustaining university program, is made up of three areas of action in support of research and training:

1. Training grants: To increase the supply of scientists and engineers in space-related science and technology

2. Facilities grants: To help provide facilities needed at universities for space research

3. Area research grants: To strengthen universities and enable them to increase their role in support of the NASA program

The fourth area, institutional liaison, is operational. Its purpose is: To establish and maintain scientist-to-scientist communications between NASA and the research community.

The NASA budget for Fiscal Years 1959 to 1969 is shown in Figure 1. It can be seen that the sponsored research and sustaining university program account for only a small part of the total NASA budget.

Figure 2 presents the budget for the sponsored research and the sustaining university program on a larger scale. The sustaining program was initiated in December 1961 when $12 million was made available for this activity. The expected growth patterns of both programs are indicated in the figure. Much of the money assigned to the sustaining program would be used for training and facilities.

Figure 3 shows the projected budget for the sustaining university program. The planned budget for FY 1963 was based on $40 million although this amount has not been approved. In the first year of this program, FY 1962, \$2 million was allotted to initiate a program of training grants, \$6-1/2 million for facilities, and \$3-1/2 million for area research. It is anticipated that, in general, these amounts will increase as shown in Figure 3. It is estimated that by 1970 approximately one out of four of all scientific and technical personnel in the United States will be engaged in one way or another in space activities. The responsibility of NASA in educating and training was recognized and the current training program was created. It includes:

1. University training grant program
2. NASA international training program
3. Resident research associates
4. Summer Institute

The university training grant program will be discussed in detail subsequently. Briefly, the NASA international training program is a fellowship program for the training of foreign scientists in space technology. It is a cooperative program with foreign countries. NASA pays for training in this country and the student's country pays for his transportation back and forth. This program at present is supporting 15 foreign fellowships. It is scheduled to increase to approximately 75. The resident research associate program is one that NASA has had underway since 1958. It is not really a pure university training program but it provides opportunities for postdoctoral students to work in our centers for short or long periods of time. This program provides approximately 60 to 65 such opportunities. This program is managed by the National Academy of Sciences for NASA, as is the international training program at our various centers. The summer institute is carried on in connection with the Institute of Space Studies located in New York. In 1962, a summer institute has been organized at Columbia University at which some 62 outstanding students (56 juniors, and six seniors) from all parts of the country will spend 8 weeks on intensive courses in space science. Included are visits to the Goddard Space Flight Center and the Marshall Space Flight Center.

The University representatives who met with NASA personnel in the summer of 1961 reviewed the activities of the various programs and concluded that a training program should include:

1. Expanded predoctoral training grant programs
2. Experimental undergraduate training
3. Extension to postdoctoral training

The figures supplied to the University-NASA group by the National Academy of Sciences, for instance, indicated that among applications they received for some of the fellowship programs which they administer for various Federal agencies, there were approximately 2,000 qualified candidates who were not receiving support. Furthermore, the national average for time between the B.A. or B.S. degree and the Ph.D. degree is approximately seven years. NASA felt that by making funds available to qualified predoctoral students to allow full-time work on graduate study and thereby cut down this span, an important contribution could be made to the training of additional people and an increase could be stimulated in the output of scientific personnel that the Nation is going to require to carry on the space research program. The training grant program therefore includes blocks of money that are made available to educational institutions to assist qualified and motivated students through three years of predoctoral training in space and related sciences and technology. Funds are made available for basic stipends to students of $2,400 for a full year's participation in graduate study, with an additional $1,000 placed with the university to administer in accordance with the university's policies for such items as dependency allowances, escalation, and so forth. If the student does well his first year in this program, his stipend may be increased. In other words, the stipend range is from $2,400 to $3,400. This program was not structured to make these the finest stipends available anywhere. The intent was merely to make them competitive. The university selects the students. The student may maintain his traineeship for three years if he maintains the standards required by the institution. If he does not, he may be replaced. We also make funds available to the university in connection with this program; in this area we are doing some experimenting. Some training programs that are underway have set the university allowance at a fixed number of dollars per student. We are not sure that this amount should be uniform from university to university. We were unable to find any good basic information as to what this cost really should be. Therefore, the university is granted an institutional allowance for the handling of this program with the understanding that they will indicate how they propose to spend the money. Does the university contemplate a modest equipment budget for the research the student will carry on? Do they expect to use some of these funds for course content improvement? Do they propose to use funds to hire additional faculty members to carry the teaching load imposed by these additional students. As the program grows, perhaps a fixed amount per student will be established. In order to get this program underway in January 1962, while we were working on the procedural details, we more or less arbitrarily selected some 10 universities throughout the country; we advised them that we would support 10 students at each of these schools, so that they could begin the selection procedures. The major criteria for selecting these universities were:

1. Accreditation ratings of the university.

2. Resources—laboratories, equipment, library, faculty, and so forth.

3. Previous and current efforts in planning and development of research activities in the space sciences.

4. Suitability of disciplines in which traineeships are requested and the university's research record in these disciplines.

5. Location of university and extent to which its region is adequately served with respect to existing opportunities for advanced training.

6. Need of the university for assistance in utilizing fully its own training capabilities.

The 10 schools that were selected are:

1. University of Chicago
2. Georgia Institute of Technology
3. State University of Iowa
4. University of Maryland
5. University of Michigan

6. University of Minnesota
7. Rice University
8. Rensselaer Polytechnic Institute
9. A & M College of Texas
10. University of California at Los Angeles

We make no claim that these are the finest 10 schools in the country. They were selected for a number of reasons. One important reason is that they were universities which were doing extensive research in the space sciences. In other cases, they were schools that had some relationship with the activities of our research centers. Also, in any program the geographic distribution of effort must be considered. We have had very favorable reports from the schools on the acceptance of this program. There has been a question of whether we are adding to the supply of manpower or displacing it. As mentioned previously some 2,000 qualified students are not getting this type of support; many of these probably would not continue their education, at least along these lines. We therefore believe that we are adding to the supply. It is anticipated that this program will grow. We expect to receive proposals from schools in late summer 1962 or early fall so that we will make the next group of such grants no later than November, 1962; thus, the universities will have an opportunity to make the existence of this training program known to students. Where this year we have supported a hundred students, we would hope to add 500 to 600 more who would enter school in September of 1963. As a result of their studies, our advisory group that met in 1961 placed before NASA a goal of some 4,000 students in this program with a view toward contributing to the production of a thousand Ph.D.'s a year.

Approximately a year ago NASA received authority to supply facilities to universities and nonprofit organizations, and our legislation even goes so far as to state that title to these facilities can be vested in the institution if it is determined by the Administrator to be in the best interest of the Government. Thus, we have been actively working on how we should best implement this part of our program; we have reviewed a number of proposals. We have been developing our policies and procedures to the point where we will begin to make some grants of this nature. The pattern that we would hope to follow, in general, is one in which we would consider making available money for those institutions which are active in research in the space areas, which have begun to make substantial contributions to the program, and which have become poorly housed as a consequence. From the 1962 budget, $6-1/2 million is being considered for this type of activity.

The criteria which will be used as a basis for issuing grants of this nature are as follows:

1. Relative importance of the research to the national space program.

2. Demonstrable competence, past achievements, and potential future accomplishments of the research groups.

3. Commitment of the institution to work in the space sciences.

4. Quality of supporting staff and facilities, and availability of other necessary support.

5. Soundness of building plan and reasonableness of cost.

6. Urgency of the university's need.

In general, NASA does not have a matching fund requirement. We will make funds available to educational institutions for research laboratory space. We will make facilities grants to educational institutions, but we will make no guarantee of future research support. We expect the university to operate and maintain the facilities for a reasonable period of time for the purpose for which they were intended. If good research is carried on in the facility, we will probably support it, but we will not guarantee support for activity. The five institutions and the facilities for which we are prepared to issue the grants are as follows:

1. Stanford University:
   Biomedical Instrumentation

2. State University of Iowa:
   Physics and Astronomy

3. University of Chicago:
   Space Experiments

4. University of California:
   Space Sciences

5. Rensselaer Polytechnic Institute:
   Materials Research

These facilities can be additions, they can be entire buildings, they can be parts of buildings. We merely want to make them available in those places where they are supporting our program and where there is interest in the university in carrying out its program.

The last part of this program is area research grants, the purpose of which is to strengthen the role of the universities in space sciences and technology by:

1. Filling gaps between related research projects already in existence

2. Providing a broad base of support upon which research projects may be built without damaging the universities' academic structure

3. Establishing new groups and opening up new avenues of research endeavor in places where latent talent is recognized and urgent need exists

These grants will be used in a number of experimental ways to support activities, both research and training, at universities. We find in some cases that we have activities going on in a university that come out of our program offices. While they may be specific, the addition of some of this type of funding will permit either more longevity or stability for the program or will permit some research assistants to get a small project underway that is directly related to the program. In the second area, some of the programs with which NASA is concerned will, by their very nature, introduce large perturbations in a university's overall activities.

If one of these programs gets in trouble, we want the university to draw on NASA money rather than on the university's funds. This amounts to putting a layer of funding between the university and the various projects so that if something goes wrong with a flight hardware project, for example, the university has NASA money with which to work out the problem.

Lastly, we propose to use some of these funds for the establishment of new groups. What are the criteria for selecting individuals or schools that possess an interest but lack sophistication? We do not know what these critera are, but we are prepared to experiment a little, and will probably make some mistakes. On the other hand, we hope that we will help to increase the interest and activities in this regard. This will not be a large share of our activity, but we will use some of the funds in this way.

The last area under sustaining university programs is institutional liaison, the purpose of which is: to establish and maintain scientist-to-scientist communication between NASA and the research community. It is necessary to supplement the civil service staff with a rotating group from the universities to provide an awareness of the problems of the universities. It may be possible that we can implement this on some kind of a contractual relationship.

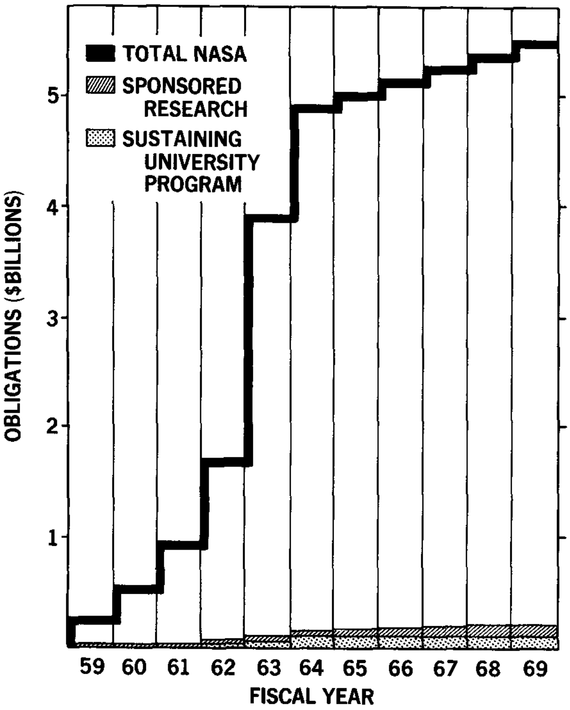


Figure 1

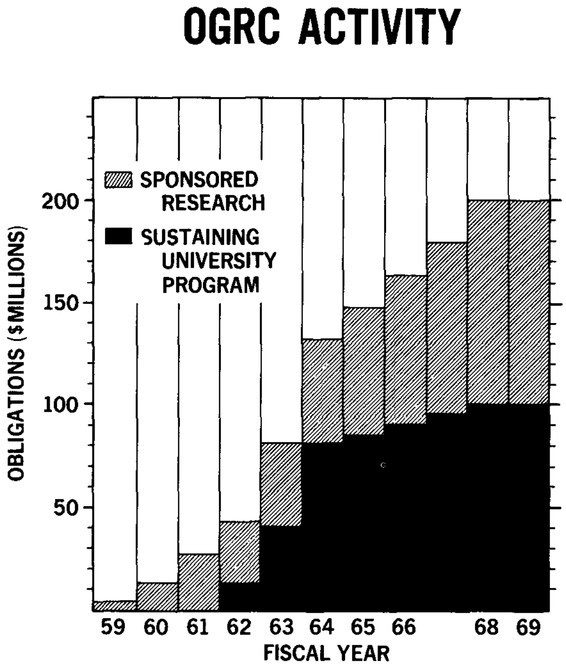


Figure 2

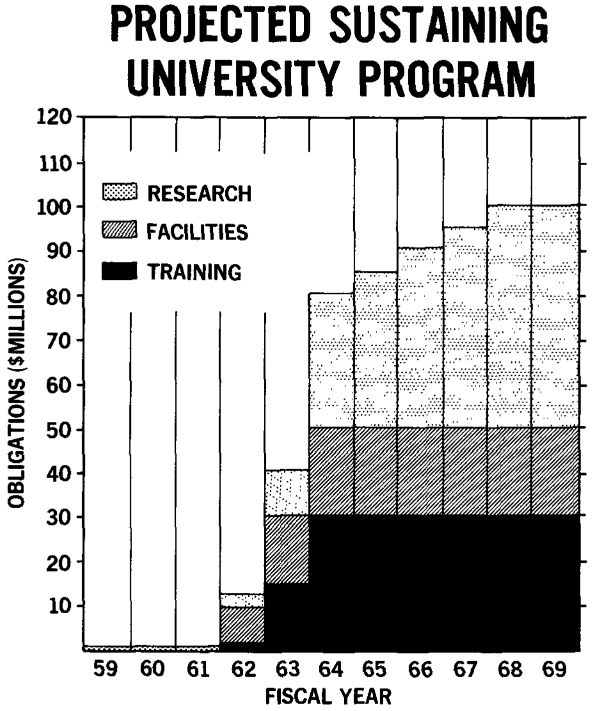


Figure 3

LAUNCH VEHICLE AND PROPULSION PROGRAM

By V. L. Johnson

The Office of Launch Vehicles and Propulsion Programs works not only for the Office of Space Sciences but for all NASA in developing, preparing, and launching the vehicles which are in our area of interest. We develop vehicles primarily to support mission requirements. We try to limit the total number of vehicles to a few different types in order that we can launch a number of each type in an attempt to reduce the cost and increase the reliability of each vehicle as much as possible. This paper will not cover vehicles such as Saturn nor advanced vehicles using nonchemical means of propulsion in any great detail. These are not yet operational vehicles; they are concepts and technological programs which we will use in the future.

The five vehicles shown in Figure 1 are currently in use and will be used for the next four or five years in almost all the space science work in the United States. A comparison of current payload capabilities in a low Earth orbit is shown in the figure: Scout, 150 pounds; Delta, 500 pounds; Thor-Agena B, 1,600 pounds; and, by extrapolation, Atlas-Agena, 5,000 pounds; and Centaur, 8,500 pounds.

The escape possibilities are: Scout has none at present; Delta is very modest (60 pounds); Thor-Agena also has no escape capability; Atlas-Agena and Centaur have escape capabilities of 750 and 2,300 pounds, respectively, and are planned for use in the lunar and planetary program.

We are improving the capability of Scout; the present 150-pound payload will be increased to better than 240 pounds by the end of 1962. Delta is in the same position; the 500-pound payload will be increased to about 800 pounds during 1962. The Delta escape capability will go up to about 120 pounds by the end of Calendar Year 1962. The first operational Centaurs will not have the 2,300-pound capability. Through an improvement program we hope to obtain about 2,500 pounds for the Surveyor program.

Figure 2 shows a Scout launching from Wallops Island, Virginia, early in 1961. Scout is a four-stage solid-propellant vehicle. Note that Scout is launched at an angle. This feature is unique among our orbital vehicles. Scout launches from the Pacific Missile Range are vertical. The program was initiated in 1958. In the development series the first and second launchings were successful. The third launching failed because of lack of ignition of the second stage. The fourth launching was successful and placed a 12-foot balloon in orbit. Of the next two one was successful and one failed completely. The last three were completely successful shots. There have been no NASA launchings of Scout since March 1962. There have been two launchings by the Air Force on the West Coast.

Figure 3 indicates the methods by which we are improving Scout's capability. The 150-pound original capability has been increased to about 165 pounds by an improved third-stage motor. The improved first-stage motor to be flown soon will increase the capability to slightly over 200 pounds and the improved fourth-stage motor, which will be flown late in 1962, will raise this to roughly 250 pounds in a 300-mile orbit.

Figure 4 indicates, in general, the NASA mission for the Scout vehicle. Explorer IX was a 12-foot balloon which is still orbiting. Explorer XIII was a micrometeorite satellite which reentered the atmosphere in 3 days. Scout is a coordinated NASA-DOD program. NASA is the development and procurement agency; DOD buys their Scout vehicles through NASA. The NASA conducts Scout launchings from Wallops Island; the Air Force, or an Air Force crew, handles Scout launchings from the Pacific Missile Range, including NASA Scouts which will be launched from that location. This is a fully coordinated program—the Air Force has at least an equal number of payloads scheduled to fly on Scout.

Figure 5 is a photograph of the first Delta launching from the Atlantic Missile Range. It failed during postflight and we did not achieve orbit of the Echo balloon on the first attempt.

Figure 6 gives the characteristics of Delta, or Thor-Delta, which is a three-stage vehicle derived from the Thor-Able and Vanguard series of vehicles which preceded it. The first stage is a Thor vehicle powered by kerosene and lox; the second stage is similar to the Able stage using hydrazene and nitric acid; the third stage is a spin-stabilized solid rocket which is identical to the fourth stage of Scout. The payload capability, currently 500 pounds, is limited by a structural factor and is not a performance limitation of the vehicle, which is capable of 800 pounds. The space probe capability will be somewhat better than 120 pounds by the end of 1962. Delta is used for many application and scientific satellites. A number of space probes are planned for the vehicle. It is managed within NASA as an element of technical direction by the Goddard Space Flight Center. The prime contractor is the Douglas Aircraft Company. The launchings through March 1962 are shown on the right side of Figure 6. All were successful with the exception of the first one. In April 1962, Delta orbited the S-51 or Aerial satellite built in the United Kingdom, the first international satellite; in June 1962 it orbited Tiros V.

Figure 7 indicates the missions which have been assigned to Delta. Delta was originally conceived as an interim vehicle which would be used by NASA only until vehicles such as Scout and Thor-Agena B became operational. The relatively successful record of Delta and the fairly large cost differential between Delta and Thor-Agena B has resulted in an extension of the Delta program. It is no longer referred to as an interim vehicle; it is now one of the NASA stable of vehicles and will remain so for a number of years. In addition to the missions shown, we have added to Delta the Imp or Interplanetary Monitoring Probe mission, a probable ionosphere monitor, a probable biological shot, and some missions for the Office of Advanced Research and Technology. The Interplanetary Monitoring Probe can be carried on Delta only because we entered into a program of improvement of payload capabilities for the vehicle. Had we not done this and had we maintained the same payload weight or spacecraft weight for Imp, the only vehicle in the stable capable of launching it would have been the Atlas-Agena. Improving Delta and carrying this particular payload on Delta saves approximately $5 million per launching. We think that this sort of gradual improvement in a vehicle is well worth the cost and also well worth the admitted risk of making any changes to a vehicle which is operating well.

Figure 8 is a photograph of the Atlas-Agena used with Ranger I at AMR.

Figure 9 describes both Thor and Atlas-Agena B. The Agena B program for NASA is under the technical direction of the Marshall Space Flight Center operating through the Air Force to the contractors involved. The Thor-Agena B, a vehicle which has no escape capabilities, is being used by NASA in relatively low Earth orbits. The first launching of Thor-Agena B for NASA will take place in late 1962. This is substantially the same vehicle that the Department of Defense called the Discoverer series before they abandoned names and adopted numbers which now describe all their programs. Atlas-Agena B is a DOD-developed vehicle. The NASA is using it in very much the same configuration that they are except that the NASA configuration has a substantially larger mission capability and a substantial lunar capability; its uses are indicated in Figure 9.

Figure 10 lists the missions which are currently being flown on the Agena B vehicles, Thor and Atlas. We will probably add some additional escape capability to Atlas-Agena B for planetary missions while we are waiting for the Centaur development to be completed. There are possibilities of adding high-speed reentry missions to the Atlas-Agena B program. Possibly with the addition of a third stage, and added propulsion capability in the payload, 24-hour equatorial orbit missions can be included in the Atlas-Agena B program.

Some details of the Centaur are given in Figure 11. A relatively new technology is employed in Centaur; the high-energy upper stage uses liquid hydrogen and oxygen. We are encountering all the problems that have been predicted for this combination. The development program is costing more money than originally anticipated and it is taking more time than anticipated. The Advent program no longer depends on Centaur and, in fact, it has been completely reoriented by DOD. As a result of this, since Centaur now has no 24-hour equatorial mission, we have modified the development program for Centaur and are now developing it as a two-burn upper stage vehicle which by itself will have no capabilities in the 24-hour orbit. We do feel that there are inevitable 24-hour missions for a vehicle in Centaur's class and we are proceeding with a study to use the Surveyor with a solid-propellant additional stage flying on top of Centaur. This will give a rather appreciable capability in the 24-hour equatorial orbit, approximately 1,300 pounds. This is a study program only. We will complete the study but we will delay the development, until we see a firm mission coming up for this type of vehicle. The contractor for both stages is General Dynamics-Astronautics; Marshall Space Flight Center has technical direction of the program for NASA.

The Centaur program was initiated in late 1958. The program was, in fact, initiated as an experiment in adapting the use of hydrogen to an upper stage and some while after the initiation of the program, it did not have assigned missions. This is one of the reasons that the program appears to be overrunning very badly. It has been overrunning, but not so badly as the initial figures for the program in 1958 indicate. The program has expanded considerably and it is now aimed at a fully operational vehicle to be available in mid-1964.

Figure 12 is a cutaway or an exploded view of the Centaur. It is 108 feet long by 10 feet in diameter. The main element in Centaur is the hydrogen tank. The hydrogen tank must be insulated during ascent through the atmosphere and during its time on the pad. The insulation panels are jettisoned just above the atmosphere in space. There is a small gap between the insulation panels and the nose cone. This gap is filled by a weather shield (not shown) which goes over the insulation panels and up against the nose cone. In the first flight of Centaur, this weather shield came off prior to Mach 1. An analysis of the failure indicated that we had simply made an error in calculating the aerodynamic forces on the weather shield. The loss of the weather shield probably exposed the open end of a tunnel containing wiring which was ripped off and probably caused the hydrogen tank to rupture. The Atlas worked well. The vehicle did explode but not before the tank had ruptured and the hydrogen cloud, or probably hydrogen vapor cloud, had completely enveloped the vehicle.

There is still an aerodynamic problem with Centaur after 45 seconds of flight. We may find others on the next flight. The engine has been qualified for flight. Recently two engines satisfactorily passed a full-duration run at the Flight Research Center. We are sure that we will find some other problems in actual space flight, but we think that the major engine problems have been solved. There is a major continuing problem in connection with the bulkhead, which separates the oxygen tank from the hydrogen tank. The problem is to control the heat flux across this bulkhead from the relatively warm liquid oxygen into the hydrogen tank. The present design depends for its efficiency on the maintenance of a very high vacuum between the two thin walls of the bulkhead. Any leakage of either oxygen or hydrogen into this space rapidly destroys the insulating efficiency of the bulkhead. The problem has been somewhat alleviated by the elimination of the 24-hour-orbit requirement for Centaur. The plan to use Atlas-Centaur as a two-stage vehicle to go into a 24-hour orbit required a 5-1/2-hour coast period between the second burn of Centaur, which took it from a low parking orbit into a transfer orbit at the 24-hour altitude and the third burn, which occurs at synchronous altitude in order to create a circular orbit and to change the plane to equatorial. This 5-1/2-hour coast period with about 10 percent of propellent aboard imposes a severe requirement on the bulkhead insulation coverage. Since this plan has now been dropped, the bulkhead problem has been reduced in magnitude, although we still have not solved the problem to the point where we can predict the heat flux across the bulkhead and the attendant loss of hydrogen by boiloff. There are also guidance problems in the vehicle although these are not particularly severe. We are going ahead with the study of the Surveyor bus, which will give a 24-hour orbit capability.

Figure 13 indicates the three sites from which we are currently launching orbital and escape vehicles: Wallops Island, Virginia; Atlantic Missile Range (AMR), Florida; and Pacific Missile Range (PMR), California. Azimuth angles are indicated in the figure; these are the normal range limits from each of the ranges. Wallops Island is used only for launching Scout. None of the other vehicles are now being launched from Wallops nor do we plan to put a capability for launching the other vehicles there. The Atlantic Missile Range is currently being used to launch Delta, Atlas-Agena, and Centaur. The launches from AMR include those satellite launches which go into easterly inclined orbits. Some of the reentry shots will also go from AMR to take advantage of the substantial down-range instrumentation. These range limits are associated with inclination. Launching straight along the 47° flight azimuth from AMR will give approximately a 48° or 49° inclination orbit. A 110° launch to the southeast results in an inclination orbit of approximately 33°. Vehicles having higher inclinations have been launched from AMR. Recently a Delta was launched in a northeasterly direction achieving an inclination of 58°. This was accomplished by flying an angle of attack of about 30°, yawing the second stage to the left around the coast of Newfoundland, disposing of the first stage off the Nova Scotia coast, disposing of the second stage south of Iceland, and then overflying northern and central Europe. The previous Delta launch was to the southeast. This was also a dogleg launch with two yaws in the program. The resulting inclination was about 54° in this case. A recent Transit spacecraft achieved an inclination angle of 67° to the southeast.

It is technically possible to achieve a polar orbit from AMR. This could be done without overflying Miami. It is not particularly dangerous to either life or property, any more so than most of the other space orbits are, and within a few years either the NASA or the DOD may possibly attempt a polar orbit from AMR after full investigation of all the possibilities involved.

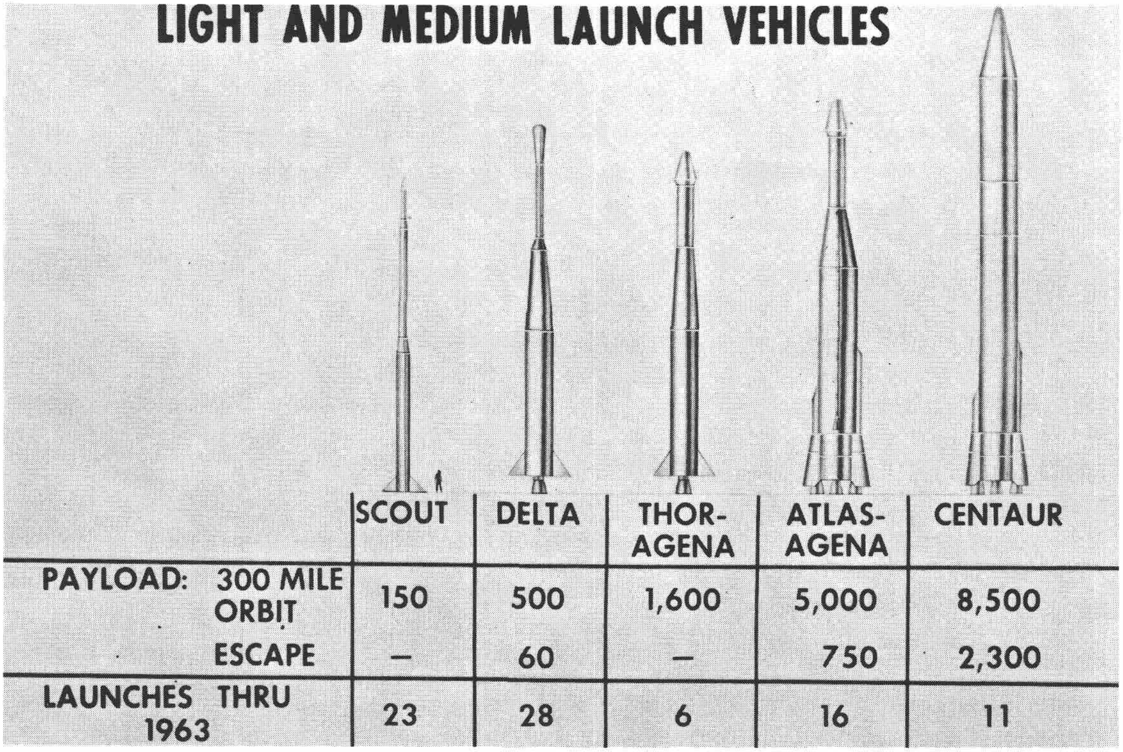
We take extreme precautions on all our launches to avoid disposing of stages either deliberately or inadvertently over land areas. The NASA is using the Pacific Missile Range to launch Scout, Thor-Agena B, and Atlas-Agena B. There is no Delta or Atlas-Centaur capability at PMR. The Pacific Missile Range is used primarily for polar orbits. Launching south from PMR allows an almost completely clear flight with no great overflight problems except the overfly of Russia, halfway around the world, at orbit entry.

A comparison of the payload capabilities of the five vehicles discussed herein is presented in Figure 14. Capabilities of the improved Scout will fall about halfway between its current capability and that of Delta. The improved Delta will fall about halfway between its current capability and that of Thor-Agena B.

Centaur and Atlas-Agena B payload capabilities when launched in an inclined orbit due east from Cape Canaveral are indicated in Figure 1 by the solid lines. The dashed lines are included to indicate the payload penalty paid in attempting to fly an equatorial orbit from AMR. This penalty is substantial in Centaur; in Atlas-Agena B without a third stage the capability substantially disappears if an equatorial circular orbit is flown from the Atlantic Missile Range. The addition of third stage to either of these vehicles, if properly sized, will give Atlas-Agena B a payload capability of approximately 500 pounds in synchronous orbit and it will give Centaur a payload capability of approximately 1,300 pounds in an equatorial orbit from AMR.

Figure 15 shows the payloads in terms of apogee altitude for Delta, Thor-Agena B, and Atlas-Agena B. The inclination of 63.4° is associated with an eccentric orbit in which the line of apsides (the line between the perigee and the apogee) does not rotate in the plane of the orbit. This is of interest to communications experimenters in that the apogee of a spacecraft launched into this orbit would remain over the same latitude. It would not rotate within the orbit as is the case at all other inclinations. Because of its very large capabilities on this scale, Centaur is not shown in Figure 15. In the lower left-hand corner, crossover occurs because a three-stage vehicle (Delta) has a much greater eccentric orbit capability than a two-stage vehicle (Thor-Agena B). They approach the space capability when they reach high-rate eccentric orbits and, in this case, Delta and Atlas-Agena B both have usable capabilities in highly eccentric orbits.

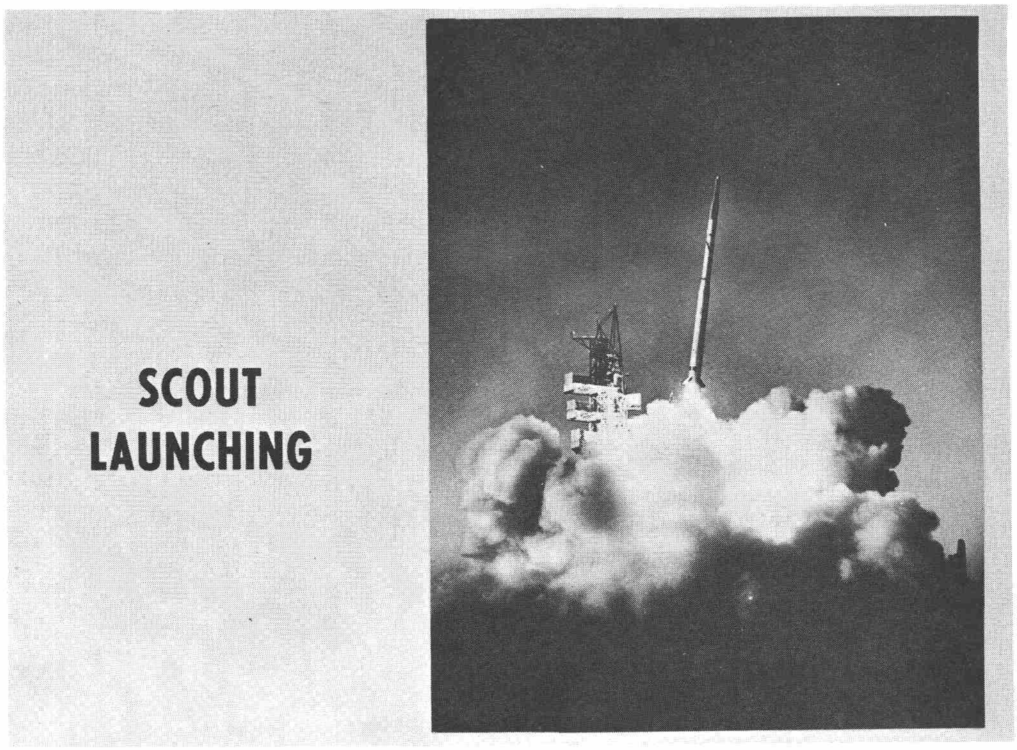
Constant efforts are being made to improve the reliability and decrease the costs, where possible, of our present vehicles. Also an attempt is being made to reduce the turnaround time on launch pads; if this time can be reduced the requirements for new launching pads and all the attendant equipment costs can be reduced. From the standpoint of new vehicles, we have some studies underway investigating the possibility of developing new vehicles in the same payload class as the present operating vehicles. The aim would be to achieve an economic advantage, which includes not only the cost of the vehicle but also the reliability, the turnaround time, the capability of the vehicles to launch exactly on time, which can be quite important for certain missions, and so forth. Studies indicate that within the next few years the technologic base will allow the development of a new vehicle with the assurance that we can amortize the development cost in a very short period of time. Advanced vehicles, using nonchemical means for propulsion, will be required for flight beyond the near planets, for flight far out of the plane of the ecliptic, and, ultimately, for flight beyond the solar system. Such vehicles will certainly be developed when the technology of propulsion is sufficiently advanced to warrant a vehicle design and when we have missions which will support such development.



LIGHT AND MEDIUM LAUNCH VEHICLES

		SCOUT	DELTA	THOR-AGENA	ATLAS-AGENA	CENTAUR
PAYLOAD:	300 MILE ORBIT	150	500	1,600	5,000	8,500
	ESCAPE	—	60	—	750	2,300
LAUNCHES THRU 1963		23	28	6	16	11

Figure 1



SCOUT
LAUNCHING

Figure 2

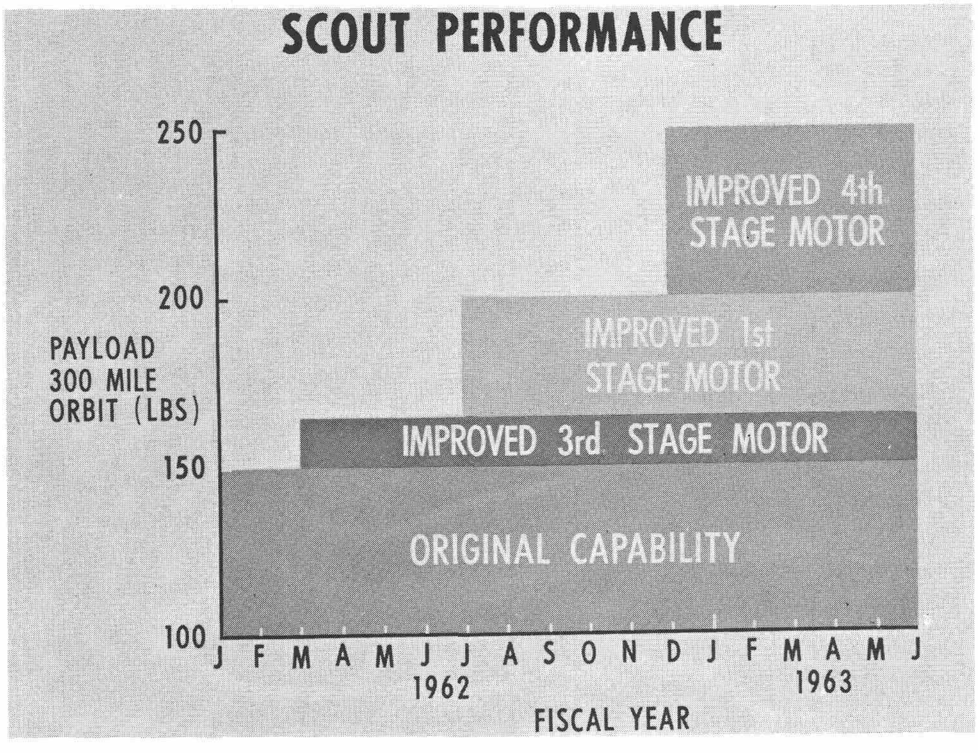
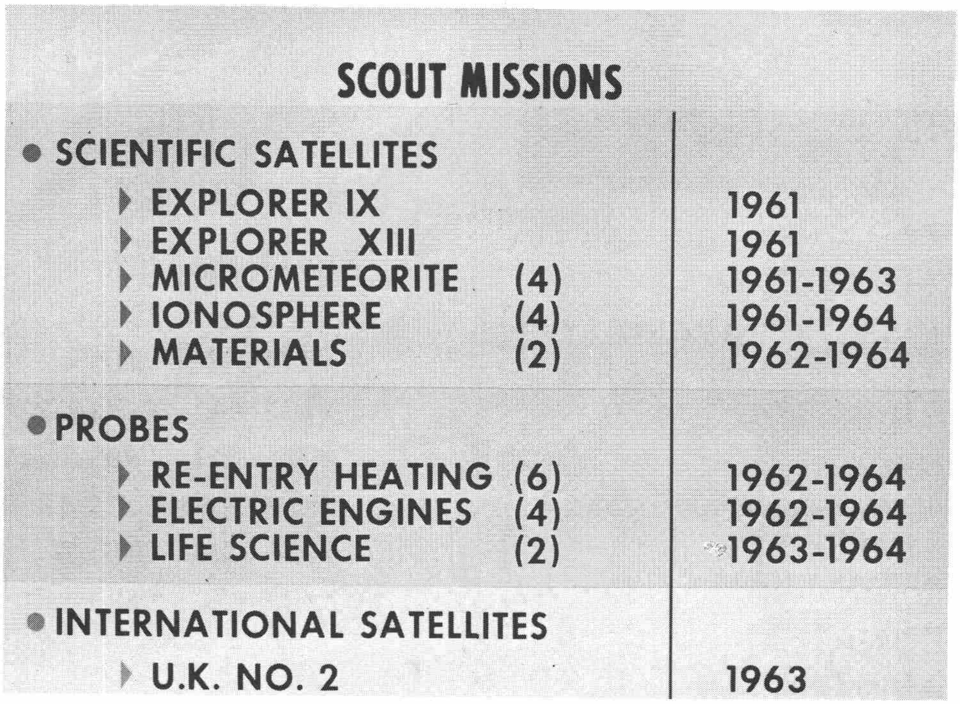


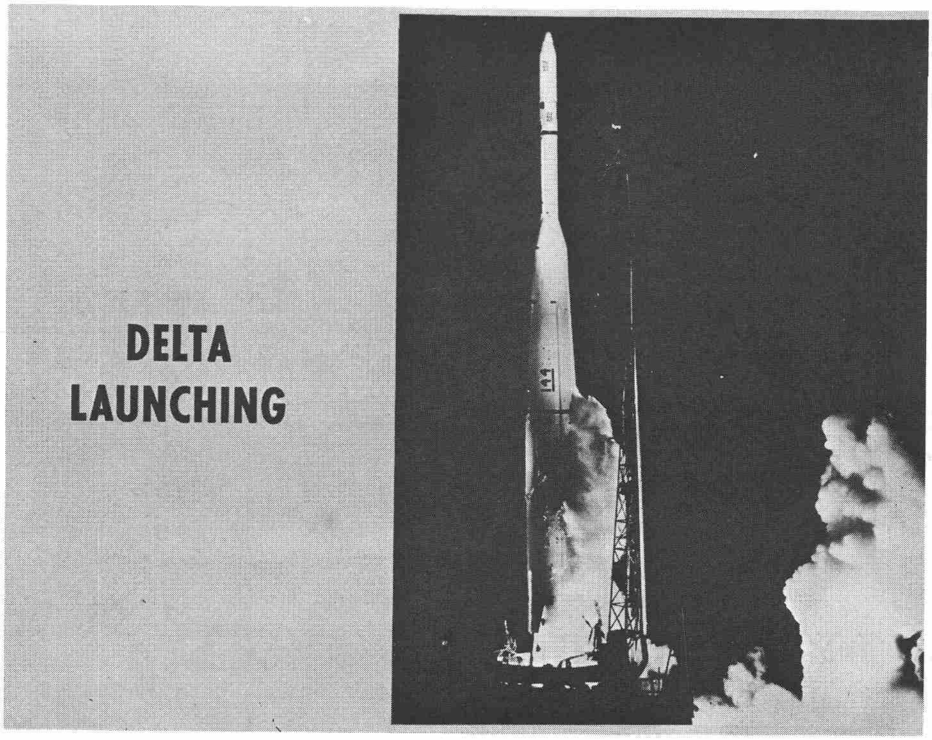
Figure 3



SCOUT MISSIONS

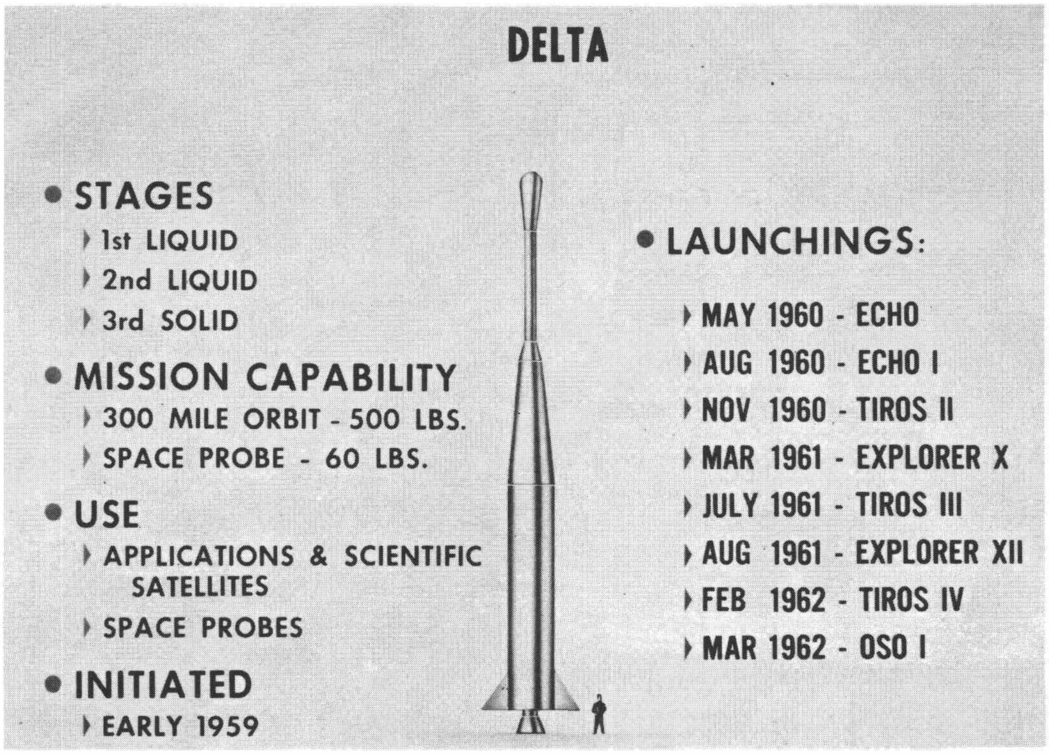
● SCIENTIFIC SATELLITES		
▸ EXPLORER IX		1961
▸ EXPLORER XIII		1961
▸ MICROMETEORITE	(4)	1961-1963
▸ IONOSPHERE	(4)	1961-1964
▸ MATERIALS	(2)	1962-1964
● PROBES		
▸ RE-ENTRY HEATING	(6)	1962-1964
▸ ELECTRIC ENGINES	(4)	1962-1964
▸ LIFE SCIENCE	(2)	1963-1964
● INTERNATIONAL SATELLITES		
▸ U.K. NO. 2		1963

Figure 4



DELTA LAUNCHING

Figure 5

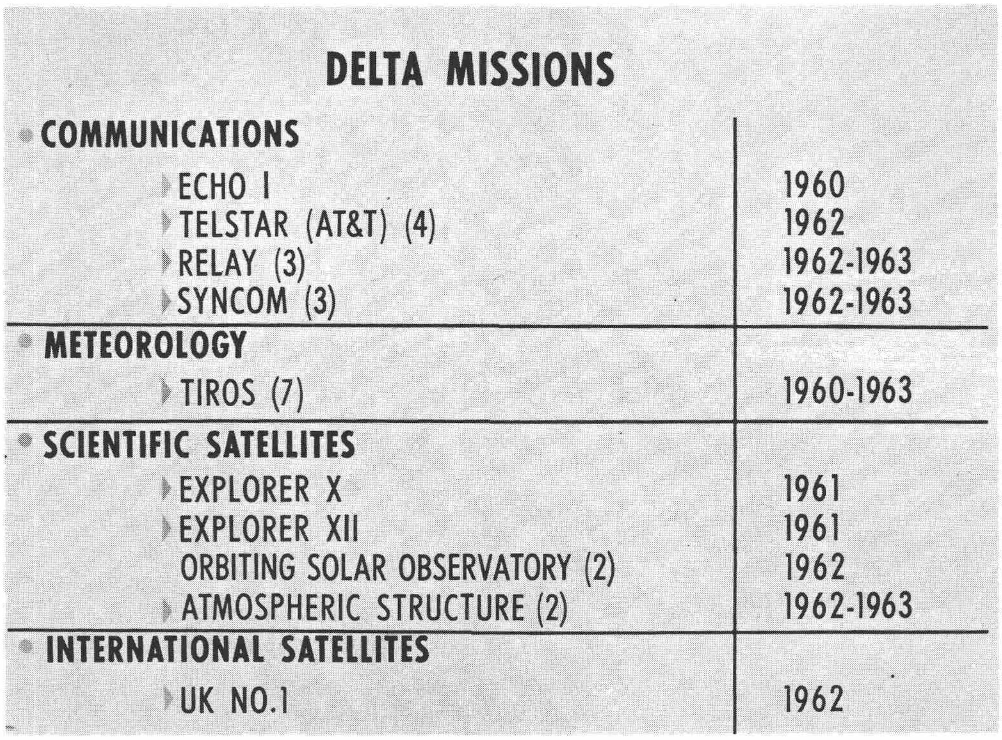


DELTA

- STAGES
  - 1st LIQUID
  - 2nd LIQUID
  - 3rd SOLID
- MISSION CAPABILITY
  - 300 MILE ORBIT - 500 LBS.
  - SPACE PROBE - 60 LBS.
- USE
  - APPLICATIONS & SCIENTIFIC SATELLITES
  - SPACE PROBES
- INITIATED
  - EARLY 1959

- LAUNCHINGS:
  - MAY 1960 - ECHO
  - AUG 1960 - ECHO I
  - NOV 1960 - TIROS II
  - MAR 1961 - EXPLORER X
  - JULY 1961 - TIROS III
  - AUG 1961 - EXPLORER XII
  - FEB 1962 - TIROS IV
  - MAR 1962 - OSO I

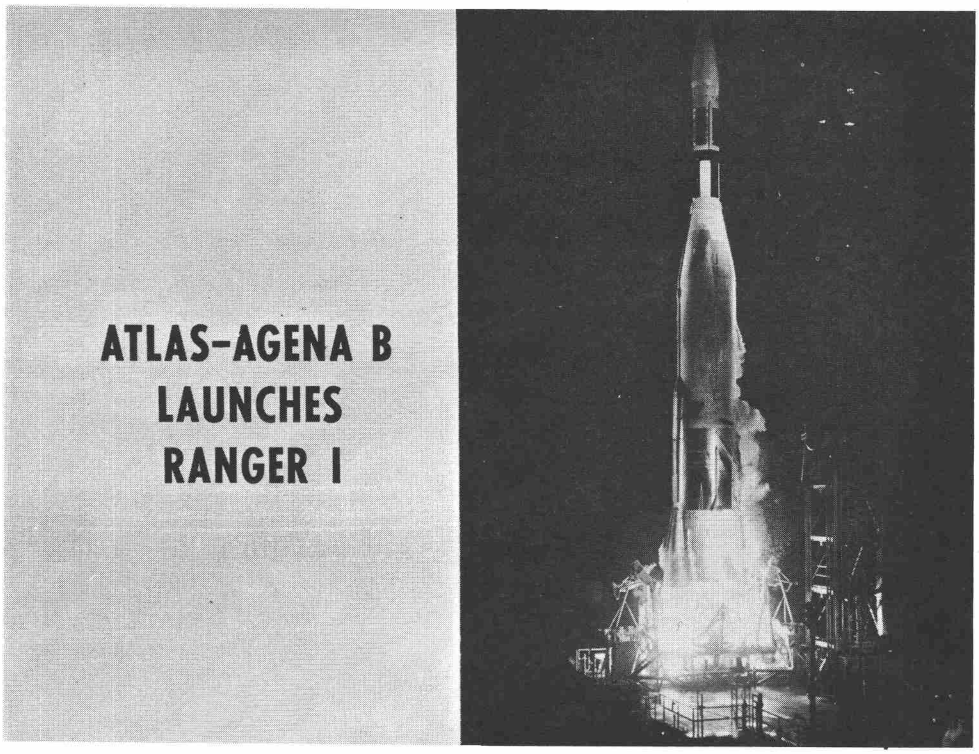
Figure 6



DELTA MISSIONS

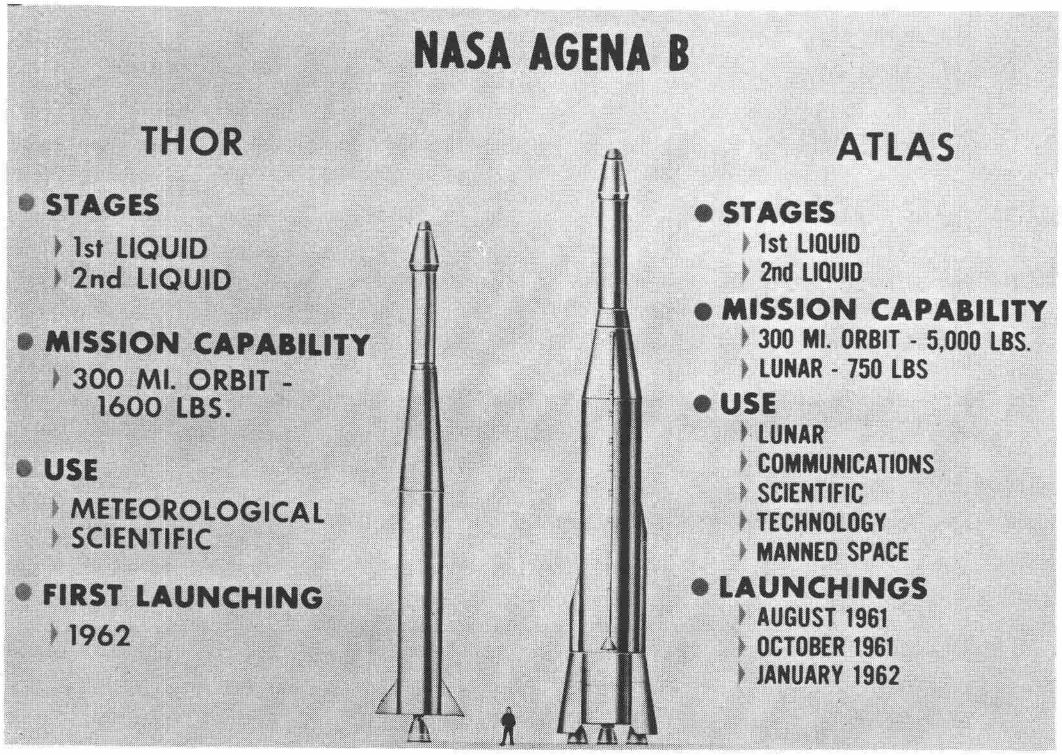
COMMUNICATIONS	
ECHO I	1960
TELSTAR (AT&T) (4)	1962
RELAY (3)	1962-1963
SYNCOM (3)	1962-1963
METEOROLOGY	
TIROS (7)	1960-1963
SCIENTIFIC SATELLITES	
EXPLORER X	1961
EXPLORER XII	1961
ORBITING SOLAR OBSERVATORY (2)	1962
ATMOSPHERIC STRUCTURE (2)	1962-1963
INTERNATIONAL SATELLITES	
UK NO.1	1962

Figure 7



ATLAS-AGENA B
LAUNCHES
RANGER I

Figure 8



NASA AGENA B

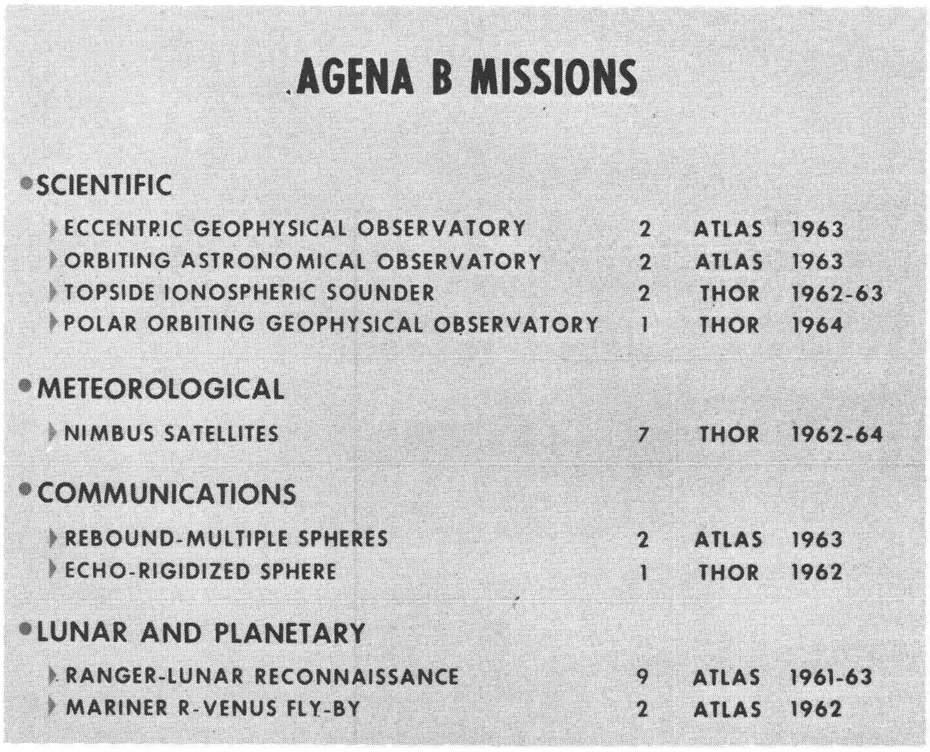
THOR

- **STAGES**
  - 1st LIQUID
  - 2nd LIQUID
- **MISSION CAPABILITY**
  - 300 MI. ORBIT - 1600 LBS.
- **USE**
  - METEOROLOGICAL
  - SCIENTIFIC
- **FIRST LAUNCHING**
  - 1962

ATLAS

- **STAGES**
  - 1st LIQUID
  - 2nd LIQUID
- **MISSION CAPABILITY**
  - 300 MI. ORBIT - 5,000 LBS.
  - LUNAR - 750 LBS
- **USE**
  - LUNAR
  - COMMUNICATIONS
  - SCIENTIFIC
  - TECHNOLOGY
  - MANNED SPACE
- **LAUNCHINGS**
  - AUGUST 1961
  - OCTOBER 1961
  - JANUARY 1962

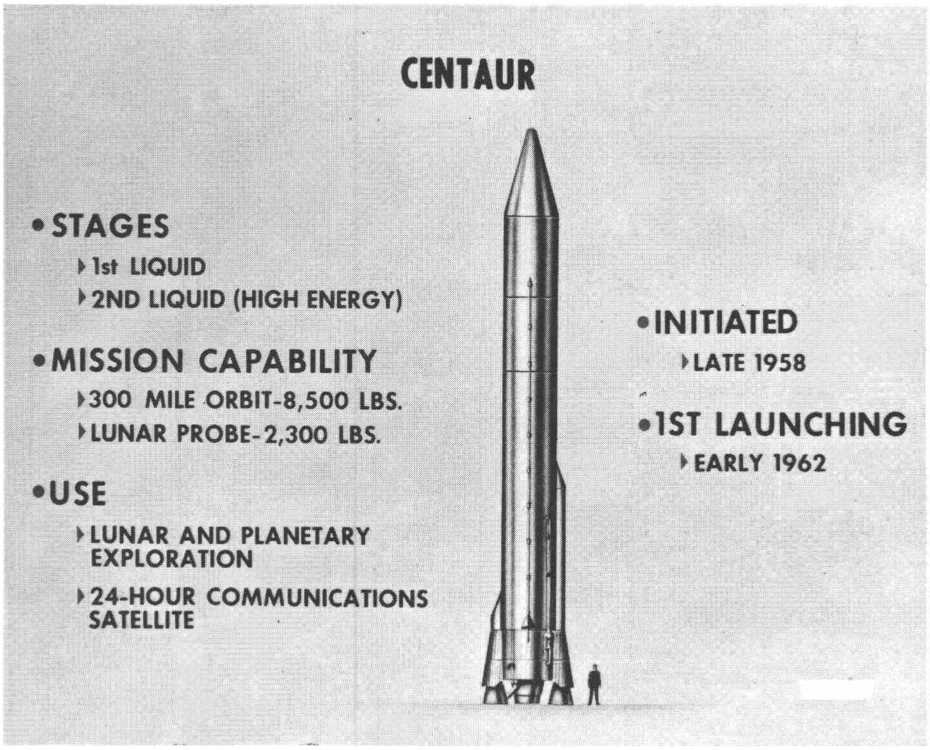
Figure 9



AGENA B MISSIONS

SCIENTIFIC			
ECCENTRIC GEOPHYSICAL OBSERVATORY	2	ATLAS	1963
ORBITING ASTRONOMICAL OBSERVATORY	2	ATLAS	1963
TOPSIDE IONOSPHERIC SOUNDER	2	THOR	1962-63
POLAR ORBITING GEOPHYSICAL OBSERVATORY	1	THOR	1964
METEOROLOGICAL			
NIMBUS SATELLITES	7	THOR	1962-64
COMMUNICATIONS			
REBOUND-MULTIPLE SPHERES	2	ATLAS	1963
ECHO-RIGIDIZED SPHERE	1	THOR	1962
LUNAR AND PLANETARY			
RANGER-LUNAR RECONNAISSANCE	9	ATLAS	1961-63
MARINER R-VENUS FLY-BY	2	ATLAS	1962

Figure 10



CENTAUR

- STAGES
  - 1st LIQUID
  - 2ND LIQUID (HIGH ENERGY)
- MISSION CAPABILITY
  - 300 MILE ORBIT-8,500 LBS.
  - LUNAR PROBE-2,300 LBS.
- USE
  - LUNAR AND PLANETARY EXPLORATION
  - 24-HOUR COMMUNICATIONS SATELLITE
- INITIATED
  - LATE 1958
- 1ST LAUNCHING
  - EARLY 1962

Figure 11

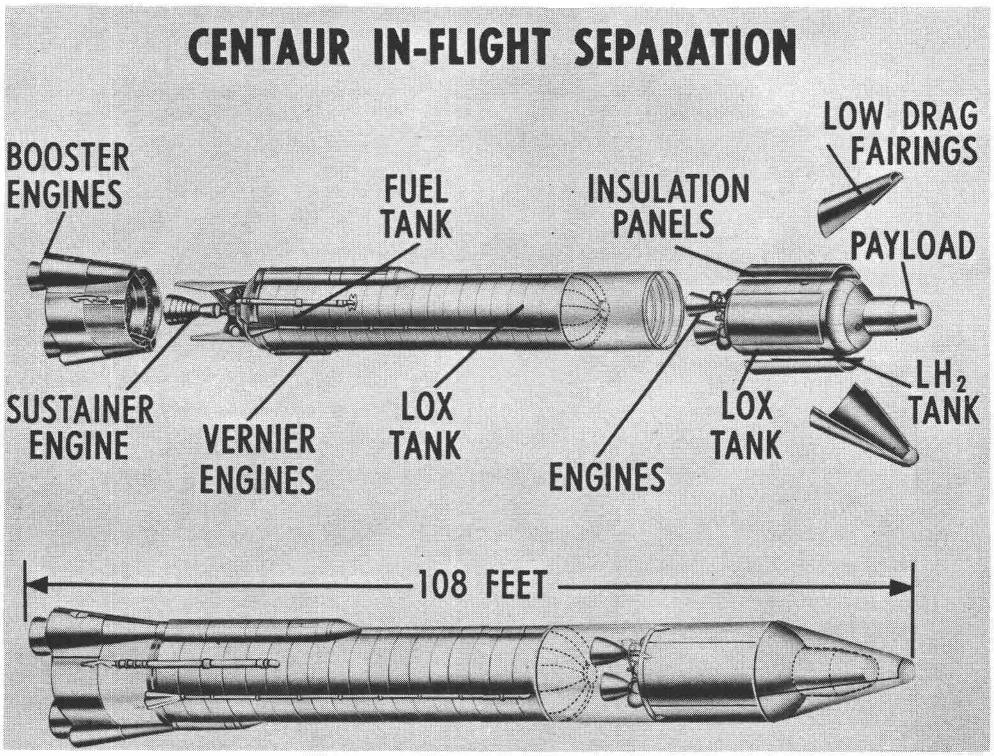


Figure 12

U. S. LAUNCH SITES

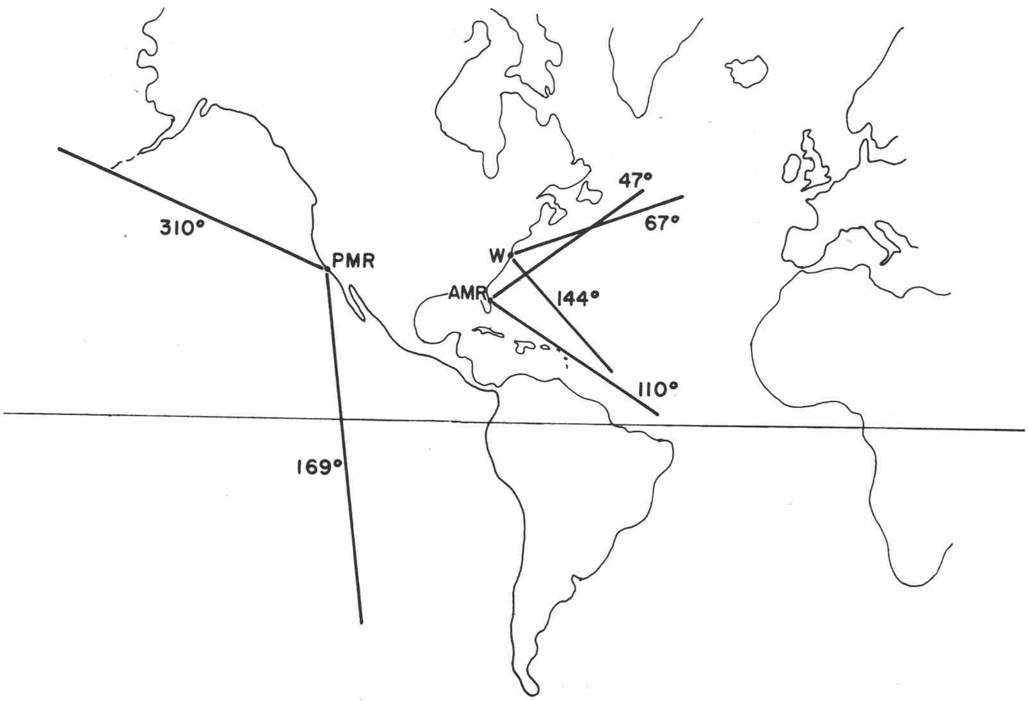


Figure 13

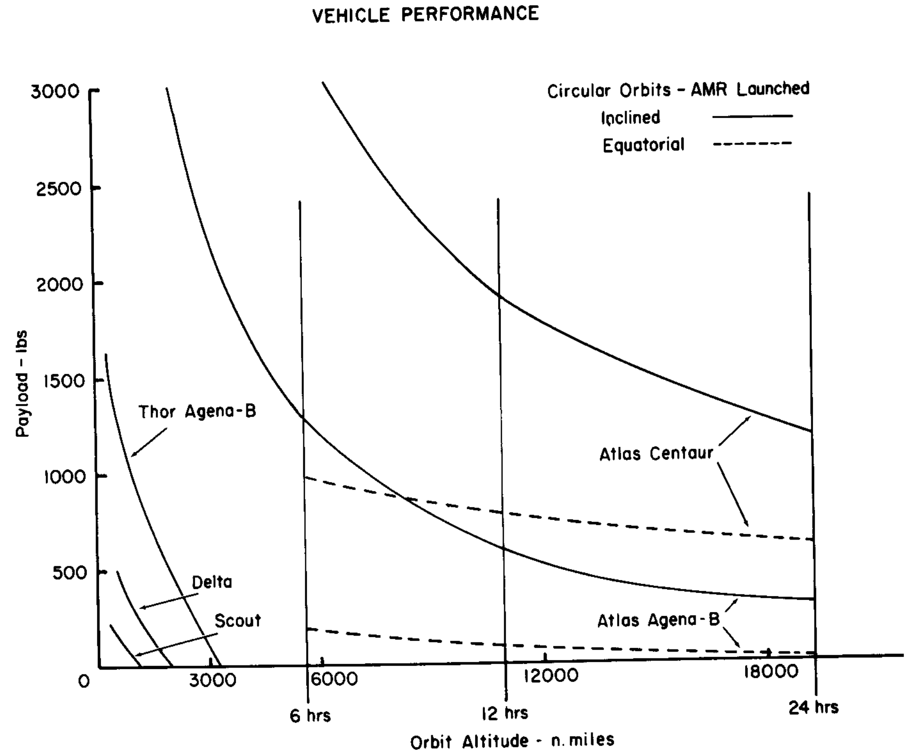

VEHICLE PERFORMANCE

Figure 14

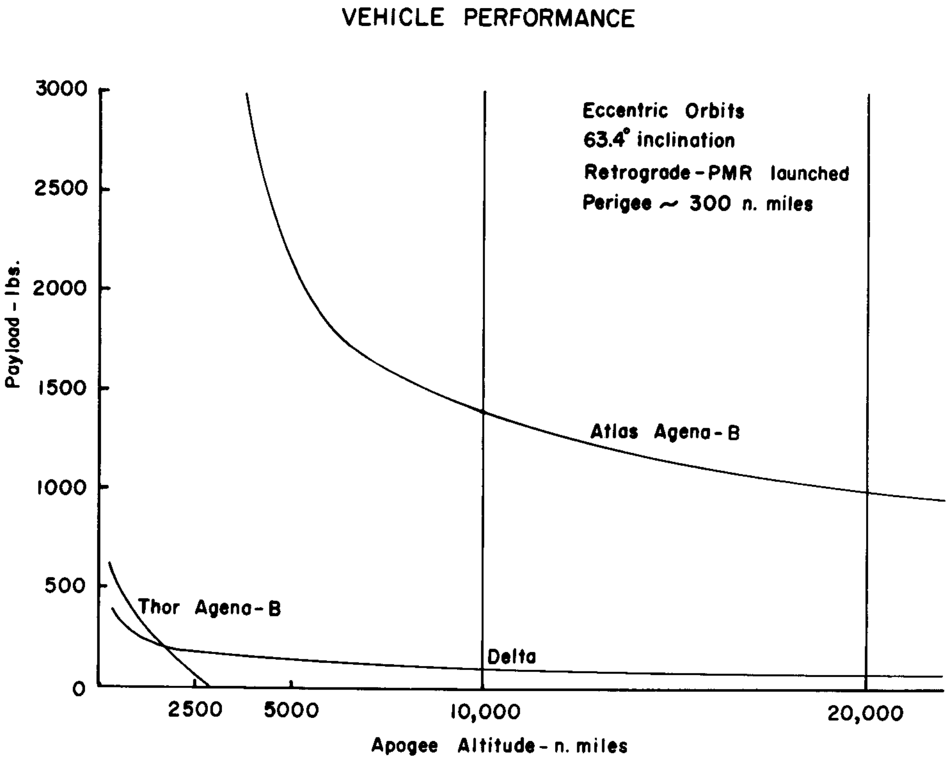

VEHICLE PERFORMANCE

Figure 15

GEOPHYSICS AND ASTRONOMY PROGRAM

By John E. Naugle

Introduction

The NASA Space Science Program is divided into three major scientific programs: the Bioscience Program, the Lunar and Planetary Program, and the Geophysics and Astronomy Program. This paper concerns the Geophysics and Astronomy Program.

The fundamental mission of the Geophysics and Astronomy Program is to learn more about the Earth and its space environment, to understand solar-terrestrial relationships, and to understand the nature and the origin of the solar system and the universe. A secondary purpose is to provide needed information and support of both civilian and military requirements in space. We use balloons, sounding rockets, and satellites to carry the instruments into space to accomplish these objectives. So far we have used spacecraft which remained attached to the Earth, whereas the Lunar and Planetary Program uses spacecraft which are designed to escape from the Earth to encounter, orbit, or land on other bodies in the solar systems.

The accomplishment of these missions requires large amounts of funds and resources. Difficult decisions have to be made. The following factors are involved in an understanding of the problems: how the program is planned; what resources are required; who participates in the program; how it is carried out; and what the criteria are which we use to determine if the program is progressing satisfactorily; the time required to design and build a new spacecraft.

About a third of this paper will be devoted to a discussion of the mechanics of a Program Office: what its function is; the people involved; the size of our budget and what we do with it; how we make some of these difficult decisions; and how the program is carried out.

The second third of the paper will be a discussion of our scientific program, its objectives, the experiments and flight program underway, and what we hope to achieve with the considerable resources which are made available.

The final third of the paper will be devoted to specific problems which we have or which I see in connection with this program.

Responsibilities of a Program Office

There are three primary responsibilities of a Headquarters Program Office: (1) formulation of a continuing program; (2) technical direction of the approved program; (3) continuing evaluation of the progress of the program.

Formulation of the Program

The program is formulated by scientific discipline. It is executed in various spacecraft and the scientific program will be discussed in terms of the flight missions; however, it should be emphasized that the program is planned by scientific discipline.

Figure 1 shows the breakdown of the Office of Geophysics and Astronomy. For administrative reasons it is divided into four areas, Astronomy and Solar Physics, Chemistry, Physics, and Flight Systems. We have, we think, broken the science into disciplines which can be handled by a single individual, the head of the program. This individual should know the workers in this discipline; he should know the problems in the discipline, the merits of the relative methods of attempting to solve these problems, and he should have some stature in the field. There is a considerable amount of responsibility and authority centered here. This is the level at which decisions are made with regard to which university programs are funded and how much the funding should be. These program heads are responsible for judging whether a particular project or a university or a NASA center is progressing satisfactorily. We like to have these positions filled with mature, able, creative scientists. Unfortunately, most mature, able, creative scientists are not particularly interested in administering the programs of other scientists. It is difficult to find the proper scientists to fill these positions. In addition to being able and creative, these program heads should be honest in the scientific and fiscal sense. They are the people to whom a university scientist comes to discuss support for new ideas or new experiments. They each direct the spending of a considerable sum of money each year. They have to be loyal to the scientific fraternity, because this is really the only reason a competent scientist has for taking a job like this. He has to feel that the contribution which he can make to science in such a job is worth giving up some of his own work. In order to obtain competent people for these jobs, we are, if necessary, hiring people for a one-year tour of duty. In order for them to make the maximum use of their year at NASA, we are using people who have served a year on a NASA Subcommittee prior to coming to work full time. This enables them to learn what the program is and how it operates; therefore, they are able to take over management of the program as soon as they arrive. So far, the only area in which we are doing this is in the area of ionospheric physics. Dr. Erwin Schmerling is on leave from Penn State for a year to act as Head of the Ionospheric Physics Program.

These people are responsible for the formulation of the programs in their area. How do they do this? The ideas for experiments and the concepts to be tested are born in the minds of individuals. It is the role of a program head to assemble these ideas and individual programs or projects into something resembling a coherent program and to estimate the resources required to carry it out. The program is assembled from proposals and suggestions which come primarily from:

1. Individual scientists at universities and NASA field centers

2. Scientific Advisory Groups, the Space Science Board, and the seven discipline subcommittees

3. Technical requirements for environmental data

4. Technological developments

5. The Head of the Program

The form of these proposals may vary from a complete program to launch several satellites for a particular set of measurements which may only require approval, to a suggestion for some measurement by a theoretical physicist in which it is necessary for a program head to interest someone in doing the experiment, to interest a NASA center in undertaking the management of the project, and to convince his management at NASA Headquarters that this is a worthwhile project deserving of hard-to-get funds and resources. In order to formulate and carry out a successful program this person finds that he must have the confidence and cooperation of three major groups: the scientific community, the NASA centers which execute the program, and the NASA management who must obtain the funds and resources from Congress and who have the final authority and responsibility for the use of these funds and resources.

The head of a program is responsible for planning a program which will not only obtain the maximum scientific results with currently available resources, but which will also provide a healthy scientific effort five and 10 years from now. He must conduct a program which insures a continuing influx of young creative scientists. He must recognize, now, for instance, that there will be a need for a vast coordinated program of solar physics at the next solar maximum and he must plan, cajole, and connive with these three groups to insure that the proper launch vehicles and spacecraft are developed, that the new instruments are perfected, and that the funds and resources are available.

Each year all the proposed and ongoing Geophysics and Astronomy Programs are reviewed and decisions are made as to what the total program should be. Some projects may have to be deferred because the launch vehicles are not ready. Others may have to be deferred because the budget guidelines within which we have to work do not permit all the projects which we would like to start. A final Geophysics and Astronomy Program is recommended to Dr. Newell who in turn must weigh the relative merits of all the Programs in the Office of Space Sciences and decide what the total Science Program will be. After further reviews within NASA and presentations before Congress, a final program is approved and the necessary funds and resources are allocated to accomplish these approved programs.

Technical Direction of Approved Programs

How is the program carried out? Who makes the day-to-day technical decisions? There are three major groups which carry out the program:

1. Universities. The universities participate in the program in two ways. We provide funds to universities to carry out a program of advanced research. With these funds, data from previous experiments are analyzed; laboratory and theoretical studies are carried out. New experiments are conceived and developed to a breadboard stage. Students participate and receive training in space sciences. These programs are funded under contracts or grants and in some cases under a level-of-effort grant extending for three years. The major share of the contracts and grants including all the level-of-effort grants are under the technical direction of a Program

Head. For some contracts for a specific task or for a new group who have not participated in the program, he may follow the work quite closely. In the case of the level-of-effort grants there is only an agreement between the Head of the Program and the Principal Investigator as to the general area of research, with the over-all technical direction of the effort delegated to the Principal Investigator.

Universities participate by providing experiments for spacecraft. The design, development, and construction of a breadboard model of the experiment is carried out under a contract or grant from Headquarters. If the experiment is good, the experimenter will want to fly it on a spacecraft. To do so, he submits a proposal to NASA. This proposal is reviewed by the appropriate subcommittee and by the Program Office involved. The recommendations are forwarded to the Space Science Steering Committee which makes the final recommendation to Dr. Newell of all experiments for all spacecraft in the Space Science Program. If the experiment is selected then a number of prototype and flight units must be constructed, calibrated, and tested. Since these must be constructed to meet the specifications and schedules of the spacecraft involved, the contract and technical monitoring is handled by the center responsible for the spacecraft. Some universities prefer to manufacture the hardware themselves; others may elect to subcontract this work to industry. In either case, the Principal Investigator is responsible for the successful operation of his experiment. If the mission is successful, the data are given to him and he analyzes and publishes the data. He has the exclusive use of the data for a certain mutually agreed upon period of time after he receives it. It is then his responsibility to analyze, interpret, and publish the results of the experiment.

The funding and detailed technical management of the contract for the production of this flight hardware is handled by the center (usually the Goddard Space Flight Center) responsible for the management of the spacecraft project.

2. Goddard Space Flight Center. This is the center which is responsible for the technical management of almost all the projects in the Geophysics and Astronomy Programs. A project may be as simple as a single sounding rocket flight or as complex as the development of an Orbiting Astronomical Observatory. This center is responsible for the design and manufacture of a spacecraft in-house or for the preparation of the specifications for the selection of an outside contractor to build the spacecraft. Goddard is responsible for the checkout of the spacecraft, for tracking of the satellite, for acquisition of the data, and for transmission of the data to the experimenters.

Scientists at this center also contribute experiments for spacecraft. They submit proposals for review and for selection by the Space Science Steering Committee just as other university groups do. These scientists also contribute very substantially to the success of the over-all program. They work with the project engineers and spacecraft designers to insure that the spacecraft are designed to meet the objectives of the scientific experiments. Project scientists serve as a communications link between the project engineers and the scientists, both university and in-house. By working closely with project people they are able to understand and interpret to the scientists the necessary constraints which must be placed on their experiments to insure the over-all success of the mission. By being scientists, and by working closely with the experimenters on a spacecraft, they are able to explain to the project people the constraints which a particular experiment must place on the spacecraft.

We at NASA feel that a strong university scientific program is necessary to insure a successful program. We feel just as strongly that a strong in-house program is necessary to insure a successful and continuing program. We recognize that there will be continuing friction and competition between the two groups. Scientists outside of NASA feel that the in-house experimenters have the inside track on payload space. The in-house experimenters feel that too much of their time and effort are taken up working with the outside experimenters and that too little recognition is given to their contribution to the success of the mission. We think the competition is a very good thing. We deplore the friction and try to keep it to a minimum.

3. Industry. The third major contributor to the program is industry. Industrial organizations participate in the program in every way from supplying electronic parts, to the design, manufacture, testing, and the integration of experiments into a spacecraft. In all cases this work is done under the direction of a project manager at one of the NASA field centers. For the Geophysics and Astronomy Programs, all the programs are managed by the Goddard Space Flight Center.

Evaluation of Program Progress

The third major function of the Program Office is to perform a continuing evaluation of the progress of the program.

What are the criteria by which we judge whether a project is progressing? In the case of the hardware, the spacecraft, there is really only one criterion and that is a harsh one—success. In the scientific program we are not developing vehicles; therefore, although we may learn from our failures, there is no logical mechanism by which a failure can be turned into a success. This is the criterion by which the project people operate.

In the case of the scientific experiments, the same criterion must be applied. This means that an experiment for a spacecraft must be built and tested to very exacting specifications. Here again there may be friction and misunderstanding between a scientist new to the space program and the project people. He may feel that some of the tests are unnecessary. However, if the Project Manager fails to insist on the proper tests and the experiment fails, then the manager has not done any favor to the experimenter.

An experimenter is given the exclusive use of the data from his experiment for a certain maximum length of time after he receives them. He is expected to publish the data in a reputable scientific journal in a timely fashion. After this time (which is a time mutually agreed upon between the Office of Space Sciences and the experimenter) the data are available for the use of other interested scientists.

We expect that a University Program, in addition to providing experiments which will yield significant results, will also provide training for graduate students. Therefore, one of the criteria by which we evaluate the progress of a University Program is the number of good students which graduate per year.

Resources

The following brief discussion concerns the funds and resources which are available and how these are used.

Figure 2 shows the total budget for the program for 1961 and 1962 and the amount we have requested for 1963. A major share of this money is used for salaries, vehicles, and spacecraft. Roughly 75 per cent of the budget is used in support of the experiments and research which is the purpose of the program.

You will note that the budget has been increasing almost linearly for these three years. This has been due to an increasing number of participants in the program and increasing costs and numbers of spacecraft which have been launched. Roughly 20, 70, and 100 experiments were flown or will be flown in this program in 1962, 1963, and 1964, respectively. We have tried to estimate the number of scientists participating in the program. This is extremely difficult to do. There are about 40 universities, 10 Government, and 5 industry laboratories participating in the program and we estimate that there are about 150 scientists participating. Thus, it costs about $1 million per scientist per year to support this program.

Where does the relatively small fraction of this budget marked research go? Figure 3 shows the breakdown of this item. The major increase in the in-house effort between FY 1961 and FY 1962 reflects the coming into being of GSFC. This is the amount of money which is spent for research and development. This is the money that is used to develop new experiments, to analyze and interpret the data from experiments, and for laboratory, balloon, and rocket research. The amount of money allocated to university and to in-house effort reflects our intention to keep these as dominant and roughly equivalent elements in the research program.

Figure 4 shows the breakdown of the money which is spent for experiments. This is the money which is used to buy the actual experimental hardware which flies on a spacecraft. The rather large incremental increase in this budget between FY 1962 and FY 1963 reflects the funding for the large observatories. Here, again, in-house and university effort are roughly comparable.

Spacecraft and Launch Vehicles

The major share of our resources goes into the purchase of spacecraft and launch vehicles. What type of spacecraft do we use, how much do they cost, and how long does it take from conception of a particular experiment until it is launched?

We use three major kinds or sizes of spacecraft—sounding rockets and geoprobes, the small Explorer and Monitoring satellites, and the large Observatories.

Figure 5 shows the sounding rockets which are used in the program, together with their payload capabilities, and approximate cost of the vehicles alone, that is, not including payload, support, and so forth. At present Nike-Cajun rockets can be launched at Wallops Island, White Sands, Ft. Churchill, and Norway. We have a cooperative program with India to establish a Nike-Cajun launcher on the geomagnetic equator. Table I shows the length of time required to initiate a sounding rocket program and the range of costs.

TABLE I

Flight Systems Development
Typical Sounding Rocket

Time	Capability of short reaction times from conception to launch due to availability of standard hardware: Normally 2 to 12 months
NASA manpower	1 to 20 man years
Funding	10,000 to 1,000,000

The Explorers are a small, 100-pound class of satellites launched mainly on the Delta, with a few of the Scout and the Thor-Agena. Figure 6 shows some of these satellites. The appendix contains a description of all the Monitors and Explorers which have been launched since 1 January 1961, together with those underway or planned for future launches. Figure 7 shows the length of time required to initiate a type of monitor, the Interplanetary Monitoring Probe. The need for such a satellite was first noted early in 1961 after the successful launch of Explorer X. Goddard prepared a preliminary design in the fall of 1961. Final approval was obtained at the end of the year and first launch is scheduled in mid-1963. Thus, it will take roughly two years from conception to birth for Imp. Three launches on a Delta with an elliptical orbit with a 300,000-kilometer apogee are scheduled. The total cost of the program is about $12 million.

Figure 8 shows the three observatories used in the program. Data are being analyzed from the first Orbiting Solar Observatory, Oso. Oso is launched with a Delta vehicle. Twenty experimenters, a project team, and the Space Technology Laboratory are all hard at work on the first Orbiting Geophysical Observatory, Ogo. The first launch, on an Atlas-Agena, is scheduled for mid-1963. The most complex and costly observatory is the Orbiting Astronomical Observatory, OAO. Figure 9 shows the timetable for OAO. The first efforts to get this program underway began in 1958. The first launch, on an Atlas-Agena, is scheduled for 1964. The gestation period for an OAO is about six years. Figure 10 shows the costs associated with this program. Study contracts are being let for an advanced Oso.

Clearly, these observatories are extremely complex and costly. They are also the only way certain experiments can be conducted. As we bring these monsters into being we will keep a substantial Explorer program underway to insure that we continue to obtain significant results in case of failures or delays with the large observatories.

Geophysics and Astronomy—Flight Program

The general objectives of the Geophysics and Astronomy Program have already been given. It is good to have general objectives for the program but the experiments and the Flight Program determine the results which are obtained.

Figure 11 shows the Flight Program for 1961 through 1965. A detailed discussion of each spacecraft and the experimenters and their experiments is given in the appendix. Three major considerations affect the program. One, of course, is the resources available; the second is the spacecraft technology available; and the third, particularly in geophysics and solar physics, is the phase of the solar cycle we are in. At present we are planning and flying experiments aimed at studying phenomena at solar minimum. We have a series of two satellites in highly eccentric orbit to study the magnetic fields and trapped radiation in the magnetosphere. The first Ego, in 1963, will continue this work; in addition, it will make possible much more detailed studies of the energy spectra composition and angular distribution of the trapped radiation and also make possible correlation between changes in the population of the charged particles and fluctuations of the geomagnetic field. The Topside Sounders which will be used to study the topside of ionosphere are S-27 and S-48. The S-6 is an Atmospheric Structure Satellite. Imp will spend the majority of the time in interplanetary space measuring the plasma, the magnetic field, and the energetic particles there. It will also be used to study the boundary between the geomagnetic field and the interplanetary field. Pogo will be used for aurora studies, airglow, ionospheric physics, and energetic particle studies. As a result of the success of Oso we are scheduling these observatories on a regular schedule of one every six months in order to insure that the Sun is continuously monitored during International Quiet Solar Year.

Topics for Consideration by the Space Science Summer Study

Consideration of the Long Range Scientific Goals in Space Science. We think that there will be fundamental scientific discoveries in space. We think that there will be measurements made which cannot be explained by the use of the existing physical laws and that new laws will have to be devised. We regard the program in space science research as extending measurements onto a large scale just as the invention of the electron microscope has enabled man to study nature on a very small scale. Carrying telescopes above the atmosphere enables astronomers to study over a much larger and very interesting wavelength region. I have already mentioned the need for a major coordinated effort at next solar maximum to study the solar system. This certainly should be a joint project among all the nations with capability of making measurements in space. One envisions, during this time, probes transmitting simultaneously from orbits near the Sun and from orbits far out in the solar system. One could envision a solar observatory launched into an orbit so that it would observe the invisible side of the Sun so that we would be able to track the development of the sun spots during their entire life. This would also provide warning of solar activity from sun spot groups coming from around the limb of the Sun. One of the questions which it is not too early to consider would be the optimum use which could be made of spacecraft during the next period of maximum solar activity. Preliminary studies are underway to determine what hardware will have to be developed for a probe to go in close to the Sun. Studies will have to be started on the configuration of a spacecraft which will go out several astronomical units from the Sun. It will not be possible to do everything which is conceived of between now and next solar maximum. Priorities will have to be assigned to certain missions.

Doctrine for the competitive selection of experimenters and experiments for specific missions. Experimenters for a specific mission are selected as follows. A letter is sent out to all the competent scientists in the various discipline areas inviting them to submit proposals for a spacecraft such as the Polar Orbiting Geophysical Observatory. Experimenters then send in a proposed experiment. They state the scientific objectives and the technique by which they hope to achieve these objectives. These proposals are then distributed to the discipline subcommittees concerned. These groups read, discuss, evaluate, and place the experiments in one of four categories. The first category is those experiments which should be flown on the spacecraft concerned. The second category is reserved for old reliable experiments which are available and ready to fly but which have potentially been replaced by new experiments. The third category is for experiments which have not been developed to a point where there is a reasonable chance of them meeting deadlines. The fourth category is for experiments which are not suitable for the mission. The experiments for each subcommittee, which were placed in category 1, are then reviewed in Headquarters and a decision made as to which of those from each discipline will be finally considered for the spacecraft. For an Ogo with 150 pounds of payload available, about 225 pounds of experiments will be placed in category 1. Funding is then made available to these experimenters to develop their experiment to a breadboard stage. Some three to five months later a meeting is held of all the category 1 experimenters. Each experimenter spends about 15 or 20 minutes discussing his experiment before the other experimenters. Each experimenter is free to question the other experimenters. At this time each experimenter should have an accurate estimate of the weight, power, and telemetry requirements. At the conclusion of this meeting, the Space Science Steering Committee recommends the final experiments for the spacecraft. If, as in the case of an Orbiting Geophysical Observatory, the total payload capability is 150 pounds, then approximately 225 pounds of experiments are chosen. The center responsible for the integration of the spacecraft, usually the Goddard Space Flight Center, is then notified of the experimenters which have been chosen and the center then contacts the experimenters to provide funds and necessary specifications so that the experimenters can design and build the flight hardware.

Proper relative roles of NASA laboratories and those in universities and independent research foundations and industry. We recognize that this is a continuing problem. Outside experimenters in universities feel that scientists at NASA laboratories have an advantage. The scientists in the NASA laboratories feel that if they have to undertake to help in the design and management of these spacecraft that they should certainly have space for their experiments. They feel that the outside experimenters are not properly appreciative of the contributions which they make to the success of the spacecraft. We feel that both groups are essential to the conduct of the program. We feel that university scientists are needed because many of the best ideas originate there. In order to have a continuing program, there must be a continuous supply of good graduate students into the space program. We think that the best way to insure this is to have university professors participating in the program.

It has already been noted that it costs about $1 million per year per scientist in this program. Table II shows the number of repeat and the total number of experiments which have been available on all the spacecraft. The dashed line shows that the experiments for spacecraft in 1964 are largely chosen. Note that there is not a large number of experiments in the program. Also note that the total number of experiments in 1964 is 94, and our estimated number for 1966 is only 151 or a 50 per cent increase. We cannot encourage a large increase in the number of people participating in the program unless there is also a proportional increase in the funds and resources required to provide spacecraft to carry their experiments.

TABLE II

Experiment Space Available

	New	Repeat	Total
1962	12	10	22
1963	34	38	72
1964	61	33	94
1965	46	67	113
1966	93	58	151

APPENDIX

GEOPHYSICS AND ASTRONOMY FLIGHT PROGRAM

Introduction

This appendix presents a description of the flight program, of each spacecraft, and of the experiments aboard.

Explorers, Monitors, and Geoprobes

The Explorers and Monitors constitute two classes of artificial Earth satellites.

The Explorers, as the name indicates, are primarily satellites for exploration of the Earth's environment. They are satellites that weigh about 100 pounds, of the type that can be put into orbit by Delta and Thor-Agena launch vehicles. The problems of interest to the scientist making use of an Explorer are those connected with the ionosphere, the neutral atmosphere, the magnetic field in the neighborhood of the Earth, the radiation belts, and the energetic particles reaching the Earth from the Sun and from galactic space.

The Monitors are satellites of similar character to the Explorers insofar as size and launch vehicles are concerned. Their difference is primarily in the emphasis upon repeated measurements over a period of time of some quantities already known from earlier discoveries. As their name implies, they are monitors. The problems of interest to the scientist making use of a monitor are largely concerned with solar activity and its effect on magnetic fields, cosmic rays, and the Van Allen belt.

Brief descriptions of the Explorers, Monitors, and Geoprobes that were launched in 1961 and 1962 and those scheduled to be launched between 1962 and 1965 are given as follows:

Explorer IX (1961 Delta)

Explorer IX was launched 16 February 1961 from Wallops Island using a Scout launch vehicle. This marked the first use of Wallops Island for launching an artificial Earth satellite. The satellite was a small inflatable sphere of 12 feet (3.65 meters) in diameter. It was placed into an orbit with apogee of 1,605 miles (2,584 kilometers) and perigee of 395 miles (636 kilometers) with an inclination of 38.6 degrees. The weight of the sphere was only 15 pounds; therefore, it served as an admirable instrument for the observation of atmospheric drag. To enhance its usefulness over Echo I, it carried a radio beacon. This, however, failed and reliance was placed on visual observation by means of the Baker-Nunn cameras operated by the Smithsonian Astrophysical Observatory. Results from this simple satellite were excellent in that they were able to show the short-term variations in drag resulting from solar activity.

Explorer X (1961 Kappa)

This satellite was launched on 25 March 1961 using a Thor-Delta vehicle. In effect, it was a deep space probe and rose to an apogee of 145,000 miles, but its full orbit was not tracked. It carried as instrumentation a rubidium vapor magnetometer, two flux-gate magnetometers, a plasma probe, and an optical aspect sensor to give spacecraft position. The data recovered are permitting studies of magnetic fields within and outside the Earth's magnetosphere.

Explorer XI (1961 Nu)

This was the Gamma Ray Astronomy Satellite launched 27 April 1961 using a Juno II vehicle. It went into orbit with an apogee of 1,108 miles (1,800 kilometers), perigee of 308 miles (490 kilometers), and an inclination of 28.8 degrees. The objective of this satellite was the detection of extraterrestrial high-energy gamma rays of the type that result from decay of neutral pi mesons. It carried an experiment designed to detect and map the direction and intensity of the galactic gamma rays above the Earth's atmosphere and to extend earlier balloon experiments. The experimentation has been done by W. L. Kraushaar of MIT.

The instrumentation consisted of a gamma-ray telescope and sensing devices. The telescope consisted of a sandwich of scintillation crystals and a Cerenkov detector contained in an anticoincident shield. Gamma rays could be differentiated from neutrons. Explorer XI on launch achieved a higher apogee than planned, and the orbit reached into the inner Van Allen belt. As a result the useful data were considerably less than had been anticipated. The data obtained are in the process of analysis and some preliminary results have already been reported by Kraushaar.

Explorer XII (1961 Upsilon)

The Energetic Particles Satellite was launched on 15 August 1961 into an orbit with perigee of 180 miles, apogee of 47,800 miles, and inclination of 33 degrees. It weighed 83 pounds. The highly eccentric orbit permitted measurements both out in interplanetary space and within the Earth's magnetosphere. The satellite spent about 90 per cent of the time in the Van Allen belt itself. The objectives of this satellite were to describe the protons and electrons trapped in the Van Allen radiation belt, to study the particles coming from the Sun, and to study cosmic radiation from outside the solar system.

This satellite carried seven experiments: (1) a proton analyzer to measure the proton flux and distribution of energies in the space beyond six earth radii; (2) a three-core flux-gate magnetometer sensitive to a few gammas; (3) a trapped radiation experiment with four Geiger counters and three CdS cells to measure the fluxes of particles from the Sun, galactic cosmic rays, and trapped Van Allen belt particles; (4) a cosmic-ray experiment to monitor the rays beyond the effects of the Earth's magnetic field that consisted of a double telescope detector of cosmic rays, a single crystal detector of energetic particles, and a Geiger-Mueller telescope for detecting cosmic rays; (5) a phototube coated with a powdered phosphur to serve as an ion electron detector to measure particle fluxes in the Van Allen belt and above; (6) solar cells to study the deterioration of protected and unprotected cells from exposure in the Van Allen belt; and (7) an optical aspect experiment to determine the orientation in space of the craft.

Explorer XIII (1961 Chi)

This satellite was launched on 25 August 1961, but failed to reach the planned orbit and, in consequence, had a lifetime of only 2-1/2 days. Its purpose was to study the effects of micrometeoroid or cosmic-dust collisions with spacecraft. The data collected during its 2-1/2 days were telemetered back during one 13-minute interval.

Explorer XIII essentially consisted of a group of experiments installed around the fourth stage of a Scout launch vehicle. Five types of micrometeoroid detectors were carried. One consisted of a battery of pressurized cells in which impact would release the pressure. The second consisted of foil gages to show micrometeoroid impact by change in resistance. In the third experiment a change of resistance of wire grids showed when a wire was broken by impact. Another detector used CdS photoelectric cells to transmit light through holes eroded in a thin opaque film. A fifth experiment reported mechanical impact on piezoelectric crystals.

Geoprobe (P-21)

This spacecraft was launched on 19 October 1961 to an altitude of 4,261 miles. Its total lifetime was somewhat less than two hours. The Electron Density Profile Probe, as it was called, was launched from Wallops Island, Virginia, with a Scout vehicle into a nearly vertical trajectory. It weighed about 90 pounds. Launching was done near midday at a time when the ionosphere appeared quiet. The payload was spin stabilized about its axis before the fourth-stage separation. The instrumentation carried consisted of: (1) a continuous wave (CW) propagation experiment to measure the ionosphere profile, (2) a radiofrequency (RF) probe experiment to measure the ionospheric electron density especially at altitudes above 600 miles where data are particularly scarce, and (3) a swept-frequency probe to provide information on the power absorbed by electron pressure waves. Two CW signals were transmitted to the ground, one at 12.3 megacycles per second and one at 73.6-mc/sec; they were controlled to an exact 6:1 ratio by use of a common crystal oscillator. The RF probe experiment was relatively new, and this particular unit served the dual purpose of providing information on electron densities in the ionosphere and information concerning the probe itself. Unfortunately, although the payload followed its planned trajectory, the CW propagation experiment was only partially successful because the disturbance created in the ionosphere by the rocket discharge interfered with the propagation of the 12.3-mc/sec signal.

The Ionosphere Beacon Satellite (S-45)

The orbiting of this satellite was first attempted unsuccessfully in the first quarter of 1961 and was then repeated in the second quarter, again unsuccessfully. The purpose of this satellite was to determine the general structure of the ionosphere and to determine the propagation characteristics of the ionosphere up to satellite altitudes as it applies to communication and UHF frequencies. The payload consisted of a transmitter giving rise to several frequencies, all harmonics of one basic frequency. Those selected were, in megacycles: 20.005, 40.010, 41.01025, 108.027, 360.09, and 960.240. The 108-megacycle frequency was to be used for tracking and telemetry. The frequencies were selected to provide sufficient frequency coverage to investigate the effects of the ionized region on the radio communications spectrum.

The payload was of the shape of two truncated cones mounted base to base separated by a short cylinder. The height was 24 inches and the diameter 30 inches. The total weight was about 70 pounds. The orbit selected was one with an apogee of 1,600 miles, a perigee of 200 to 250 miles, and an inclination of 51 degrees.

Swept-Frequency Topside Sounder (Canada) S-27

The primary objective of this satellite is to examine the structure of the ionosphere from above in a manner similar to that now being used by ground-based sounding stations. In particular, it is desired to obtain information about the ionosphere in the region above the maximum electron density of the F-layer, usually about 188 to 250 statute miles above the Earth's surface. The experiments are being developed by the Defense Research Telecommunications Establishment of Canada and the National Research Council of Canada. A secondary objective will be to measure the cosmic noise level in the 2- to 15-megacycle frequency spectrum.

The topside sounder will be carried in a satellite in a circular orbit inclined 78° from the equatorial plane, having an altitude of 700 miles. Launch will be with a Thor-Agena. The satellite instrumentation will provide downward transmission and echo reception over a frequency range of about two to 15 megacycles. The received data will be telemetered to Canadian, United States, and United Kingdom receiving sites since these nations are presently cooperating in this project. The Canadian experiment uses the swept-frequency technique of ionospheric sounding.

Fixed-Frequency Topside Sounder (US) - S-48

The primary objective of this satellite is to measure the electron density of the F_2 region, usually about 188 to 250 miles above the Earth's surface. It is further intended to study the variations of electron density distribution under varying magnetic and auroral conditions, and with time of day and latitude.

The Fixed-Frequency Topside Sounder will be carried by a Scout vehicle into a circular orbit inclined at 80 degrees at an altitude of 1,000 kilometers (about 600 miles). The satellite instrumentation will provide downward transmission towards the F_2 region of six fixed frequencies over a range of about two to 15 megacycles, and will receive the echoes. The data will be telemetered to ground stations along the 75th meridian and will be received by stations in the United States, Canada, and probably the United Kingdom. The instrumentation is being prepared by R. W. Knecht at the Central Radio Propagation Laboratory.

Geoprobe (P-21a)

This was the second electron density profile probe, called the Nighttime Electron Density Profile Probe. It, like P-21, was launched from Wallops Island on March 29, 1962 to a peak altitude of 3,900 miles. Its objectives and instrumentation were the same as those for P-21 with the difference that P-21a was intended to explore the nighttime ionosphere, and therefore was launched at 2:27 a.m., E.S.T. Care was taken to avoid the difficulty with transmission of the lower frequency signal that had been created by the rocket discharge of P-21. However, difficulty was again experienced with the CW transmission, this time caused by improper deployment of the antenna. Data were secured at 73.6 mc/sec relative to the Faraday rotation, but the 13.2 mc/sec signal was not usable. Excellent ion trap data were obtained, and the results are now in the process of analysis.

Ariel (S-51)

This is the International Satellite, UK No. 1. It marked the first time that the United States launched a satellite for another country, although it had previously flown experiments of individual foreign scientists. This spacecraft was launched 26 April 1962 by means of a Delta vehicle. It was put into an orbit with perigee of 242 miles, apogee of 754 miles, and inclination of 53.9 degrees. It weighed about 150 pounds. Both Great Britain and the United States appointed its own project manager, its own project coordinator, and its own scientists to work on the Ariel to expedite the work. Scientific information coming from the program will belong to the British scientists and will be made available to the world's scientific community in the same way as NASA's scientific results.

The Royal Society's British National Committee on Space Research was responsible for the choice of experiments proposed for the satellite, and in consultation with NASA, selected the actual experiments that were flown.

The experiments in Ariel were designed by three university groups in Great Britain. Dr. R. L. F. Boyd and a group at University College, London, designed experiments to measure: (1) electron temperature and density with a Langmuir probe, (2) ion mass composition and temperature with a spherical probe, (3) solar Lyman-Alpha emission in the ultraviolet, (4) solar X-ray emission in the three to

12 angstrom band in collaboration with the University of Leicester, and (5) solar aspect.

Professor J. Sayers and a group at the University of Birmingham designed the experiment to measure electron density by means of a radiofrequency plasma probe. This experiment determined the same quantity as Dr. Boyd's first experiment. The data from the two provide a cross check of the variation of electron density at heights between 200 and 600 miles.

Professor H. Elliott and a group at Imperial College, London, designed the experiment to measure the energy spectrum of the heavy cosmic rays.

Atmospheric Structure Satellite (S-6)

This satellite will be launched with a Delta vehicle into an orbit with a perigee of about 200 kilometers, an apogee of 1,200 kilometers and an inclination of 51 degrees. Its purpose will be, as its name indicates, to study the atmosphere. It will measure atmospheric density directly by means of gages designed at the Goddard Space Flight Center. It will measure electron temperature and density with equipment designed by N. W. Spencer of GSFC, and will study the neutral atmosphere composition with two mass spectrometers being developed by Consolidated Systems for Goddard Space Flight Center.

Energetic Particles Satellite (S-3a)

This satellite will monitor solar activity, cosmic-ray phenomena, and correlate energetic-particle activity with observations of the Earth's magnetic field. It will be launched into an orbit with a perigee of about 180 miles, an apogee of 47,800 miles, and inclination of 30 degrees.

The experimentation to be carried will consist of CdS detectors, with and without a magnetic broom for the study of the flux of electrons and protons in the region of trapped radiation. These experiments are under the direction of J. A. Van Allen of the State University of Iowa. He is also supplying an experiment using G-M tubes for the measurement of the electron energy spectrum and the proton flux. An experiment using a double scintillation counter to study galactic cosmic radiation has been designed by F. B. McDonald of Goddard Space Flight Center. He is also supplying a unit for the measurement of lower energy solar and galactic cosmic rays. An ion electron scintillation detector for flux and energy spectrum of trapped radiation has been designed by Leo Davis of Goddard Space Flight Center. Bader of Ames Research Center has supplied a plasma probe, and Cahill, formerly of the University of New Hampshire, a flux-gate magnetometer. A Geiger counter telescope for cosmic rays has been designed by McDonald of Goddard Space Flight Center.

Imp (Interplanetary Monitoring Probe) S-74, S-74a, S-74b

Imp is a spin-stabilized spacecraft, similar to Explorer XII, which will be launched by a modified Delta vehicle into an inclined eccentric orbit with a perigee of 120 miles and an apogee of 110,000 miles. The experiments aboard each will consist of a rubidium vapor magnetometer, two flux-gate magnetometers, two plasma probes covering the range from a few electron-volts to several Kev, two energetic particle detectors covering the energy range from 10 to 200 Mev, a third plasma probe covering the thermal energy range, and an ionization chamber and Geiger counter to monitor the total charged particle flux. Three spacecraft are scheduled and funded, two in 1963 and one in 1964. Under consideration are plans to continue this mission at the rate of one more launch in 1964 and two in 1965.

Polar Ionosphere Beacon Satellite (S-66)

The primary objectives of this satellite are: (1) to determine the total electron content in a vertical cross section between the satellite and the ground, as a function of latitude, season, and diurnal time; (2) to relate the gross behavior of the ionosphere to the solar radiation responsible for producing the ionization; and (3) to study in more detail the irregularities known to be present in the ionosphere and in radio wave propagation characteristics of the ionosphere as it applies to communications and UHF frequencies. Participating organizations will be CRPL, Stanford University, University of Illinois, and Pennsylvania State University.

This satellite will contain stable oscillators from which four harmonically related frequencies (20, 40, 41, and 360 megacycles), one minitrack tracking and telemetry frequency, and two Transit Network frequencies will be continuously emitted. The harmonically related frequencies will enable the vertical columnar electron density to be computed by using Doppler techniques. The Faraday effect will be investigated using the 40- and 41-megacycle frequencies. Ground stations will be equipped with special receivers that will enable these measurements to be made. The S-66 satellite will be launched with an inclination of 85 degrees from the Pacific Missile Range into a near circular orbit at an altitude of about 625 miles.

International Satellite (UK No. 2) - S-52

The objectives of the International Satellite UK No. 2, S-52, are to supplement the ion, electron, and radiation studies of the International Ionosphere Satellite UK No. 1, now called Ariel, by investigating certain phenomena in the atmosphere, the ionosphere, and beyond. The S-52 satellite will carry three experiments furnished by the United Kingdom. The galactic noise experiment is designed to record galactic noise in the 0.75- to 3.0-megacycle region. The ozone measurement experiment will measure the vertical distribution of ozone in the Earth's atmosphere. Quantitative measurement of particle flux is the objective of the micrometeoroid experiment. Participating organizations are Cambridge University, University of Manchester, and the Meteorological Office of the U. K. Air Ministry.

The second international spacecraft will be launched by a Scout vehicle into an Earth orbit having a nominal perigee of 370 kilometers (230 statute miles), an apogee of 1,850 kilometers (1,150 statute miles), and inclined 51 degrees from the equatorial plane.

The basic configuration will be that of project S-51 and will have an outside diameter of 23 inches by 35.5 inches long. The satellite will carry two booms to facilitate the erection of a long wire dipole antenna for the galactic noise experiment. There are four micrometeoroid detectors, all mounted near the satellite equator. The broadband ozone experiment (2,500 to 4,000 angstroms) will use two photocells with appropriate filters. The scanning ozone experiment uses a simple prism spectrometer to project a solar spectrum on a photomultiplier. Gross weight of the spacecraft is expected to be 165 pounds including separation mechanism.

Orbiting Solar Observatories

The Orbiting Solar Observatories (Oso) are a series of stabilized space platforms designed primarily for solar-oriented experiments. The objectives of these experiments are to provide continued observation of the Sun and the solar atmosphere in the X-ray, ultraviolet, and infrared regions of the spectrum. The first spacecraft designed for this purpose consisted of a rotating wheellike structure composed of nine wedge-shaped compartments which were connected to an upper fan-shaped stabilized section by an aluminum shaft. The oriented spacecraft was continuously pointed at the center of the Sun to an accuracy of ±1 minute of arc. The wheel experiments were generally sky-mapping experiments comparing radiation from the Sun to that in other portions of the sky. The observatories are launched from the AMR by Thor-Delta vehicles and orbit the Earth in a circular orbit at an altitude of 300 miles.

The first Orbiting Solar Observatory, Oso I, was launched 7 March 1962 into an orbit with perigee of 343 miles, apogee of 370 miles, and inclination of 33 degrees. All instrumentation worked well until 15 May 1962 when the telemetered tape-recorded playback began to malfunction. Thereafter, spacecraft data continued to be acquired in real time when the satellite was in the range of one of the minitrack network stations. On 22 May 1962 after 1,138 orbits a malfunction in the spin control caused the Oso I to spin up to a point where the servosystem could no longer orient the pointed scientific instruments and the solar cells. During its 77 days of useful life, the Oso I had performed almost 1,000 hours of observation of solar phenomena from above the Earth's atmosphere. It observed more than 75 solar flares and subflares.

Spacecraft structure, fabrication, and equipment for the first Oso spacecraft were furnished by Ball Brothers Research Corporation, Boulder, Colorado. The wheel diameter was 44 inches, the overall height of the satellite was 37 inches, and it had a total weight of about 440 pounds. Stabilization was achieved through the gyroscopic properties of the spinning wheel. After injection into orbit, three arms supporting containers filled with pressurized nitrogen gas were extended from the spacecraft, increasing its diameter to 92 inches over-all. These gas jets on the extended arms slowed the spin rate of the wheel to approximately 30 rpm from the initial spin of 100 rpm. The vehicle attitude was controlled by another high-pressure nitrogen gas tank system mounted in the center of the spacecraft. Two electrical servomotors operating in conjunction with Sun sensors stabilized the upper section of the spacecraft and pointed or elevated the experiments contained in the stabilized section. The short-term pointing accuracy was better than 1 minute of arc and the long-term accuracy better than 2 minutes of arc. Communication between the observatory and the minitrack ground stations was accomplished by an FM-FM telemetry system. Data were multiplexed into a tape recorder for playback to the ground stations on command. The tape recorder stored spacecraft data for 90 minutes of the orbit and played back the stored information in five minutes. Data were also transmitted from the spacecraft to the ground stations in real time. A solar-cell source and a nickel-cadmium storage battery pack constituted the power supply for the satellite system. The solar-cell array was made up of 1,860 solar cells, 1 centimeter by 2 centimeters covering 3.72 square feet. This array was capable of developing 27 watts of power. The average power available from the nickel-cadmium battery pack was 16 watts; of this, 7 watts were required for the control system, data system, and telemetry, and 9 watts were available for the various scientific experiments.

For the first Oso, Oso I, the oriented experiments were furnished by GSFC and are as follows:

1. X-ray spectroscopy measurements (10 Å to 400 Å).
   W. E. Behring and W. M. Neupert
   Goddard Space Flight Center

2. X-ray (20 Kev to 100 Kev) (2 Kev to 40 Kev), gamma-ray (0.510 Mev) monitoring.
   Kenneth Forest and William A. White
   Goddard Space Flight Center

3. Dust particle.
   Merle Alexander and Curtis McCracken
   Goddard Space Flight Center

The following experiments were mounted within the wheel or lower section of the spacecraft:

1. Neutron flux monitoring.
   Wilmot N. Hess
   University of California, Lawrence Radiation Laboratory, Livermore

2. Proton and electron measurements in the lower Van Allen belt.
   S. Bloom
   University of California, Lawrence Radiation Laboratory, Livermore

3. High-energy solar gamma-ray measurements (100 to 500 Mev).
   M. Savedoff and G. Fazio
   University of Rochester, New York

4. Measurement of solar gamma radiation (50 Kev to 3 Mev).
   John R. Winckler
   University of Minnesota

5. Emissivity stability measurements of surfaces in a vacuum.
   G. G. Robinson
   Ames Research Center

6. Solar radiation measurements (3,800 Å to 4,800 Å).
   Kenneth Hallam
   Goddard Space Flight Center

7. Solar ultraviolet measurements (1,100 Å to 1,250 Å).
   Kenneth Hallam
   Goddard Space Flight Center

8. Solar gamma-ray measurements (0.2 Mev to 1.5 Mev).
   William A. White
   Goddard Space Flight Center

For the second Oso, S-17, the following experiments have been selected:

1. Ultraviolet spectrometry (75 Å to 600 Å) (500 Å to 1,500 Å).
   Leo Goldberg and William Lillier
   Harvard University

2. White light coronograph measurements.
   R. Tousey
   Naval Research Laboratory

3. Lyman-Alpha scan.
   R. Tousey
   Naval Research Laboratory

4. X-ray scan, measurements of X-ray bursts, measurements of X-ray prominences.
   Talbot Chubb
   Naval Research Laboratory

5. Gamma-ray telescope measurements (100 Kev to 5 Mev).
   K. J. Frost
   Goddard Space Flight Center

6. Stellar spectrometric measurements (900 Å to 2,000 Å) (1,800 Å to 3,800 Å).
   K. L. Hallam, W. A. White
   Goddard Space Flight Center

7. Zodiacal light measurements.
   E. P. Ney
   University of Minnesota

8. Gamma-ray telescope measurements (50 Mev to 501 Mev).
   C. P. Leavitt
   University of New Mexico

Orbiting Geophysical Observatories

The Orbiting Geophysical Observatories (Ogo) are a series of standardized spacecraft of sufficient size to accommodate 30 or more experiments for conducting scientific and technological studies in various orbits from near earth to lunar space. Two types of such observatories are planned:

1. The Eccentric Orbiting Geophysical Observatory (Ego) will be placed in a highly eccentric orbit to study phenomena from 270 to 110,000 kilometers. This observatory will be particularly useful for the investigation of primary cosmic radiation beyond the geomagnetic field, the spectral distribution and nature of the outer Van Allen Radiation Belts, the far geomagnetic field and ionosphere, the transition regions to the interplanetary field and plasma, and the variation of these quantities with solar activity.

2. The Polar Orbiting Geophysical Observatory (Pogo) will be placed in near-Earth polar orbits (260- to 920-kilometer polar orbits). Pogo will emphasize the investigation of the important and interesting phenomena of the polar atmosphere, including the radiation belt "horns," auroral particles and electromagnetic radiation, low-energy primary cosmic rays, the geomagnetic field and associated current regions, ionospheric disturbances and heterogeneities, and anomalous temperature and density changes.

The Ego (S-49) will weigh approximately 1,000 pounds, which includes 150 pounds allotted to experiments. It is scheduled to be launched with an Atlas-Agena B during 1963 from AMR.

The first Pogo (S-50) is scheduled for launch with a Thor-Agena D during 1964 from PMR. Future Ogo spacecraft, designed to utilize a Centaur vehicle, will permit an additional 200 pounds of experiments, a 300-pound piggyback spacecraft and 100 pounds for additional structure and miscellaneous considerations.

Spacecraft design, development and fabrication, assembly, integration of experiments, and test and evaluation are under contract to the Space Technology Laboratories, Los Angeles, California. Procurement of some of the equipment has already been subcontracted to Advanced Technology Laboratory, Mountain View, California; Bendix Aviation Corporation, Teterboro, New Jersey; and Radio Corporation of America, Princeton, New Jersey.

The current design for the Ogo spacecraft consists of a body of approximately 2-3/4 by 2-3/4 by 5-1/2 feet, which contains portions of the stabilization control, power supply, communications, data-handling, and thermal control systems, as well as space for experiments. The power-supply system consists of solar cell panels, nickel-cadmium batteries and a charge control system. The system will furnish an average and a maximum power of 250 watts and 414 watts, respectively. The maximum power allotted to experiments is 80 watts and the average power is 50 watts. Angular orientation of the spacecraft is accomplished through torques provided by motor-driven inertial flywheels and by gas jets. Spacecraft deviations from the Sun axis are sensed by solar cells and deviations from the Earth's local vertical are determined from the outputs of horizon scanners. Thermal control is accomplished through thermal radiation shields and the use of louvers for active temperature control. The data processing and communications system accepts ground commands to program experiments, to vary transmission rates and time, and to adjust and apportion bits into experimental measurements and vehicle performance data. Storage of up to 84 million bits of data is obtained by use of two magnetic tape recorders. The spacecraft incorporates two redundant wide-band telemetry transmitters for sending the majority of the experiment and spacecraft data to the Earth either on command, or in real time, or from storage.

The following list of experiments and experimenters are for the Ego (S-49) payload. The experiments for the first Pogo have not yet been selected.

1. Seven (7) Geiger Mueller tubes to measure the omnidirectional intensities of electrons of energies exceeding 40 Kev, 120 Kev, and 1.5 Mev.
   J. A. Van Allen
   State University of Iowa

2. Scintillation type detector to measure solar cosmic-ray fluxes in the range from 2 Mev to 90 Mev.
   K. A. Anderson
   University of California, Berkeley

3. Spectrometer to measure electron energy up to 4 Mev. Ionization chamber and Geiger counters to monitor galactic cosmic radiation and continually monitor the Earth's trapped radiation (electron energy from 20 Kev to 20 Mev).
   J. A. Winckler and R. Arnoldy
   University of Minnesota

4. Faraday cup plasma probes to measure proton flux, proton energy spectrum, direction of flux, temporal and spatial variations of these quantities in the energy range from 10 Ev to 10 Kev.
   H. Bridge
   Massachusetts Institute of Technology

5. Charged-particle telescopes to investigate low-energy galactic cosmic radiation, to study energetic particle fluxes of protons above 0.2 Mev and other nuclei at higher energies and examine possibility for measuring isotopic abundances using solid-state detector techniques.
   J. A. Simpson
   University of Chicago

6. Triaxial Search Coil Magnetometer to investigate magnetic-field fluctuations in the frequency range 0.01 to 1,000 cps.
   E. J. Smith
   Jet Propulsion Laboratory

7. Plasma probe to measure proton concentrations ranging from 10^{-2} to 10^{-4} particles per cubic centimeter as a function of proton energy in the range 0.2 to 20 Kev.
   M. Bader
   Ames Research Center

8. Cosmic-ray telescope to study the charge and energy spectra of primary cosmic radiation.
   F. B. McDonald
   Goddard Space Flight Center

9. Double gamma-ray spectrometer to measure positrons (0 to 3 Mev) and to monitor solar photon bursts.
   T. L. Cline and E. W. Hones
   Goddard Space Flight Center

10. Ion-electron scintillation detector to study trapped electrons with directional energy flux, 10 Kev $<$ Energy $<$100 Kev, and the directional intensity of protons (120 Kev $<$ Energy $<$ 4.5 Mev).
    L. R. Davis
    Goddard Space Flight Center

11. Flux-gate and rubidium-vapor magnetometer to measure magnitude and direction of magnetic fields, to measure accurately the magnetic field vector over range 1 to 100 gammas to accuracy ≤ 1 gamma.
    J. P. Heppner
    Goddard Space Flight Center

12. Transmitter to radiate linearly polarized signals toward Earth to measure number of electrons beneath the satellite. Frequencies in the range of 40 mc/sec and 360 mc/sec will be transmitted.
    R. S. Lawrence
    National Bureau of Standards

13. Antenna and electronics to study radio noise and related propagation phenomena at low and very low frequencies (200 cps to 100,000 cps).
    R. A. Helliwell
    Stanford Research Institute

14. Spherical Electrostatic Analyzer to measure the concentration and energy distribution of charged particles. Energy distributions of these particles over range 0 to 1.0 Kev for densities of positively and negatively charged particles with concentrations in the range 10 to 5 x 10^6 per cubic centimeter.
    R. C. Sagalyn
    Air Force Cambridge Research Institute

15. Planar Ion and Electron Trap to obtain the densities and energy distributions of charged particles of both polarities in the low-energy or thermal range. Energies measured and concentrations are about the same as those in experiment 14. The two experiments are complementary.
    E. C. Whipple, Jr.
    Goddard Space Flight Center

16. Record and measure radio noise incident upon an electrically short dipole in the frequency band 2 to 4 mc/sec primarily to measure the dynamic radio spectra of solar bursts.
    F. T. Haddock
    University of Michigan, Ann Arbor

17. Detectors to determine the contribution of geocorona and interplanetary medium to the Lyman-Alpha glow in the night sky.
    P. W. Mange
    Naval Research Laboratory

18. Radiofrequency Ion Spectrometer to obtain a direct measurement of positive ion composition in the mass range 1 to 50 atomic mass units.
    N. W. Spencer and H. Taylor
    Goddard Space Flight Center

19. Detector to measure the vectorial velocity distribution of dust particles with dimensions of about 0.5 to 5 microns; determine the cumulative mass distribution; observe the fluctuations in velocity and mass distribution and spatial density; effect of geocentric distance.
    W. M. Alexander
    Goddard Space Flight Center

20. Equipment to determine the cause and spatial extent of the Gegenschein. Pictures will be taken in the green ultraviolet and infrared regions. (Backup experiment.)
    C. T. Wolff
    Goddard Space Flight Center

Orbiting Astronomical Observatories

The Orbiting Astronomical Observatories (OAO) are designed to provide an opportunity to explore those regions of the spectrum that are now inaccessible because of atmospheric absorption. The OAO is a precisely stabilized satellite designed to accommodate various types of astronomical observing equipment. The primary experiments for the first three observatories are all concerned with stellar astronomy in the ultraviolet range 800 to 4,000 Å.

1. The first OAO will carry two prime experiments:

    a. A mapping study of the celestial sphere in three ultraviolet ranges. This experiment will map the sky in ultraviolet down to a wavelength of 1,100 Å with three broadband television photometers and will record the brightness of at least 20,000 stars.

    b. A broadband photometry study of individual stars and nebulas. These observations will be directed toward the determination of the stellar energy distribution in the spectral region from 800 Å to approximately 3,000 Å, and the measurement of emission line intensities of diffuse nebulas in the same spectral region. These investigations are expected to provide data which will not only be useful to the entire astronomical community but will also act as an aid in designing later instrumentation.

2. The second OAO will contain a system designed to obtain absolute spectrophotometric data on selected stars, nebulas, and galaxies. The optical system will employ a relatively fast 36-inch Cassegrain telescope with a large aperture spectrophotometer and will use both the coarse (1 minute of arc) and the fine (1 second of arc) control systems. The usable spectral region will be approximately 912 to 4,000 Å.

3. The absorption experiment in the third observatory has, as its primary objective, quantitative observations of the absorption spectrum of the interstellar gas in the regions between 800 and 1,500 Å and 1,600 and 3,000 Å.

It is expected that later satellites will be used for studies of the Sun and planets. In addition, all observatories will have a limited amount of payload capacity for small secondary experiments.

The present concept of the OAO spacecraft is being developed by the Grumman Aircraft Engineering Corp., Bethpage, L. I., N. Y. The basic structure is octagonally shaped with a central tubular area containing the experiment equipment. The total weight of the spacecraft is expected to be about 3,300 pounds, of which 1,000 pounds is allocated to the experimental apparatus.

The power supply for OAO is externally mounted fixed arrays of silicon solar cells used in conjunction with rechargeable nickel-cadmium storage batteries. An average power of 330 watts is available from the arrays. The power available to the experimental equipment is to be 30 watts average and 60 watts peak.

The stabilization and control system consists primarily of star trackers, Sun trackers, inertial wheels, and gas jets. The requirements imposed on the guidance and control system will permit determination of the absolute direction of the optical axis to an accuracy of one minute of arc and an orientation of the optical axis to one degree with respect to a known reference. Also, the control system will permit an ultimate guiding accuracy of 0.1 second of arc during observation of an individual star. The major functions of the attitude control system may be categorized as follows:

1. To stabilize the spacecraft following booster separation and to establish its attitude with the required precision.

2. To slew the satellite to any desired attitude as dictated by the scientific objectives of the mission.

3. To enable the satellite to maintain a given attitude with the required accuracy for long periods of time.

The remainder of the basic system is comprised of data storage units and a communications system, including four radio links which are required to accomplish tracking, command, and telemetry.

The satellite will be launched by an Atlas-Agena B from AMR into an approximately circular orbit at an altitude of 500 statute miles, inclined to the equator at an angle of 32 degrees.

Other planned Orbiting Astronomical Observatories are the S-18, S-58, and S-68. The experiments and experimenters are as follows:

For the S-18,

a. Mapping in three ultraviolet ranges
   Fred Whipple
   Smithsonian Astrophysical Observatory

b. Stellar broadband photometry measurements in ultraviolet
   Arthur Code
   University of Wisconsin

For the S-58,

Absolute spectrophotometry measurements
James Milligan
Goddard Space Flight Center

and for the S-68,

Interstellar absorption measurements
Lyman Spitzer
Princeton University

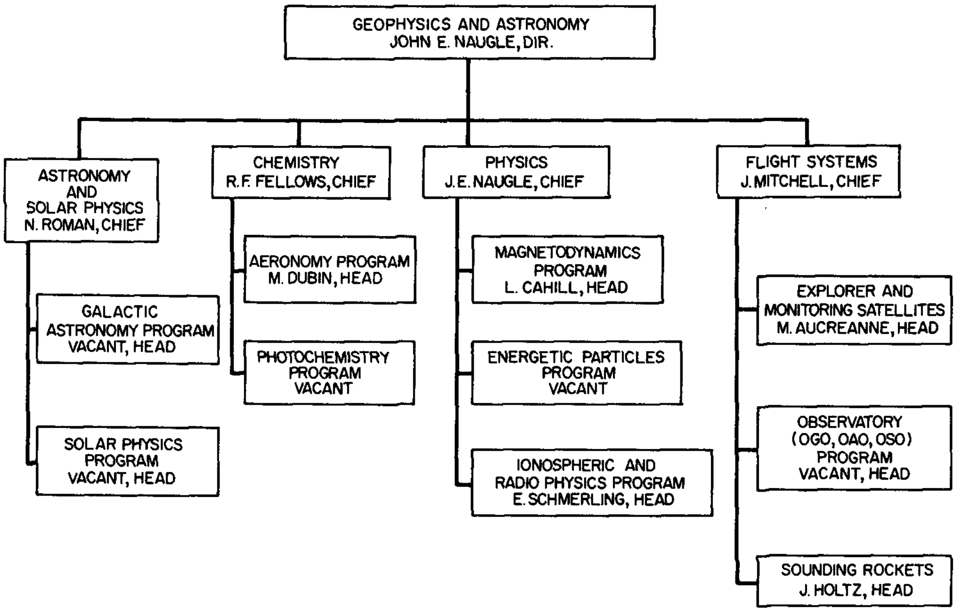
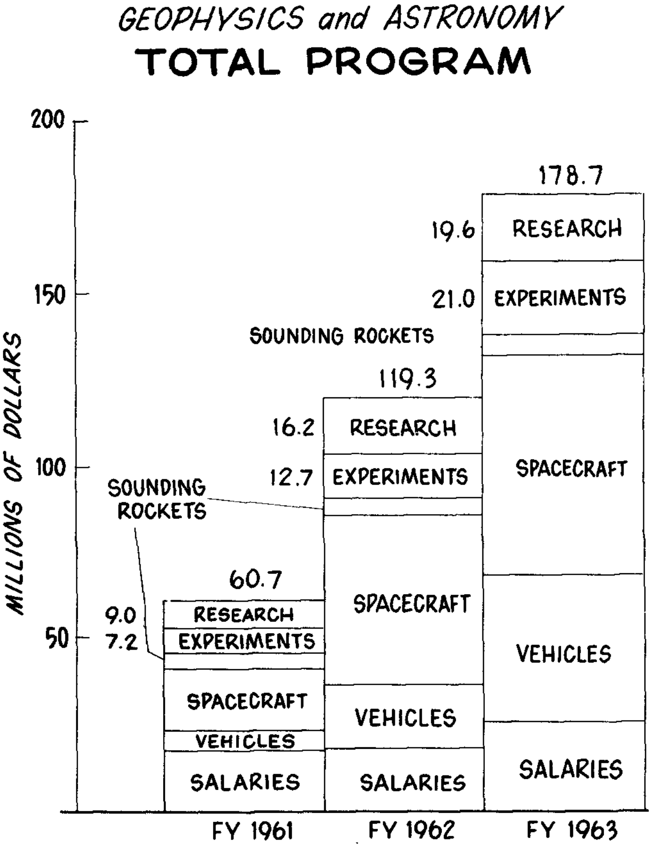


Figure 1



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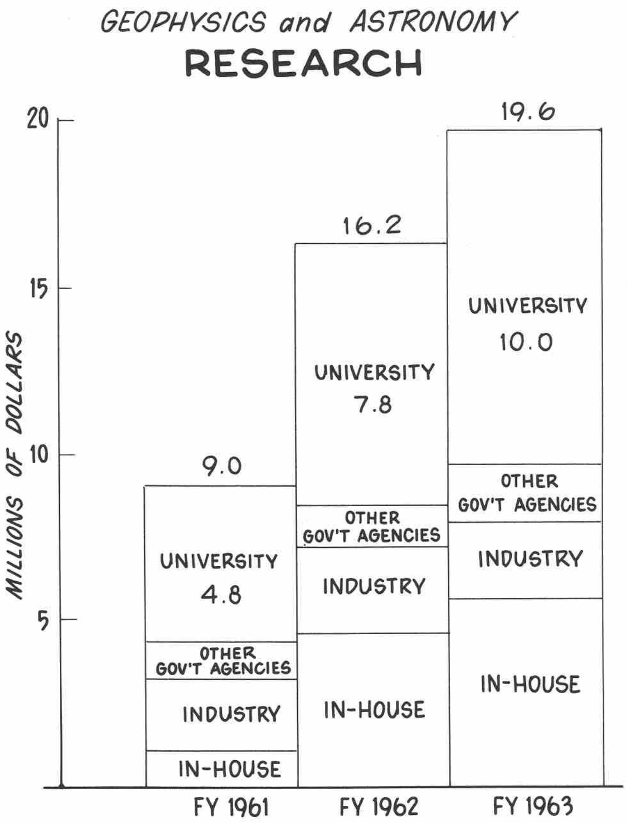

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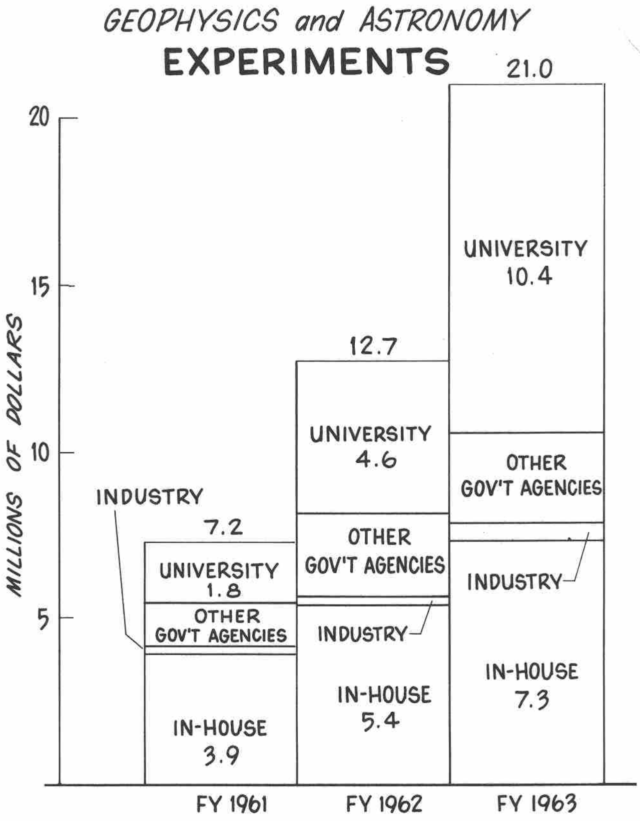

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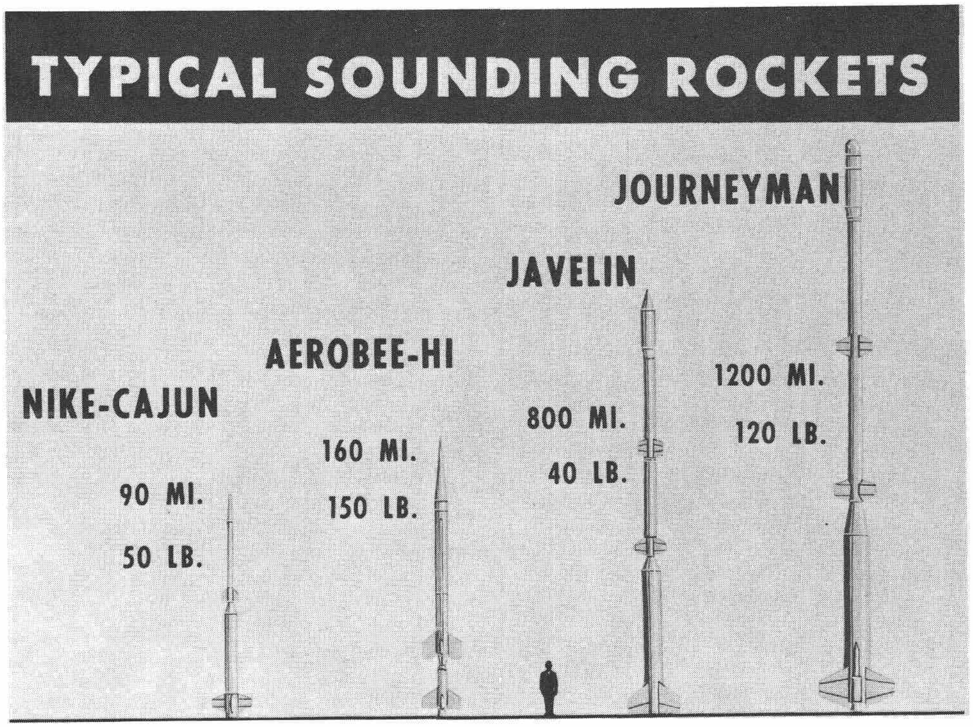

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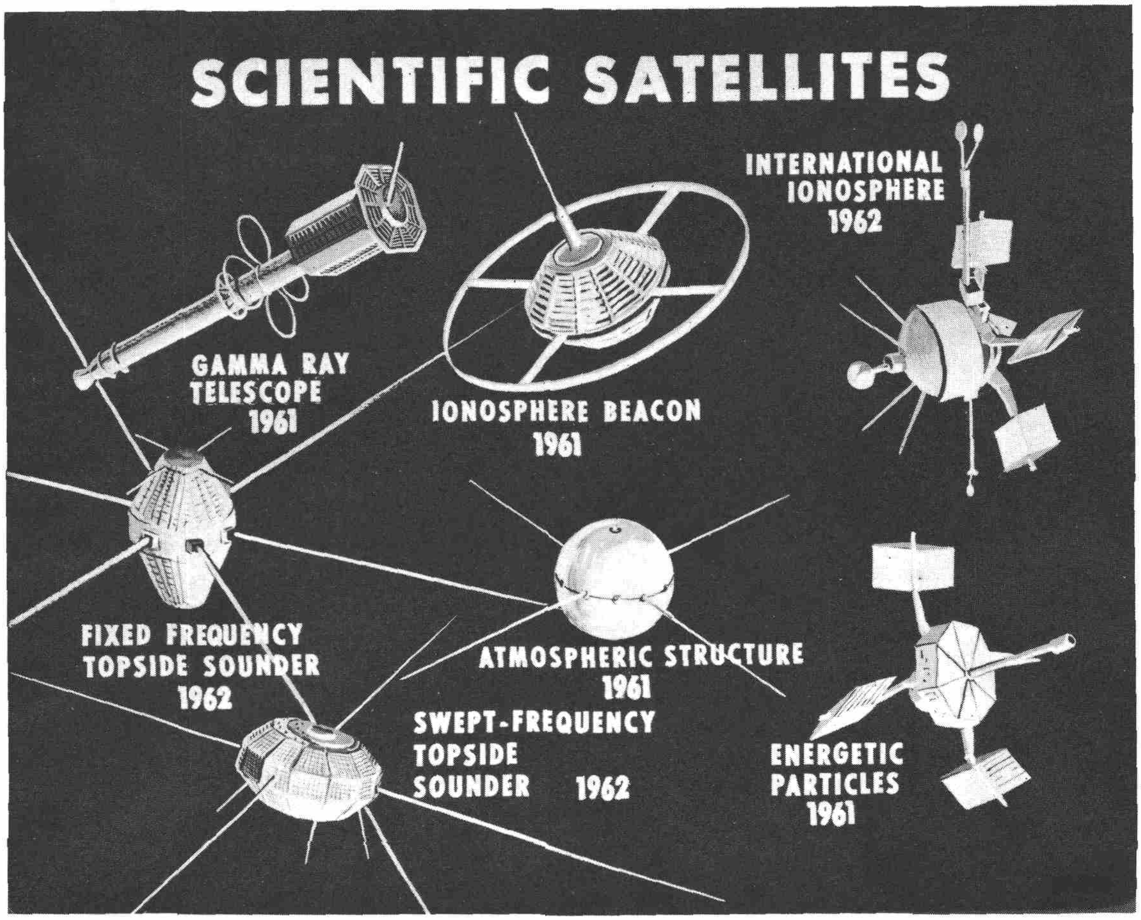

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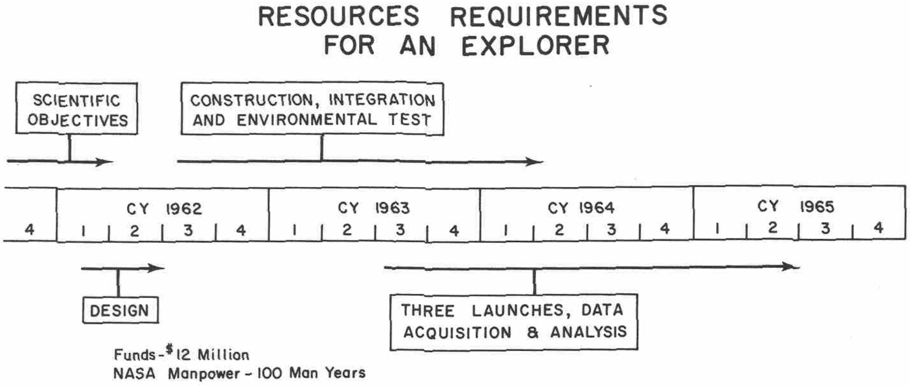

RESOURCES REQUIREMENTS FOR AN EXPLORER

Funds-$ 12 Million
NASA Manpower - 100 Man Years

Figure 7

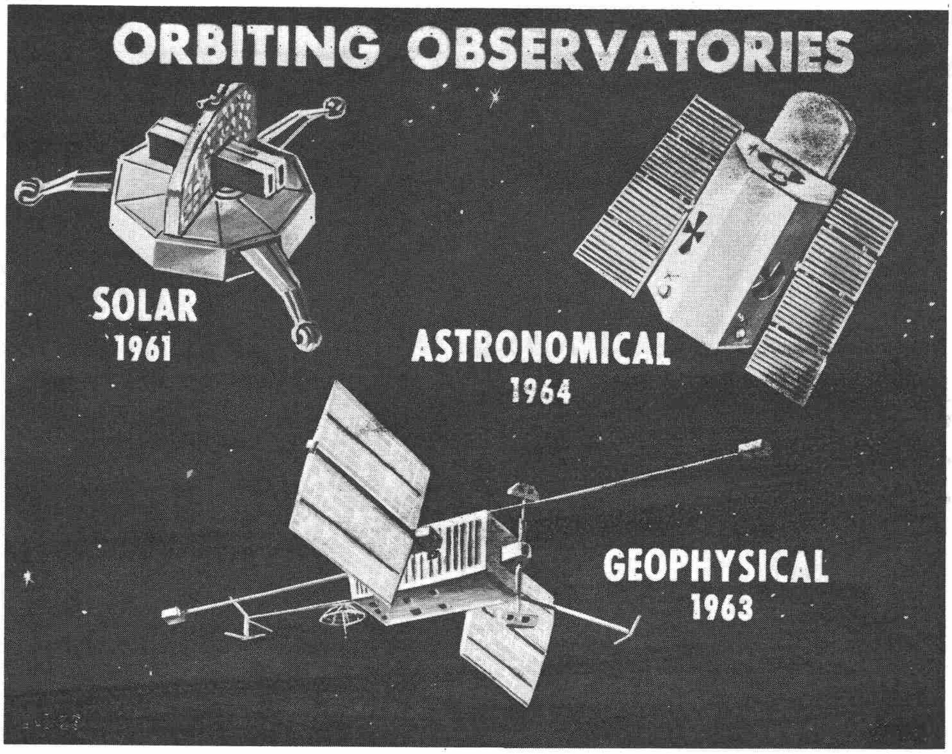
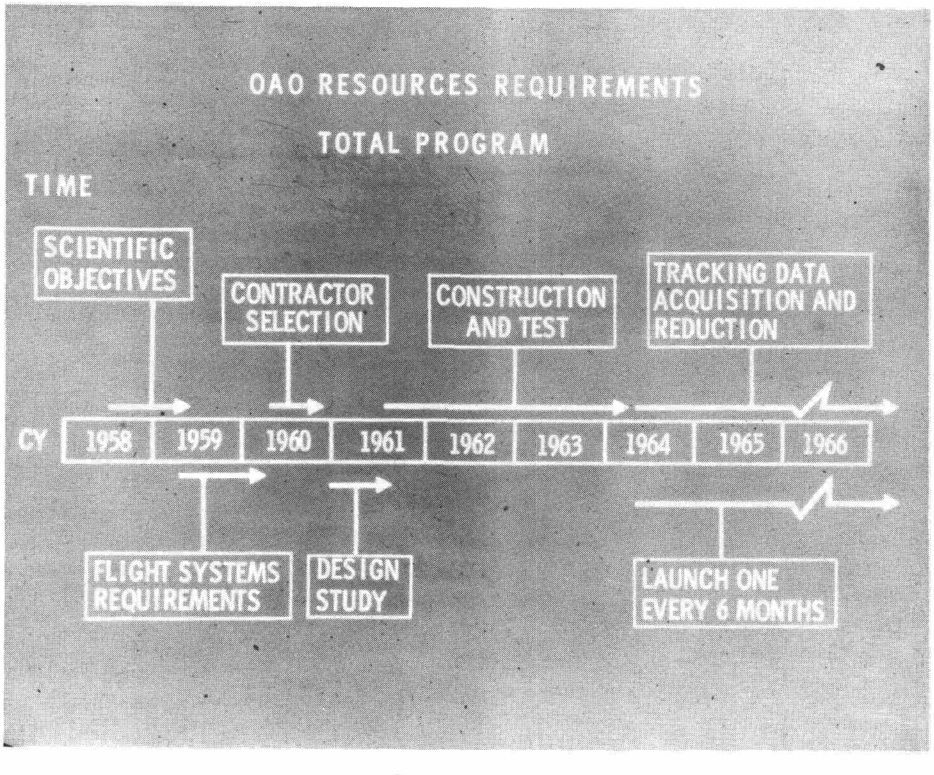


Figure 8



OAO RESOURCES REQUIREMENTS

TOTAL PROGRAM

TIME

SCIENTIFIC OBJECTIVES

CONTRACTOR SELECTION

CONSTRUCTION AND TEST

TRACKING DATA ACQUISITION AND REDUCTION

CY	1958	1959	1960	1961	1962	1963	1964	1965	1966

FLIGHT SYSTEMS REQUIREMENTS

DESIGN STUDY

LAUNCH ONE EVERY 6 MONTHS

Figure 9

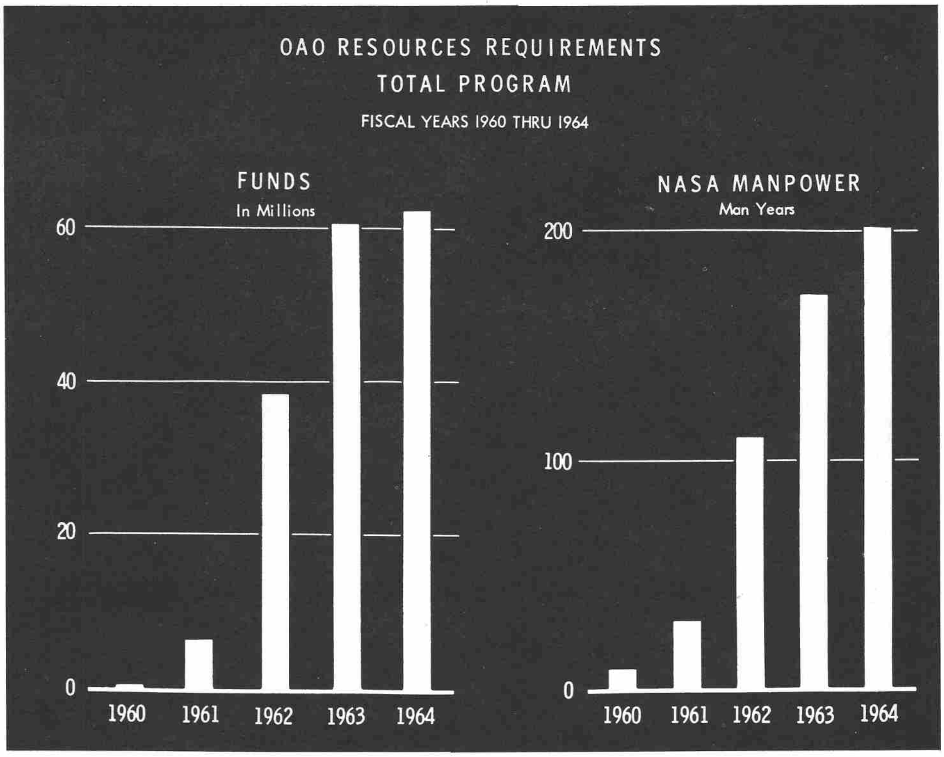


Figure 10

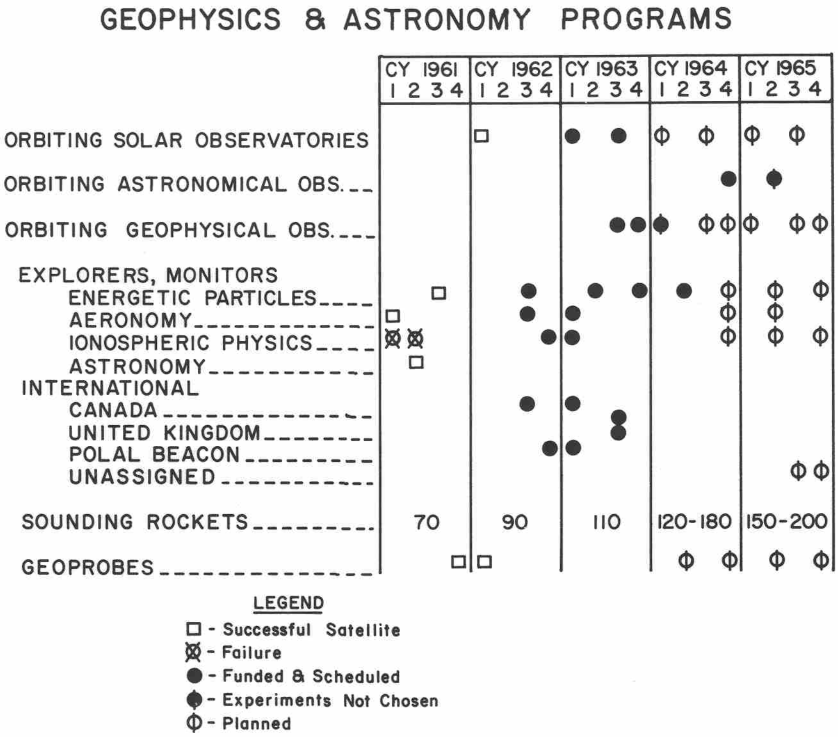


Figure 11

BIOSCIENCE PROGRAM

By Orr E. Reynolds

The investigation of space from a biological standpoint has its roots back in the early exploration phase of man's history—the early mountain climbing expeditions and balloon flights. Since then, there has been a continuum of increase in knowledge of the effect on man of increased altitude and the accompanying velocities and accelerations. The development of methods for protecting man from the effects of decreasing pressures and increasing accelerative forces was the primary result of the struggle for air superiority in World War II. Oxygen equipment, pressure-breathing equipment, pressure cabins and capsules, and restraining equipment—first belts, then shoulder harnesses, and then anti-g suits—were all in use in wartime aviation. A very large part of the technology required for equipping man to travel in spacecraft was developed as a part of regular aviation medicine, or aviation physiology.

In addition to the biological problems solved by these protective devices, however, the capability for rocket flight into space generates new hazards, effects of which are only now being studied scientifically. The two most apparent effects in importance are weightlessness and space radiation. Weightlessness may present problems that have not been encountered in the recent short-duration space flights by the U.S. and U.S.S.R. The effects of prolonged weightlessness on psychomotor functions, viseral reflexes, mineral metabolism, the recovery of muscular and circulatory tone on return to high gravitational fields must be determined and will be carefully explored by orbiting experiments over periods from 2 to 14 days (or perhaps longer) in spacecraft. It should be emphasized, however, that ultimately we should secure information on these biological processes not just on the mammalian organ systems in order to make a test preparatory to manned flight. We should study these processes at a more fundamental level in simpler biological systems. Only in this way can we determine the possible occurrence of subtle cellular effects which might alter basic biological processes. The implication is that changes may occur which normally would be missed by the observer, or delayed in more complex organisms, such as man, with better homostatic mechanisms. Accordingly, we plan to fly specimens to determine the effect of weightlessness on cell division, plant growth, embryonic development, and similar experiments in an Earth-orbiting satellite.

In the radiation area there is great difference of opinion as to the necessity for study of space radiation on living specimens in space. Although a minimal amount of information has been obtained in previous balloon and rocket experiments, unconfirmed observations suggest that there are effects produced either by the types of particles encountered in space, which we have not been able to duplicate in the laboratory, or by some synergetic action between these particles and other environmental factors such as weightlessness or change in magnetic environment. Every one of these observations is open to rather serious question since none have been confirmed and most of them tend to go in directions not predicted.

In many ways the most exciting part of the bioscience program in the Office of Space Sciences is the search for extraterrestrial life. The history of this search began with the publication of Darwin's "Origin of the Species by Natural Selection" in 1859. In this convincing display of the observations of nature, he pointed out general evolutionary features of the origin of plants, animals, and even man. This work today is the most pervading generalization in biology. It has been followed by decades of attack and criticism. Its acceptance as a scientific fact, although not explained thoroughly in process, is due to support from many other disciplines, including paleontology, geology, genetics, and comparative anatomy. The genetics developments many years later by Morgan, Mendel, and others provided evidence of the hereditary mechanism for the production of the infinite variations from which traits for survival are selected. The exact relationship between the genetic mechanisms and evolution, however, is yet to be determined.

The origin of protoplasm in organic materials has remained a problem. As a matter of fact, the term "organic chemistry" originally generated from the idea that these compounds could be produced only by living organisms. In 1928, Waller synthesized, from the inorganic salt, ammonium cyanide and, in 1951, Calvin was able to synthesize formaldahyde and formic acid from CO_2 and water. Subsequently there have been syntheses conducted by Miller and Abelson, Fox, and others of amino acids and even more complex biologic-like molecules under conditions that might well have existed during the early planetary environments. This gives us some reason to feel that we are in a position to delve into the origin of life and the development of organisms, an area of real significance insofar as the considerations of what we should look for in the search for extraterrestrial life are concerned. The program that we are actually conducting or starting to conduct in the Office of Space Sciences is outlined briefly as follows. The objective is to acquire knowledge of living organisms relevant to the space program on two bases: (1) problems that are generated by space flight and exploration and (2) biological problems which are made accessible for study only by space flight and exploration. The focus of this program is the development of better biological concepts or theory. Lack of theory has been a great weakness of biological research. Because of the complexity of living things and the high degree of organization, theory has been slow in developing. Better theory can result in more meaningful experiments. The space program provides an incentive to develop better theoretical concepts in biology. This will be undertaken in four areas: exobiology, environmental biology, behavioral biology, and physical biology. These programs are defined as follows:

Exobiology Program. Ground-based and in-flight experiments to identify and study extraterrestrial life and to determine the type of analysis necessary for such identification. Analysis and development of space probe decontamination methods, ground-based research on the origin of organic compounds (protobiochemistry), analysis of meteorites for organic constituents, distribution and characterization of microbes in the upper terrestrial atmosphere, infrared spectroscopic analysis of the planets, and the design and flight of experiments for the detection and study of life on other planets.

Environmental Biology Program. Study of the effects of outer space and planetary environments on living systems and processes, as conducted in ground-based laboratories and in outer space on space-flight missions. Ground-based studies concerned with activity of Earth organisms in ecological niches which offer extreme or unusual conditions. In-flight experiments concerned with biological aspects of the organism where exposed to weightlessness; space radiation, and hard vacuum of space.

Behavioral Biology Program. Studies concerning the biological bases of behavior, such as the investigation of behavioral pattern formation and localization at the cellular and subcellular (e.g., molecular) level, the application of cybernetics principles for the monitoring of the organism, including communication, orientation, and rhythmicity. Investigation of sensory and motor processes, vigilance, learning, thought, memory, and emotion, as they pertain to man's functioning in the space environment.

Physical Biology. Study of the fundamental bases of performance including physiology of the respiratory system, cardiovascular system, and the central nervous system, as well as studies of metabolism and nutrition, and biomechanics (e.g., vibration, acceleration, and weightlessness). Also included are the blending of the disciplines of biology and physics in the production of instruments for biophysical observation and the acquisition of new biophysical data.

We will pursue development programs in two areas—development of instrumentation and development of space flight systems. The flight program would consist of four types. First, there are experiments of opportunity, as they are called, in which a biological experiment is placed in a vehicle that is flying for some other reason. (This has not been too successful in the past and is one of the major reasons for the fact that many of the biological experiments that have been done in space have not been very rewarding.) Next, we feel that we must have some actual orbiting biological laboratories. There are two systems proposed at the present time. The fourth type of opportunity is experiments on high-altitude balloons.

Biological effort has largely lagged behind physical science effort in this area. The most important job to be done by NASA in accomplishing an effective program to exploit the opportunities of space transport to benefit biological science and for manned exploration is the enlistment of the biological science community in an enthusiastic attack on the problems.

LUNAR AND PLANETARY PROGRAM

By Oran W. Nicks

This presentation is a rather brief resumé of lunar and planetary programs. The program covers a broad spectrum and rather than go into superficial detail on all aspects, I will emphasize various portions which are examples of the rest of the work in lunar and planetary programs. The basic objectives or rationale of our programs, the organizational elements of the lunar and planetary directorate, the basic elements we consider to be fundamental and important in planning flight programs to date, and a survey of current and continuing programs including some rule-of-thumb costs for these will be briefly reviewed herein. The instrumentation of some of these systems and the facilities required will be briefly discussed to provide a perspective and understanding of what is involved in the total program. The probable returns of programs of this type will also be considered briefly as well as the ground-based programs that are an essential complementary part of all the lunar and planetary programs and the relation of our program with the manned and other programs of NASA.

With respect to basic objectives and rationale, a fundamental element of all our programs that is most annoying to both the scientist and the engineer is the fact that we are simultaneously, in every case, attempting to achieve technological developments and, at the same time, make good scientific measurements. This feature is predominant in all programs and results in frustrating compromise. Along with this evolutionary development, we are trying to plan programs to make extended use of developed systems. We hope that we can develop fundamentally sound techniques and spacecraft and use these many times, because in that way we hope to reduce both their cost and to increase their reliabilities as tools.

Another fundamental objective or part of our rationale is that an attempt will be made to make interplanetary measurements on all our shots although they are going to the Moon and the planets. It is possible that there will be experiments for the Moon and the planets which completely overlook the interplanetary elements. Generally, however, there is something to be gained by making these experiments and making them repetitively either in cislunar space or between the Earth and other planets. This is a factor which always has to be weighed against the primary aspects of the mission and, in general, this kind of experiment is included.

It is considered an advantage for projects to overlap in time. That is, a project that is being developed and used for a period of time should completely overlap the initial stage of a new and bigger and better system. This has not always been possible; however, if we could jump to 1970 and look back, we would find, I believe, that this has generally been the case.

In planning, we decided long ago that inasmuch as planetary opportunities to investigate Venus and Mars (the two planets which we are initially interested in) only occur on the order of once every two years, we would endeavor to launch two vehicles to these planets at each opportunity. The second launch would not be for backup. There is not enough time after the first launch to know whether it will be successful before the second vehicle is launched; thus, two launches are made in order to increase the chances for successful mission completion. It is hoped someday to have two spacecraft of different kinds and therefore multiples of two could be launched toward the planet. All these factors have to be properly weighed in order to maximize returns, and therein lies the most important element of our work: to decide what is most important and how it should be done.

The lunar and planetary office is organized differently than other offices in the Office of Space Sciences for several reasons. One, we have fewer programs and more long-term uses of the same type of spacecraft. Basically, our organization is divided into three segments. The first has to do with flight programs and includes elements of management, science, and engineering directed towards the accomplishments of those programs. The staff assigned to this category work specifically on the Ranger, Surveyor, and the Mariner to guide these projects toward their goals. Another segment is called Sciences. In this group are thoroughly experienced scientists who have specialized in some discipline such as planetary atmosphere, geology, astronomy, solar physics, and so forth. These scientists are concerned with the scientific planning for all present and future projects. They help any project that needs help in their discipline. They work with the scientific community and with subcommittees and, in addition, they have programs which they sponsor. For 1963 the lunar and planetary office has sponsored research contracts of $8 to $10 million in these scientific disciplines. The monitoring of this research is done at the center associated with the discipline. One of the main center responsibilities is to coordinate returned data and help provide the proper stimulus and the focal point for preparing and presenting the information to the world. The third segment is Advanced Programs and Technology. In this group are engineers and project scientists who are concerned with program planning and formulation. In addition, these technologists consult in some of the areas that are of extreme importance to unmanned space science, such as telecommunications, and they work also with the development facilities that are used in all our programs, such as environment simulators. They also have an advanced development program and some money of their own. Their program is usually conducted through the centers. They are concerned with assembling a program for advance research in the technological areas suitable to our future needs. They conduct or have conducted mission studies to determine the proper combination of orbiters and landers and so forth.

A knowledge of the basic elements of planning is essential to an over-all awareness of how experiments fit into the program. The problem basically is ordering experiments in time so as to maximize the return for the given resources which we have; resources are funds, manpower, facilities, time, and material. Some of these are interchangeable; others are not. We do have to be concerned with launch vehicle capability, availability, and reliability. The lunar and planetary programs have been paced very much by the capability of launch vehicles. The spacecraft technology is important: whether the system can be attitude stabilized, whether it is required to spin, whether the proper solar power or the proper telecommunications are available. There are some real problems in converting somewhat standard laboratory instruments into flight instruments and in miniaturizing some items that fill a room and take half the power of Boulder Dam down to a weight of a few ounces and a power requirement that will be satisfied by a few solar cells. Operation and command of these spacecraft are important. The last Ranger was launched successfully but a malfunction made it impossible to command.

Telecommunications makes research possible once the experiment is launched into space. If we can communicate with a spacecraft which passes near or lands on Mars or Venus, we will have achieved a technological milestone. The communication with Pioneer V at 22 million miles is a record so far.

The ground support equipment is also a very important item in planning. The object of a launch is to obtain data and the data analysis and handling become of fundamental importance after launch. The role of the experimenter at the universities has not, in my opinion, been amplified enough in this field. I believe that in data analysis and reduction, graduate students can be very useful; they can also contribute by their own analyses. One other variable is the type of mission to be flown. This may not appear to be of vital importance at first; however, consider the observational measurements that can be obtained by flying an instrumented spacecraft close to a planet. Atmospheric properties can be measured as well or better than by a manned spacecraft. There are entry flights, impact landings, orbiters, and other types of missions which add at least three or four variables to the list of factors which must be considered when trying to decide what is the best way to conduct the program. Reliability for the different missions is paramount in decision making.

A brief discussion of the flight program and our philosophy in the lunar and planetary flight programs includes: the Able series which was generally unsuccessful in spite of good experiments and spacecraft because it was adapted from military equipment to civilian space applications; the Pioneer V which is probably the guiding light in interplanetary research; the Explorer X which helped in the study of the magnetic fields within and outside the Earth's magnetosphere; and the four flights of Ranger. Although the scientific experiments on Ranger were not placed in the planned environment, much information of a technological nature was gained. The first two carried a fine array of interplanetary experiments, none of which really worked as they should because they stayed in Earth orbit due to the failure of the Agena on the second burn. The third Ranger went by the Moon at about 23,000 miles. It worked fairly well as a spacecraft until it got in the vicinity of the Moon, and during the terminal maneuver, we had a malfunction which caused loss of altitude control. The fourth Ranger was launched on a good trajectory in April 1962, but, just after injection, it malfunctioned because of some strange occurrence in the central controller and sequencer. This same system, which is the heart of the spacecraft, had been run some 700 hours during the checkout of all the other systems; at no time during the course of earlier Ranger tests had this kind of malfunction occurred.

The present flight program is shown in Table I.

The Ranger, which is launched with the Atlas-Agena B, has made four flights. There will be another one in 1962 and four in 1963; we have proposed five for 1964 that we believe will be an official part of our program soon. The Surveyor is to be launched on the Centaur with early test flights scheduled sometime either in late 1963 or in early 1964 with operational flights beginning late in 1964; a series of seven flights has been approved. The orbiter, which is a Surveyor adaptation is scheduled to be launched sometime late in 1964 and the program will be extended to 1970 and beyond. There are other possibilities for lunar spacecraft that are being considered; we are always attempting to ascertain the best way to accomplish missions and it could be that there will be some additions or substitutions in this program. Shown in Table I is the schedule for the Prospector; actually this has not been authorized and it is very likely that this program will be changed and integrated into the manned program. We still believe that we may use this kind of system to carry mobile or sample return vehicles but probably not until sometime in 1966 or 1967.

Figure 1 shows the Ranger spacecraft and three kinds of space payloads it carries, it has carried, or it is to carry. The interplanetary payload included about nine experiments, perhaps the most prominent of which were the Lyman-Alpha telescope, magnetometers, ion chambers, Geiger counters, and so forth. The bus is the basic spacecraft which unfolds and has a high-gain antenna which gives a gain of about 28 decibels. The sketch of the lunar landing capsule shows a radar altimeter which triggers the retromotor, electronic boxes, and gamma-ray spectrometer. A cutaway sketch of the lunar landing capsule is shown in Figure 2. The lunar landing capsule, which is to be soft landed, is equipped with a seismometer. Beneath the capsule is a solid retrorocket; the altimeter is off to the side. We have one more of these to fly late in 1962. The other two mission payloads have already been launched. The lunar photograph payload (Fig. 3) was put into the program for several reasons. First it appeared that very significant data about the Moon can be obtained simply by taking various photographs. Actually most of the data we have at present were obtained by observing the Moon in the visible spectrum. With this system there are six cameras, two of which are wide-angle cameras with overlapping coverage and four of which overlap and are of relatively narrow angle. The last picture taken with the lunar photographic payload is expected to have a resolution on the order of six inches. There will be a camera on the next Ranger (Fig. 2) in addition to the landing capsule, and it will take on the order of one to two hundred pictures on lunar approach. The Ranger spacecraft weighs about 750 to 800 pounds and the cost is now averaging on the order of $7-1/2 million to $8 million apiece. The first five Ranger spacecraft and all the operations that went with the spacecraft totaled around $60 million, including development; thus, they cost approximately $12 million apiece. They now cost somewhat less than that because use is being made of items already developed. It costs around $10 million to develop each of the two payloads. The launch vehicle for a Ranger spacecraft costs approximately $8 million, so that, in total, to launch a Ranger of the type already developed would cost on the order of $16 million. The lunar impact locations are shown in Figure 4.

The Surveyor configurations for landing missions and for orbiting missions are shown in Figure 5. The Surveyor is designed to make a soft landing with vernier engines. Otherwise the program is very similar to that for the Ranger. The Surveyor which is shown in Figure 6 carries a variety of experiments.

It descends on the order of five to seven feet per second with landing impact of approximately eight to 15 g-units. In other words, the landing stress will be about the same as the injection stress. The landing gear has a nonreusable shock absorption system which will withstand descents of approximately 50 feet per second which is one and a half to two times the rate of descent of an aircraft making a carrier landing.

A list of the instruments for which the Surveyor is being designed is given as follows:

1. Television:
    - Three cameras
    - High-resolution telescope
2. Physical Parameters:
    - Subsurface sampler
    - Surface sampler
    - Anchoring device
    - Sample processor
    - Surface geophysical subsystem:
        - Temperature
        - Thermal diffusivity
        - Magnetic susceptibility
        - Density
        - Hardness
        - Acoustic velocity
        - Resistivity
        - Soil mechanics
    - Subsurface geophysical probe:
        - Temperature
        - Thermal diffusivity
        - Magnetic susceptibility
        - Density
        - Acoustic velocity
        - Caliper (hole size)
3. Lunar body:
    - Seismograph system
4. Chemical analysis:
    - X-ray spectrograph
    - X-ray diffractometer
    - Gas chromatograph
5. Fields and particles:
    - Magnetometer
    - Plasma probe
    - Atmosphere gage
    - Radiation detector

Not all these instruments will ever be carried on any one Surveyor unless we have a substantial weight increase, but this represents the kinds of payloads that are being planned.

Figure 7 shows the typical Earth-Moon trajectory, including the injection phase near the Earth and the coasting phase; the dashed lines represent the portion of the flight when visibility is possible from the Goldstone, California, station. The commands to the spacecraft will be made from Goldstone. A midcourse correction maneuver is made about halfway to the Moon, about 16 hours after injection. Launch time is selected to allow visibility at the terminal end during landing.

In Figure 8 the Moon is shown with the extremes of areas for landings of Surveyor. It is much easier to effect a landing at near vertical; obviously, large velocity error problems are encountered if the approach is made at shallow angles. That is the reason for the 45° limit. The smaller area represents normal incidence area; it is an area and not a point because of the librations of the Moon and the different month-to-month relationship of its orbit with respect to the Earth.

The orbiting adaptation of the Surveyor (Fig. 9) is a fairly natural evaluation inasmuch as the interface with the launch vehicle, the injection, the attitude stabilization, orientation on the way to the moon, and every aspect of the cislunar trajectory are the same. It merely injects into a lunar orbit instead of going on to a lunar landing (Fig. 10). It would carry high-resolution cameras and many instruments to measure the environment of the Moon. Some of the immediate and later objectives are as follows:

Immediate objectives:

1. Oriented space platform for:
    a. lunar reconnaissance and topographic mapping
    b. preliminary selection of desirable landing sites for Surveyor and Apollo

2. Lunar space station to monitor radiation environment and other physical parameters in immediate vicinity of Moon

3. Selenodetic satellite for study of size and shape of Moon and its gravitational field

Later objectives:

1. Radio relay station for lunar surface communications beyond the horizon and between far side of Moon and Earth

2. Navigation satellite for position finding on the surface of the Moon.

The advanced programs in the lunar area include the Prospector concept (Fig. 11) and work associated with manned programs to determine what kind of experiments need to be done on the Moon after a manned landing.

Figure 12 indicates the planetary and interplanetary areas we study.

The bar chart given in Table II shows the time schedule of the planetary and interplanetary program. The letters V and M represent Venus and Mars, respectively, and show roughly where the opportunities for spacecraft launch occur. Experiments planned for 1962 are for launch with the Atlas-Agena. Some consideration is being given to extending the Agena program a little further and not initiating the Centaur flights until 1965 in the interest of more reliable systems.

Some landings may be possible in the 1965 and later time period with a small capsule from the Mariner; however, planet orbit or landing will certainly be possible with the larger systems that are planned for the Voyager (Fig. 13). Each of the spacecraft will carry interplanetary experiments and the new programs that we are inaugurating will extend into 1965 and probably into the late 1960's.

Figure 14 shows briefly the different types of flightpaths being considered. Initially we will attempt the Venus fly-by because it is the easiest mission. A landing mission on Mars and possibly on Venus would be the next easiest mission because the atmosphere provides continuous braking thus eliminating the necessity to carry retrorockets.

Figure 15 shows a picture of the Mariner R with an outline which basically resembles the Ranger. The high-gain antenna and solar panels are indicated, as well as a microwave radiometer, a magnetometer which can measure the fields on the way to and in the vicinity of the planet, ionization chambers, and Geiger tubes for measuring cosmic rays. The Mariner R, because of its similarity to Ranger, costs about the same or a little less than Ranger.

Figure 16 shows the trajectory of the Venus shots during 1962. It is fairly simple to show where Venus is during the firing window and where the Earth is during the same firing window. The trajectory shown will allow us to be communicating over a distance of 35 million miles at the time of encounter. Such variables as this affect our planning.

In the planetary program, the constraint on time of launch is the most severe variable, because we have to be ready when the planets are in the proper position and that means that times of launch are predestined. An interesting point also is that wherever we launch within this window, the spacecraft will arrive at Venus at about the same time.

The facilities required to carry out this spacecraft development program may be enumerated as follows:

- Systems and subsystems functional testing:
  - Guidance and control—
    - Gas bearing test table
    - Celestial simulator
    - Optical research laboratory
  - Telecommunications—
    - Antenna testing range
    - Electronics laboratories
  - Spacecraft propulsion test cells
  - Spacecraft materials testing laboratory
  - Spacecraft structural testing laboratory
  - Spacecraft assembly facility

Figure 17 is a cutaway view of the simulator which is under final construction at JPL for environmental tests of these spacecraft. The chamber has a test area which is about a 25-foot-diameter circle. This chamber is not yet completely through development. It is a technological step in itself; however, it is being used part time for Mariner testing. There is a source lamp and lights that provide illumination hopefully equivalent to the solar constant at Venus; these are thus capable of easily simulating the solar constant at Mars. The simulator will provide, hopefully, approximately 10^{-7} to 10^{-9} millimeters of pressure, which means

300,000 feet, at least, in altitude, and will allow study in the space and thermal environment anticipated. In these planetary shots study of such effects is very important in order to determine before actual flight the ability of the spacecraft and its components to withstand expected conditions.

The data acquisition phase of this program is carried out by three stations: one at Goldstone, California, one at Woomera, Australia, and one at Johannesburg, South Africa. (See Fig. 18.) These stations not only receive data, but provide for transmitting of commands to spacecraft. A facility is being prepared at JPL to display the performance of the spacecraft and to analyze and send the proper commands. We will have a tremendous science data display for immediate analysis. In the case of the Surveyor, for example, experiments will be commanded from Earth on the basis of data received.

The supporting research program totals about $6 million in 1962 and will be on the order of $8 to $9 million in 1963. This is the part of our program that is directly monitored and sponsored by the scientists out of Washington. We also plan about twice this amount for research and development, both scientific and technologic, at the centers. We cooperate with other offices, for example, in such matters as exobiology, where there is mutual interest. Effort is expended in a variety of disciplines at several universities, Government laboratories, and non-profit organizations. This is an on-going program which includes very basic research as well as the development of experimental instruments.

We have done a great deal of work on the probable returns of these programs. For example, simply multiplying a number of probabilities together, as we do in the case of launch vehicles, gives an idea of how many of a total number are going to be successful. One must consider this in planning; for example, if one in three missions are expected to succeed, the question arises, should you plan a program of three flights for the same purpose and accept duplication if more than one should work, or should you plan that every one is to be different and hope that at some later point in time you will accomplish what may have been the most important mission. The problem is very difficult and it seems impossible to arrive at a good answer because of the statistically small number of samples involved. It would be an advantage if the lead times were not so great; then, perhaps, the course of action could be changed as a result of the previous flight. In almost every case there is no time to schedule subsequent shots after a failure or a success. The lunar landing problem is one not yet fully appreciated, and we must expect some difficulty in achieving that technological objective. A lunar spacecraft must not only go through the critical launch period on Earth, it must essentially repeat this process at the Moon.

In relation to other programs, we are working closely with the manned-flight staff so that our answers will fit their needs and so that they know of and can use the data we obtain.

LUNAR PROGRAM

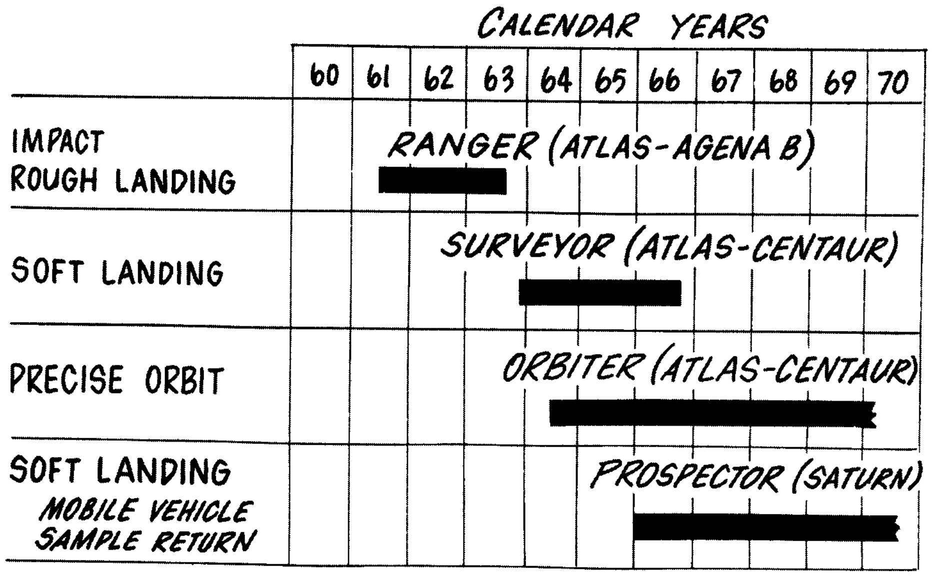
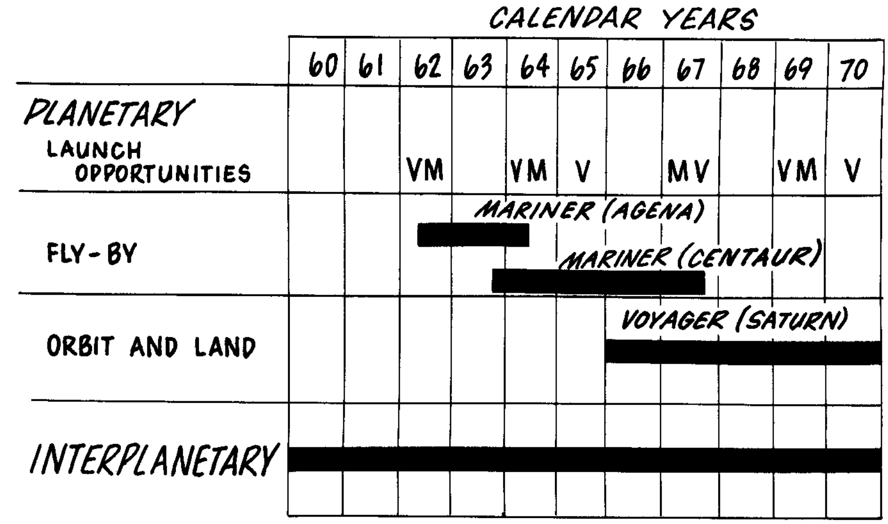


Table I

PLANETARY AND INTERPLANETARY PROGRAM



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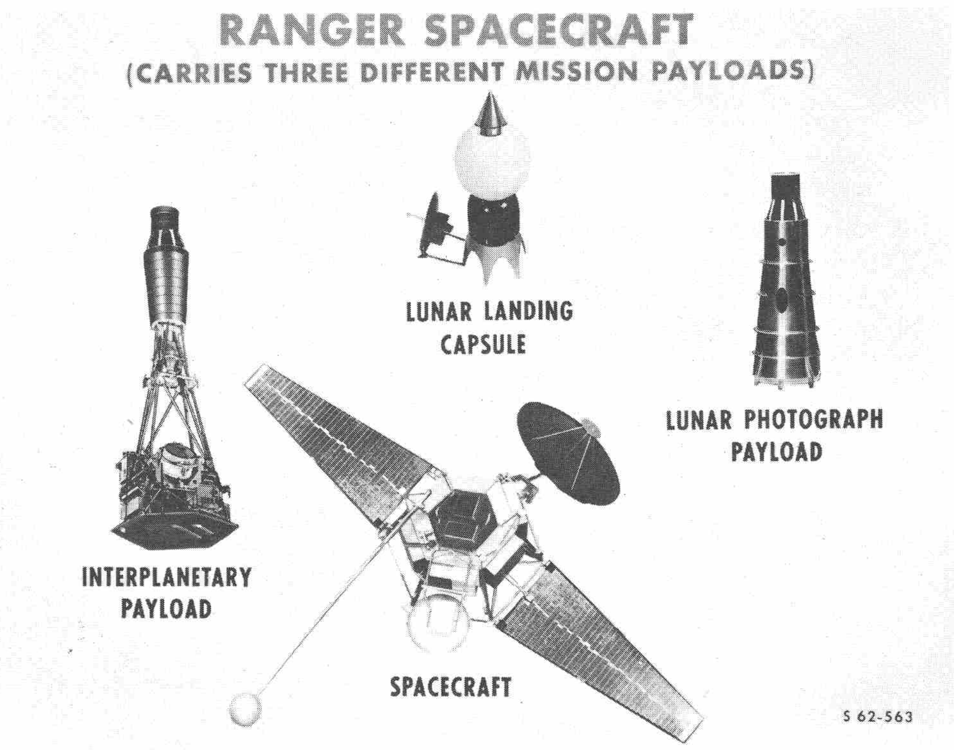


Figure 1

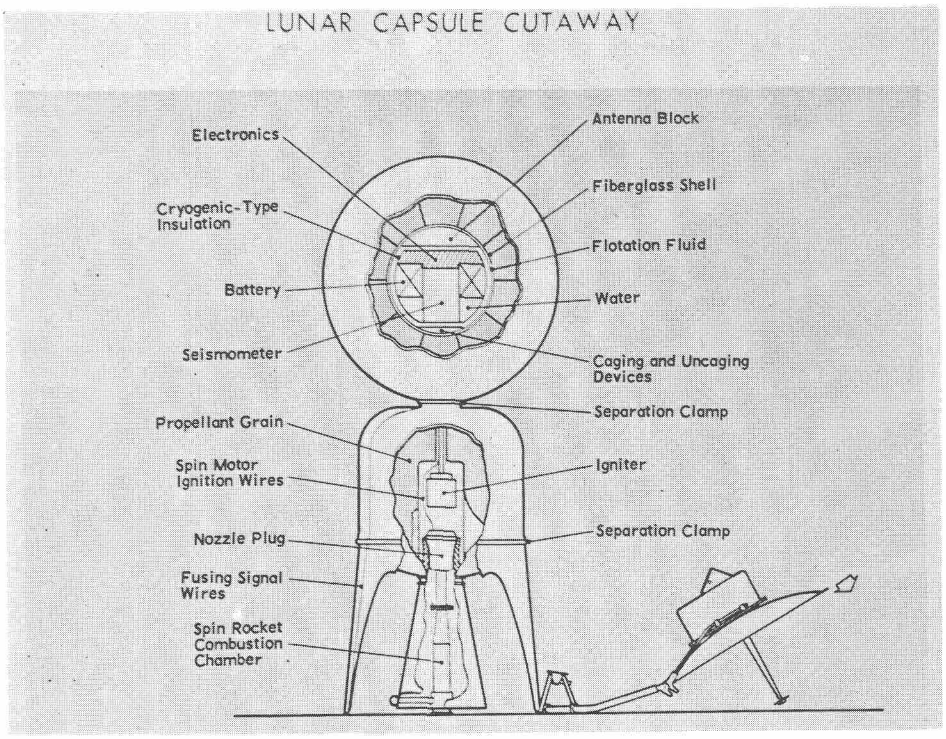


Figure 2

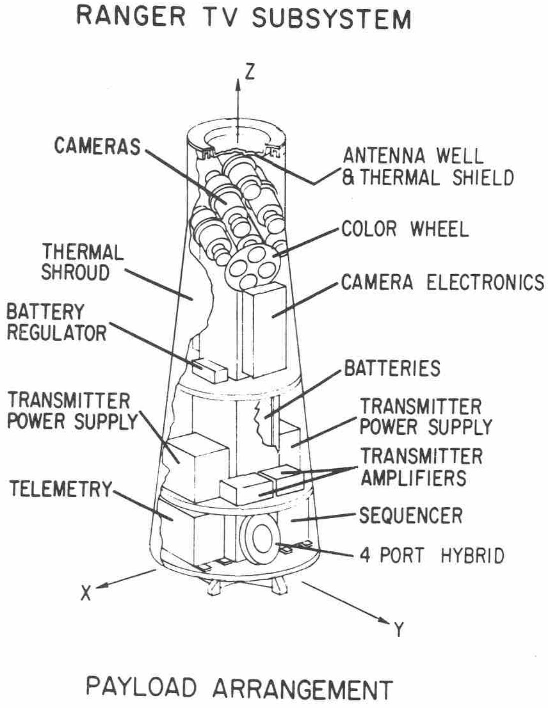


Figure 3

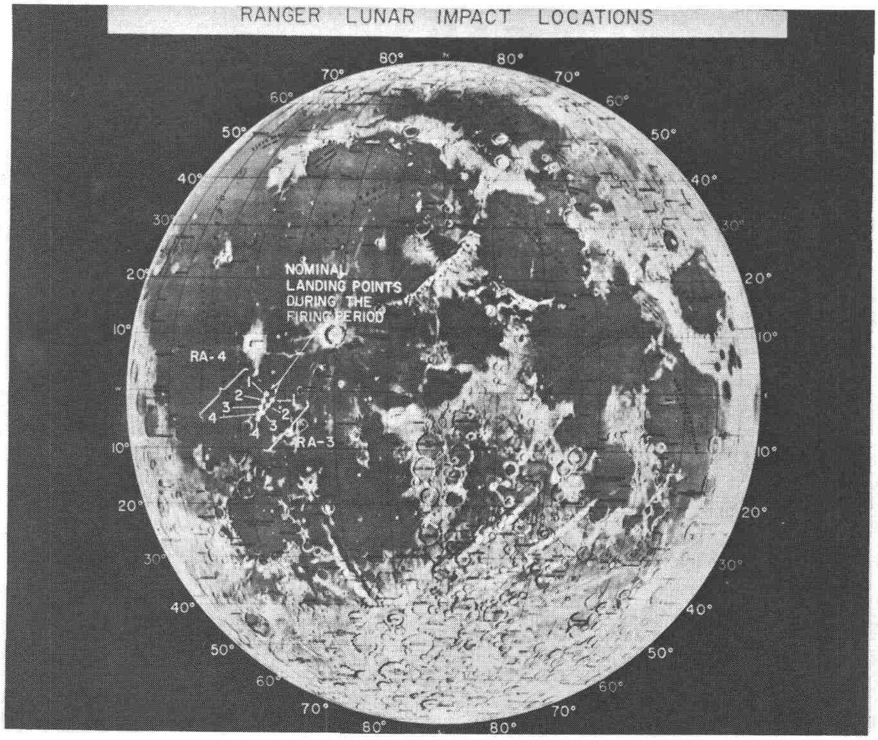
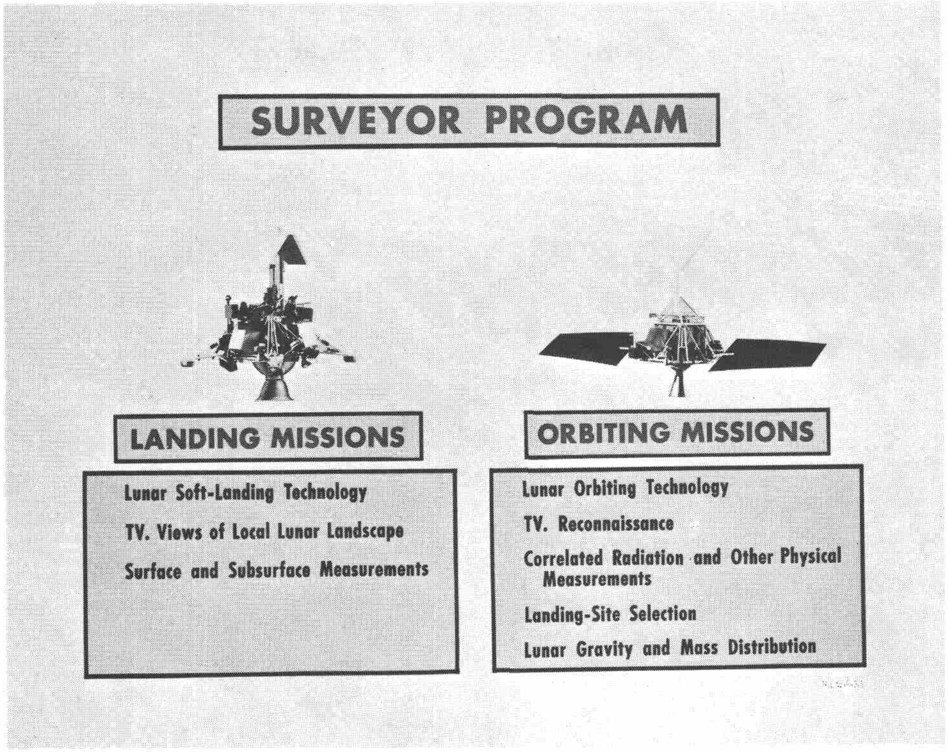


Figure 4



SURVEYOR PROGRAM

LANDING MISSIONS

Lunar Soft-Landing Technology

TV. Views of Local Lunar Landscape

Surface and Subsurface Measurements

ORBITING MISSIONS

Lunar Orbiting Technology

TV. Reconnaissance

Correlated Radiation and Other Physical Measurements

Landing-Site Selection

Lunar Gravity and Mass Distribution

Figure 5

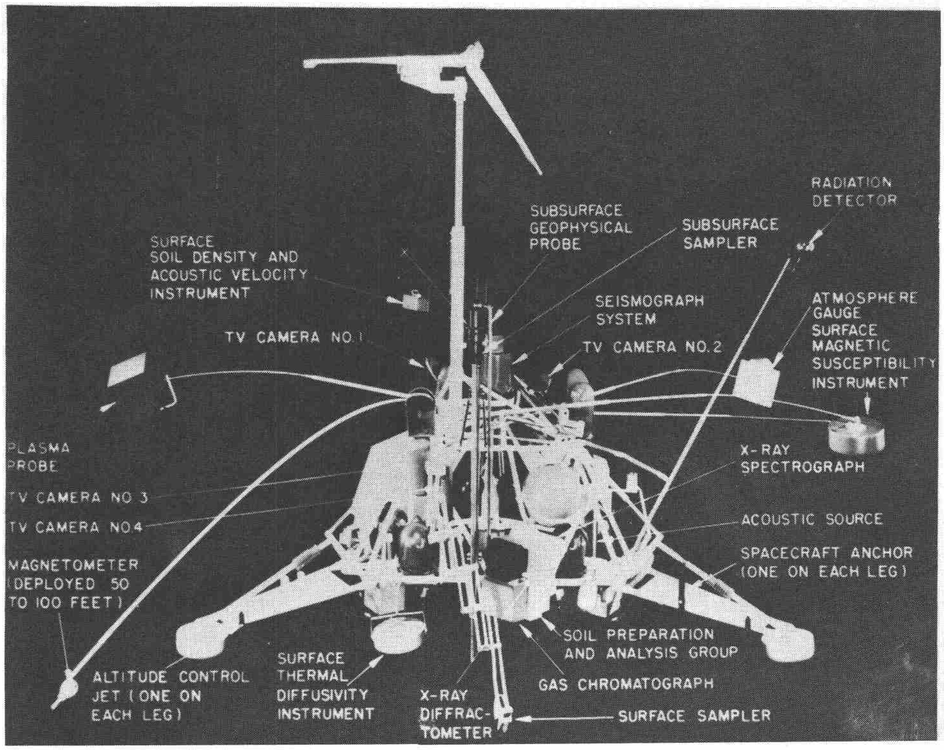


Figure 6

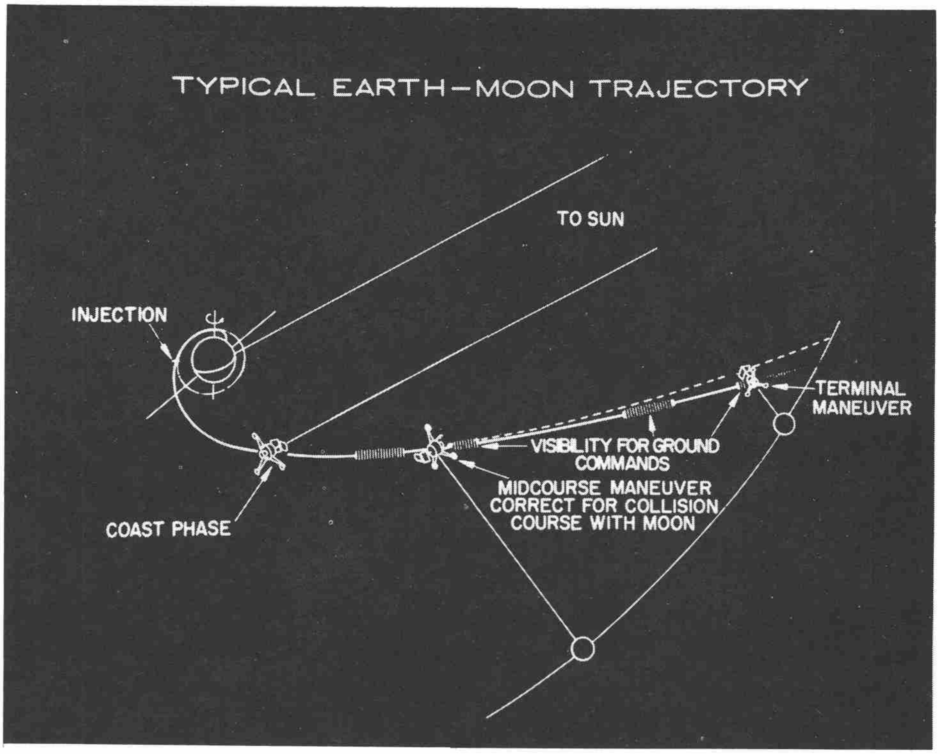


Figure 7

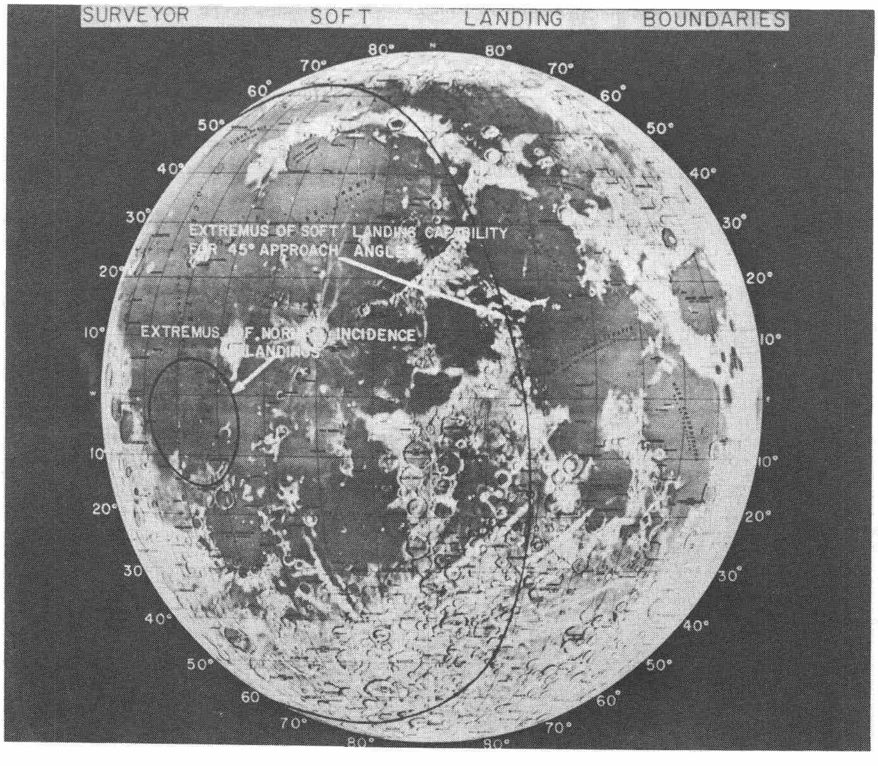


Figure 8

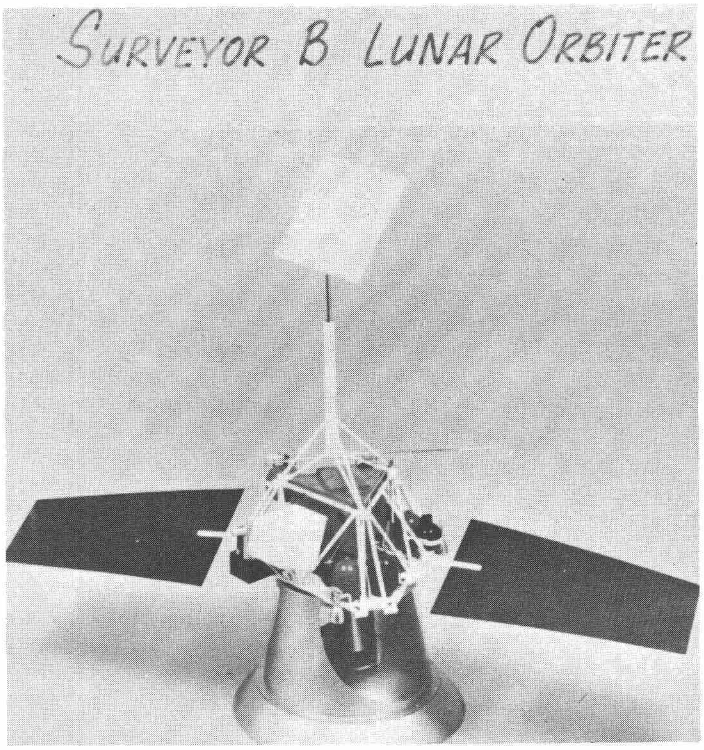
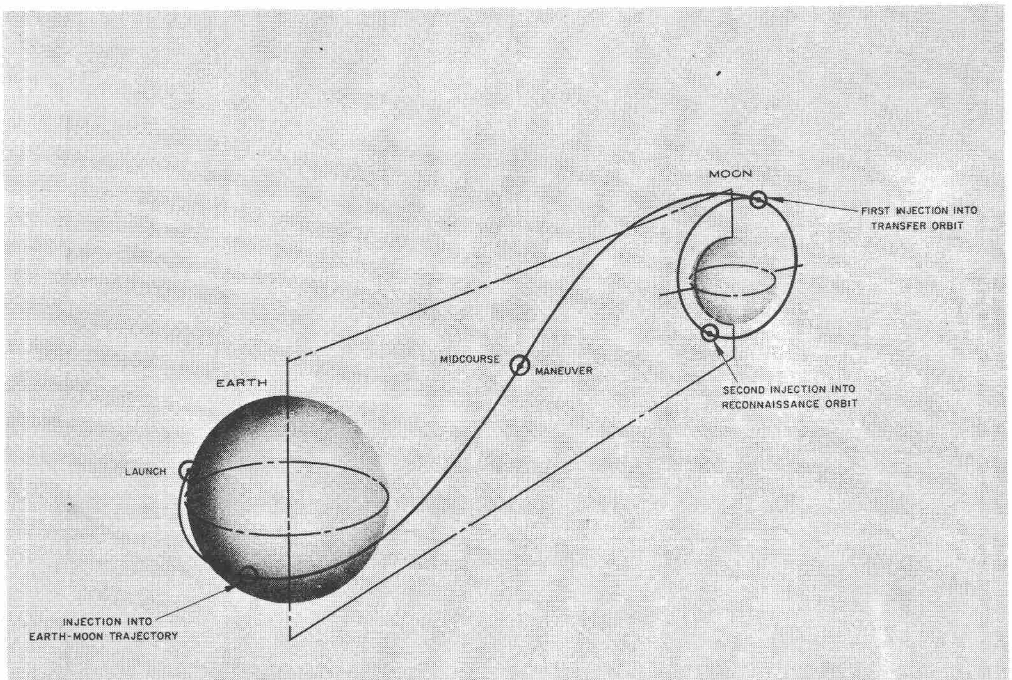


Figure 9



Transit and Injection Trajectory

Figure 10

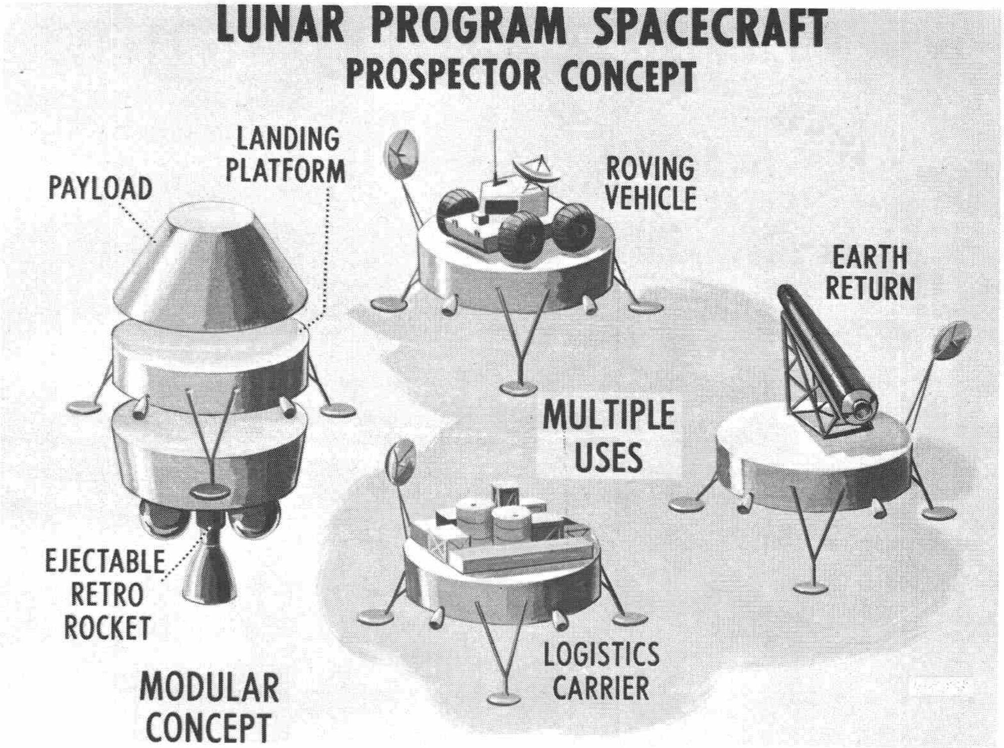


Figure 11

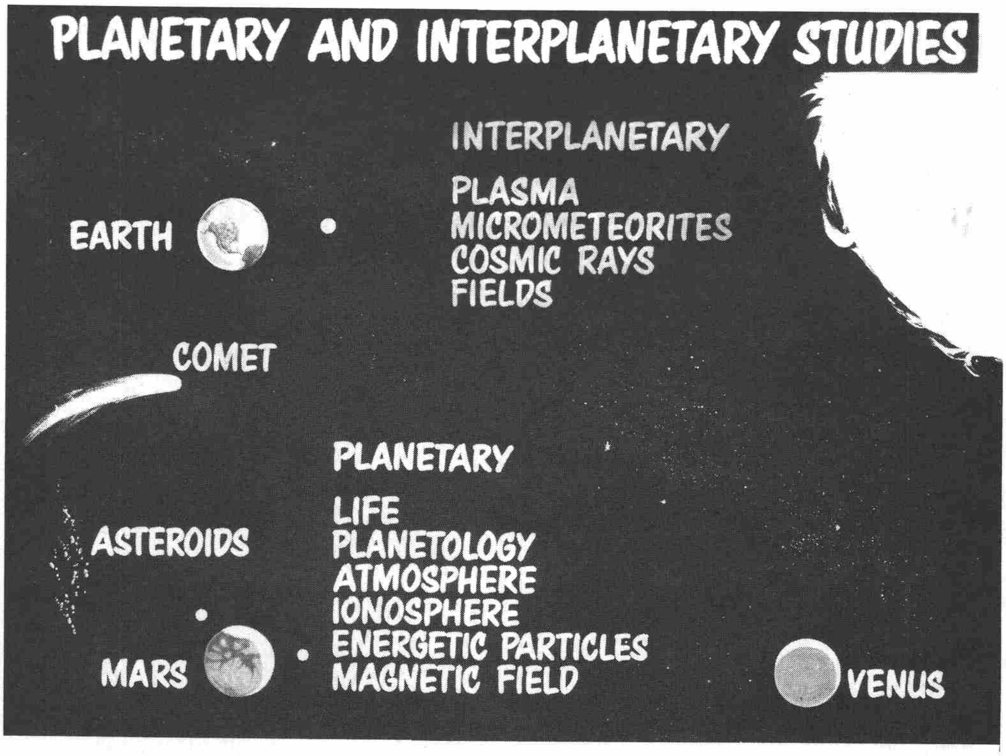
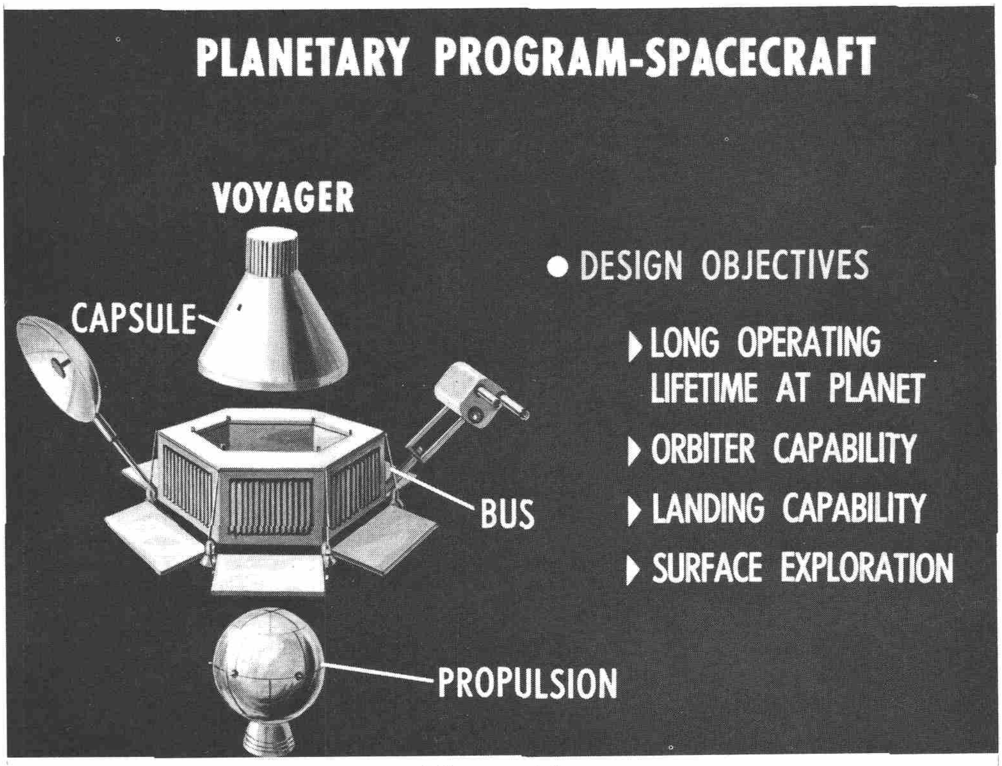


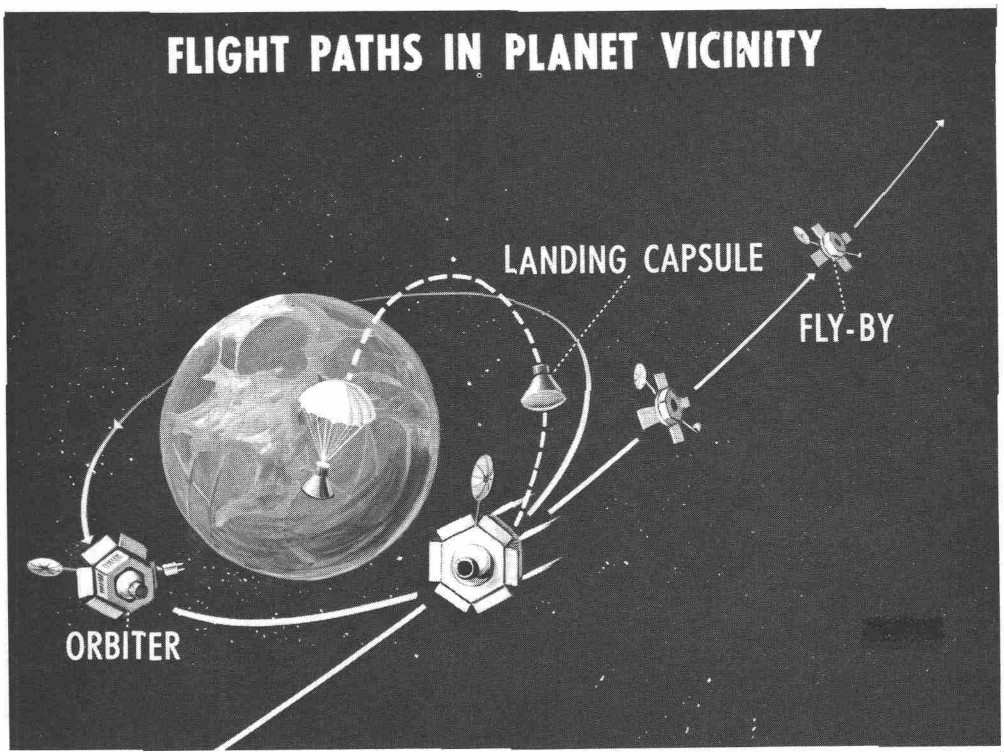
Figure 12



PLANETARY PROGRAM-SPACECRAFT

- DESIGN OBJECTIVES
  - LONG OPERATING LIFETIME AT PLANET
  - ORBITER CAPABILITY
  - LANDING CAPABILITY
  - SURFACE EXPLORATION

Figure 13



FLIGHT PATHS IN PLANET VICINITY

Figure 14

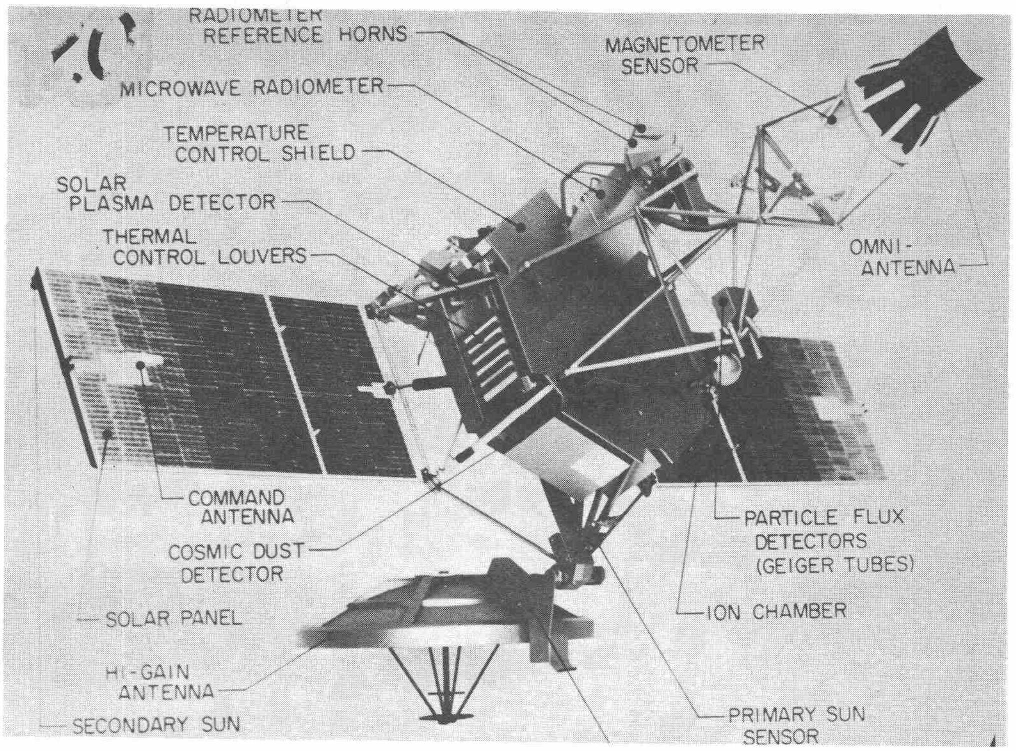


Figure 15

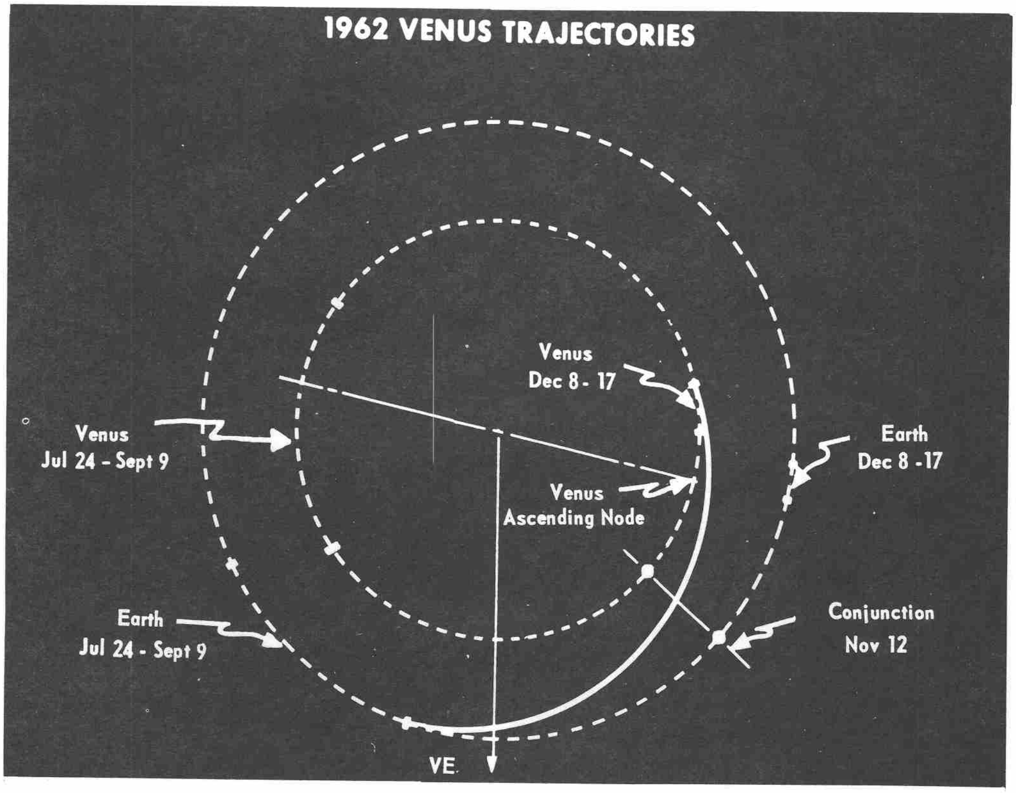


Figure 16

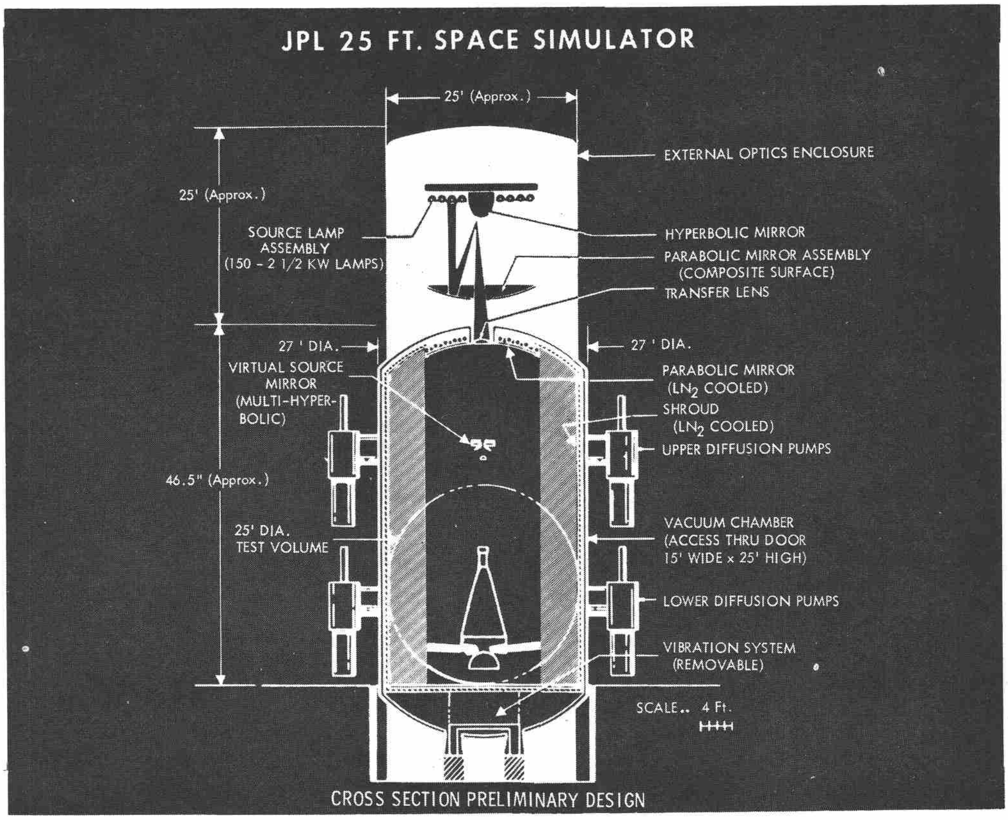


Figure 17

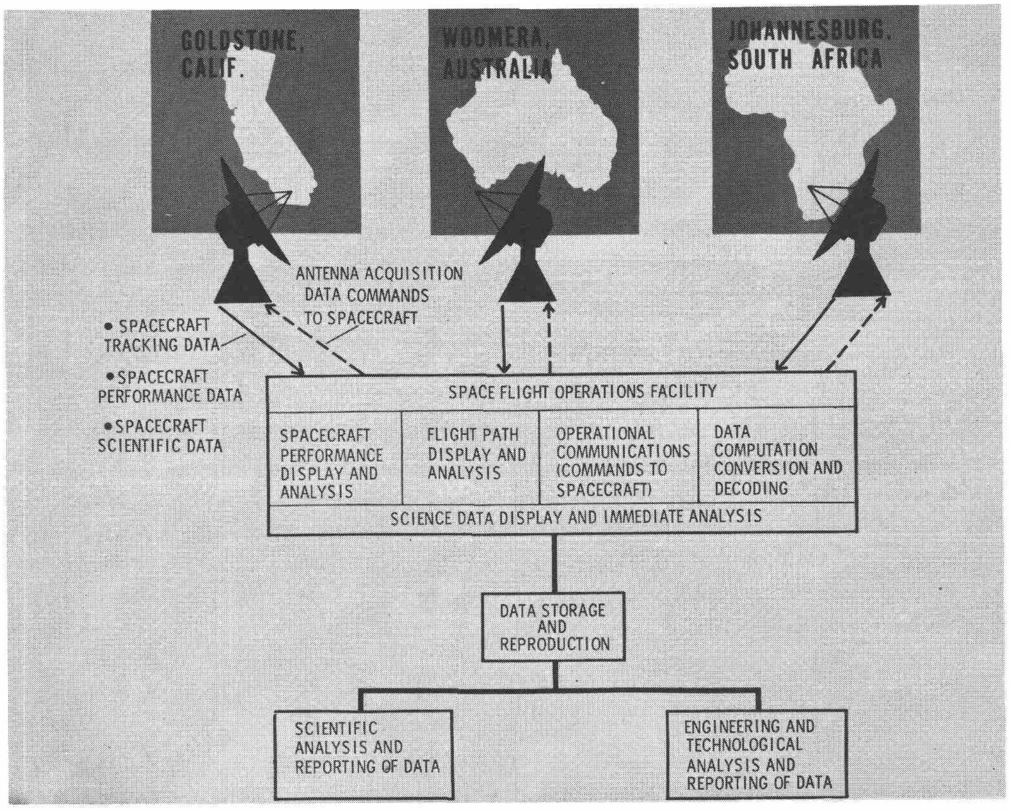


Figure 18

PROGRAM OF THE OFFICE OF TRACKING AND DATA ACQUISITION

By C. R. Morrison

Tracking and data acquisition is considered a support function in the over-all NASA program and, as such, responsive to the requirements of the experimenter, among others. It may seem somewhat of an anomaly, but we are also users of our own tracking and data acquisition facilities in order to determine where to point downstream facilities. The support of the space program requires us to carry out normal operations as well as a research and development program so that we are able to meet future space flight projects.

The tracking and data acquisition facilities, personnel, and developmental program are only one of the many tools to meet the scientific goals generated by the experimenters participating in the various massive space flight programs. Since tracking and data acquisition is a tool for the experimenter's use, just as the launch vehicle and the spacecraft are, it is important to consider the constraints that this tool places on the over-all program.

The basic types of these constraints are described briefly as follows. First, there are the technical constraints; that is, those constraints placed on the experiment due to the limitations of the present state-of-the-art in the ground data tracking and acquisition systems.

Second are the economic constraints and these are very closely related to the technical constraints. Economic constraint means the dollar constraints placed on the total cost—both the initial cost and the long-term continued or operating costs of the tracking and data acquisition complex adequate to meet all the NASA Space Flight Program.

Third are the time constraints. These timing or schedule constraints are of many different types. One of the most serious of this type is the time required for the development of a new system, the installation, and the checkout which may be necessary to meet a future space flight program. So the lead time we require includes the development time, the procurement, installation, and checkout time for the facilities. In general, the development time of new data acquisition system is equally as long as, and in some cases longer than, the development time of a spacecraft for which it is designed to be used. Thus, it is important not to forget this time factor when specifying the data system requirements.

Fourth are the operational constraints which must be considered in the planning of future space exploration programs. These operational constraints are of many types. For example, consideration must be given to the overall workload of a particular network: Are you going to overload the network beyond its capabilities with this program? Additional operational limitations include consideration of such items as the launch window and the degradation of inaccuracy and of performance of the over-all system in a hostile field environment.

Fifth are the political limitations with regard to the establishment of new facilities on foreign sites to accomplish specific mission objectives.

It appears to us in the field of tracking and data acquisition that all too often those responsible for planning future spacecraft exploration programs do not adequately consider these problems. It is, in my opinion, not sufficient for the experimenters to say we need this or that without adequately considering the problems of total workload scheduling on the network, suitability and availability of adequate data acquisition facilities, limitations of the launch area instrumentation, launch-window restrictions, realistic accuracy requirements regarding position, velocity, and data rate, reliability and suitability of ground facilities under adverse field conditions, and cost of developing installations and operating data acquisition facilities as well as reducing and processing the voluminous data which will be obtained from such experiments.

In planning for future spacecraft missions these problems should be considered in proper perspective in requesting the support which is required by the space flight program. In short, the spacecraft experimenters must consider as part of the over-all system those data requirements along with peculiarities of the launch vehicle, the special onboard experiments including the onboard processing equipment as well as the ground data processing, and the distribution of the final report.

Figure 1 shows the basic elements of a global tracking and data acquisition system. It includes the launching and insertion tracking and data acquisition. The on-orbit tracking and data selection includes telemetry, data by means of either electronic or optical systems, the Earth communications system from the data collecting point back to the control center, and the network control and computations at the control center.

Figure 2 shows the typical flow of data in a network from the actual tracking and data acquisition site, on the left side, through the appropriate communication lines and communication devices to the control and computation centers. Then, on the right, the user groups for such data are given. Because of the diverse requirements of the different user groups, the actual data processing is quite a laborious task in the total scientific program of NASA. Briefly, NASA has four basic networks which are used for obtaining data on the various NASA programs. These networks are as follows: The Smithsonian Astrophysical Observatory Baker-Nunn camera network operated throughout the world for obtaining optical data on near-Earth satellites (Fig. 3); the present manned space flight network (Fig. 4); the high-gain antenna stations, which comprise the deep space network (Fig. 5).

The basic minitrack network is shown in Figure 6. In addition, there is now a second facility at Fairbanks, Alaska, which is an 85-foot antenna XY-mounted for obtaining the data on the Nimbus meteorological satellite program. We are in the process of installing a similar antenna in the mountains of North Carolina.

The over-all planning for one network in order to insure our capability to support approved projects requires answers to the three questions of what is required, where is it required, and when is it required. It should be emphasized that such detailed planning by us comes only after all the projects have been finally approved. For us to accomplish this planning in an orderly manner it is important that we have early knowledge of and participate in the decisions made with regard to the future spacecraft projects. For simplicity, the planning that we must do for the unmanned lunar and planetary program has been chosen as an example.

Figure 7 shows the schedule at one point in time of the unmanned lunar and planetary program. We start with this as the basis of our planning to answer the questions of what is required, where, and when. This is the basis of when.

Figure 8 depicts what the actual requirements are in terms of total coverage of the Ranger program. This indicates, along with other data, what is required and, in this particular case, when it is required, because the data acquisition contact per day defines this in the case of the lunar program.

Figure 9 shows the coverage requirements for Mariner. Again, the variations of the actual data acquisition contact per day during the lifetime of the spacecraft are shown.

The detailed workloading for the Calendar Year 1962 is shown in Figure 10 and was based on the previous planning schedule. For 1963 (Fig. 11), we do not have much actual requirement per day per station. In 1964 (Fig. 12) we are requiring upwards of 24 hours a day for coverage by a given station. This means that we are going to have to duplicate our facilities by 1964 at the deep space station locations. To do this we will have to buy them now. By 1965 (Fig. 13) the situation gets a little worse, and by 1966 (Fig. 14) the situation becomes very critical.

The problem of this lead time and of locations may be reviewed briefly as follows: The installation of the additional facilities to meet the increased workload occasioned by the larger number of spacecraft planned in the future points up a political consideration. We have stations located in foreign countries and operated by the host country, in the case of the deep space stations. We have answered the first question of what is required by saying that we require duplicate facilities at these stations in order to handle the total workload. We must next answer the question of where they are required. Three of these stations are required equally spaced around the globe. One of our locations in South Africa at the moment could be severely questioned by politicians as to whether we want to invest approximately $5 million in additional funds to install a second facility in that host country. The long-term political stability of this host country, for example, must be seriously considered before the decision is made to install the additional antenna. Alternate locations and penalties which the spacecraft experimenter might have to pay must be considered and evaluated before the decision as to the actual location of this station can be made. Due to the lead time required for the installation of this facility, the decision regarding its ultimate location must be made sufficiently far in advance to avoid any program slippage. The lead time for the facility includes the funding availabilities—that is, the time it requires for us to request and obtain the funds from Congress and to negotiate with the host country for permission to install the station there. This can in some cases take up to a year and a half to negotiate. For the deep space network, even though we plan to install a duplicate of an existing facility, it is necessary for us to plan this facility now in order to meet the anticipated workload in 1964. We have actually requested funds for this facility in the FY 1963 budget and, if approved, in order to meet the scheduled date of mid-1964 we will have to start constructing approximately in August 1962. In this particular case, then, the constraint, the political as well as the time constraint, is directly applied.

Figure 15 shows the anticipated work increase as an increase of data rate in bits per second over the calendar years of the future. Pioneer V had an extremely low data rate. Then follow the data rates for Ranger I, Ranger III, Ego, and Prospector. The high data rate systems are those of the communications satellite programs.

Actual missions of the future are going to have requirements to increase the information transmission rate from the spacecraft to Earth for:

a. Prospector:
    Real-time TV

b. Voyager:
    Higher data rate and TV from Venus and Mars;
    Adequate data from Jupiter and beyond

c. Manned lunar landing:
    Real-time TV

Real time means approximately 30 frames per second.

How are we planning to meet this particular trend of the future? The lower curve of Figure 16 represents a stepwise program of planning for increasing the capabilities of our basic 85-foot data acquisition stations. Each improvement is a significant step until we reach a plateau in approximately 1966 beyond which it does not appear economically or technically feasible to continue increasing the capability of this basic system. By 1964 we should have started on the next basic facility, a 210-foot antenna. Actually, studies for this antenna started some two years ago. We have requested funds from Congress in FY 1963 for the intiation of the construction of this facility. We hope to start the developmental testing program by 1964. The actual construction will have been completed by that time and the normal growth improvements will begin.

In summary, then, the future of these spacecraft missions will be satisfied by improvements of the basic 85-foot data acquisition network and by the installation of 210-foot antennas, the first of which we hope to start developing with FY 1963 funds (Fig. 17). The 210-foot paraboloidal antenna will have a basic Cassegrainian structure for speed. We hope that this antenna will have an upper frequency operating capability in excess of 3,000 megacycles in winds up to 65 knots at all thermal conditions. We will install the antenna in regions where we will not encounter ice, and thus avoid this difficulty. The anticipated pointing accuracy will be, it is hoped, approximately 0.01°. The total system temperature is approximately 40°. This is composed of an antenna temperature of about 7° and the remaining 33° are assigned to the maser and the other plumbing associated with the antenna.

When completed this particular antenna will be the Free World's largest, most accurate, and have the best capability of any antenna of its type.

Figure 18 shows the details of the various improvements we have planned for this program, and how we hope eventually to achieve total system capability. We have increased the power aboard the spacecraft. For example, in one particular case shown, we go from 960 megacycles to 2,300 megacycles. We increased the gain aboard the spacecraft from 18.8 decibels to 26 decibels. We increased the gain on the ground and thus the system temperature was lowered. In this time period shown we increased the power aboard the spacecraft to 100 watts and in this fashion we advance the program.

We are not planning to use 8,000 megacycles. We have a frequency band assigned by ITU, but we are not instrumenting for this particular band, because the atmospheric window within which we must work to these distances limits us to some frequencies below approximately 8,000 megacycles.

In order to meet the requirements of the other programs of NASA, improvements will be made in the remaining tracking systems; these are very briefly summarized as follows. We will in the manned space flight network, for example, in order to meet the requirements for the one-day Mercury, Gemini, and Apollo flights, add such devices as the installation of real-time TCM telemetry at the site, data processing at the overseas site, and the necessary readout devices. We will have to add improved command capability as well as expand the basic FM-FM telemetry acquisition to handle the increased telemetry workload of these future programs. At some time in the future we will have to install additional high-gain data acquisition facilities of the type currently employed in the deep-space network in order to obtain tracking and telemetry data from the Apollo spacecraft during the lunar transfer. We are also planning the addition of ranging capability to our ground stations to provide increased tracking accuracy and improved orbit determination. This ranging capability which we will add will have a time equivalent resolution of approximately 20 meters.

In order to accommodate the increased geographical coverage resulting from the missions of longer durations then currently planned for the Mercury program, additional stations—some probably aboard ship—will be required. This brings us into a new host of requirements with regard to real-time communications to these ships. In addition to the ship stations, it may be necessary, if required for increased geographical coverage, that the program develop to provide additional land stations. The basic plan we have to meet the requirements of the manned space flight program place heavy reliance on the existing manned space flight network capabilities; also such augmentations as are necessary to meet the actual program objectives must be provided. A particularly important problem in planning the ground instrumentation system necessary to support the manned space flight program is the lack of stability and definition of the requirements toward which we must work. Due to the peculiar nature of this program, requirements on the ground instrumentation system are constantly changing, which makes planning for this almost impossible. This particular problem is also present in the other NASA programs, but fortunately to a lesser degree. In the Earth satellite network, or the basic minitrack network, we are planning improvements similar to those enumerated for the manned space flight network. We will, for example, install, at selected minitrack sites, an Earth satellite range and range-rate system, which is currently under development; its designed objective is a range resolution equivalent to approximately 10 meters in time. We will install additional telemetry receiving equipment, both of the TCM and FM types to handle the increased workload. We will, in addition to the high-gain 85-foot antenna stations in Alaska, install a high-gain data acquisition station at Rosman, North Carolina, and one at an unspecified location in the Far East. These three basic stations will then constitute our high-gain data acquisition facilities to accommodate the observatory class of satellites, that is, the Ego, Pogo, OAO, Oso class. We will also install at three sites in the Southern Hemisphere, that is, South Africa, Australia, and South America, medium-gain antennas of approximately 40 feet in diameter capable of receiving data from these satellites. We will install semiautomatic data-processing facilities at Goddard Space Flight Center to accommodate the significant increase of data to be acquired from the observatory class of satellites. We will also have to add to the computational and communications system in order to support these future programs.

It should be noted that in the future planning we have not considered the establishment of a large number of additional stations at sites not now occupied by NASA. We are planning to consolidate, wherever possible, tracking and data acquisition capabilities at the sites we currently occupy. It would be desirable for the spacecraft program planners of the future to design their systems so that additional sites will not be necessary to provide the additional scientific data required. The political as well as the economic and logistic considerations are largely responsible for these plans. To illustrate the economic consideration only, our tracking and data acquisition operating budget has increased threefold from approximately $44 million in Fiscal Year 1961 to approximately $158 million in FY 1963. This does not include the cost of new facilities. This, therefore, means that it is doubtful that we can provide a large number of new data tracking and data acquisition facilities throughout the world. Finally, it should be reemphasized that in the planning of future spacecraft missions the tracking and data acquisition constraints must be considered in their proper perspective if we are to obtain the scientific results hoped for in a technically sound, economically feasible, and operationally practical manner.

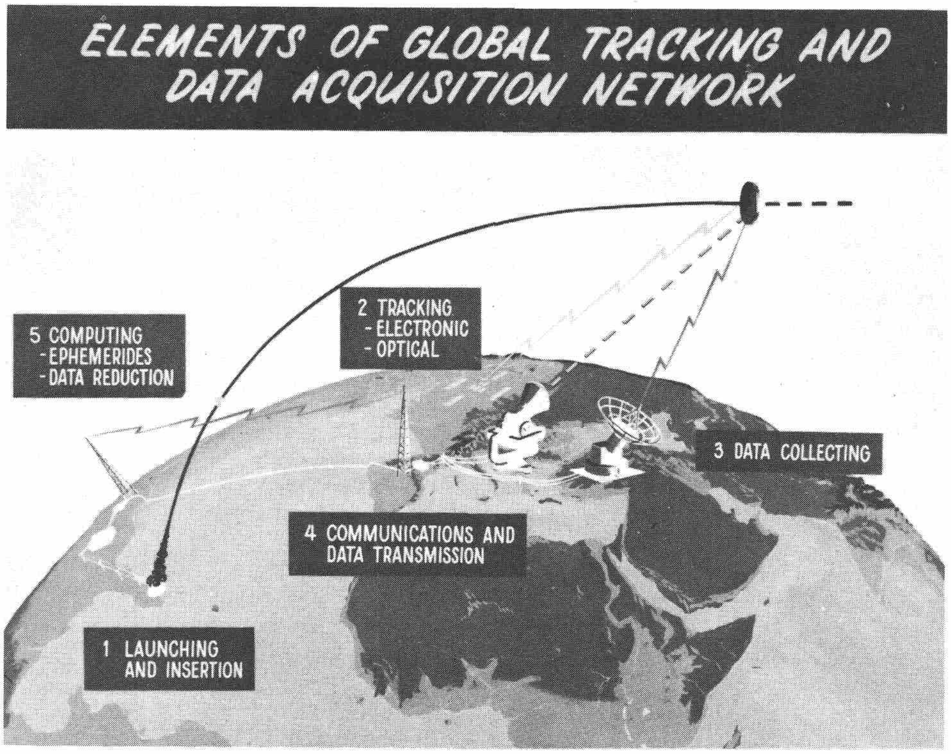


Figure 1

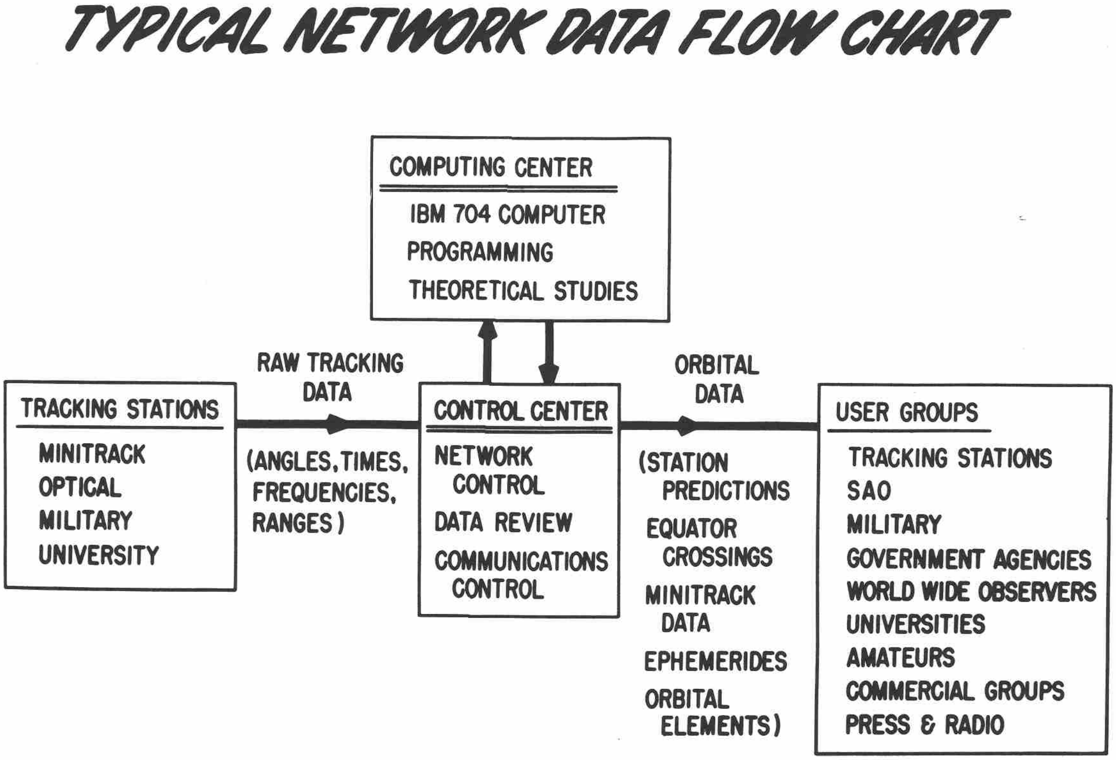
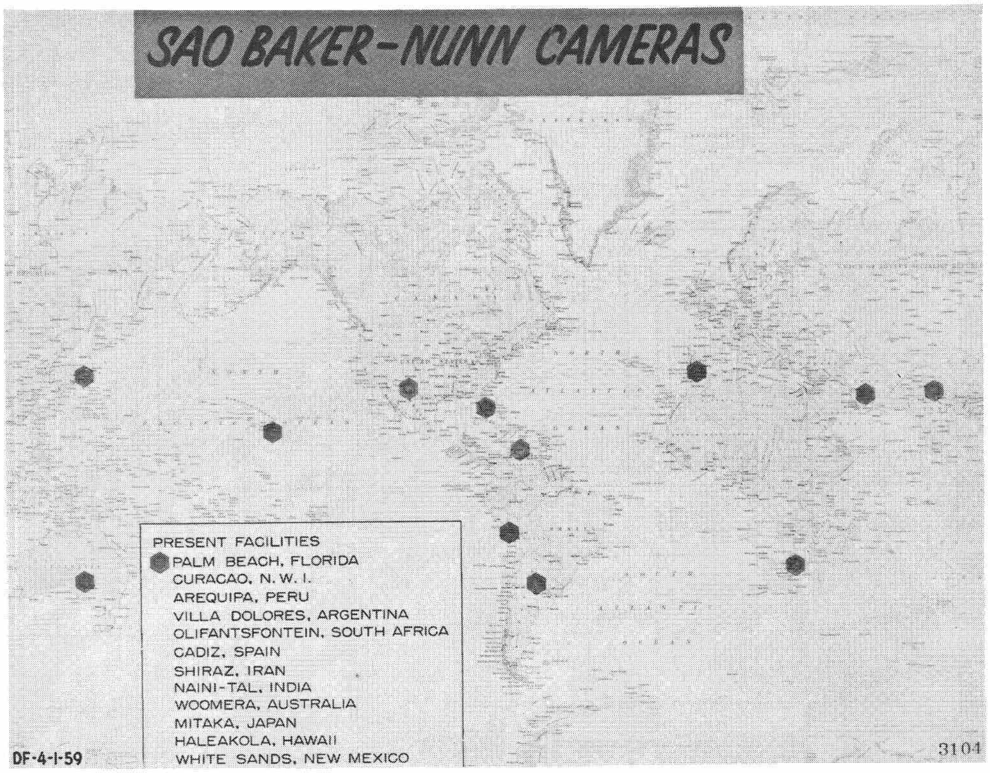


Figure 2



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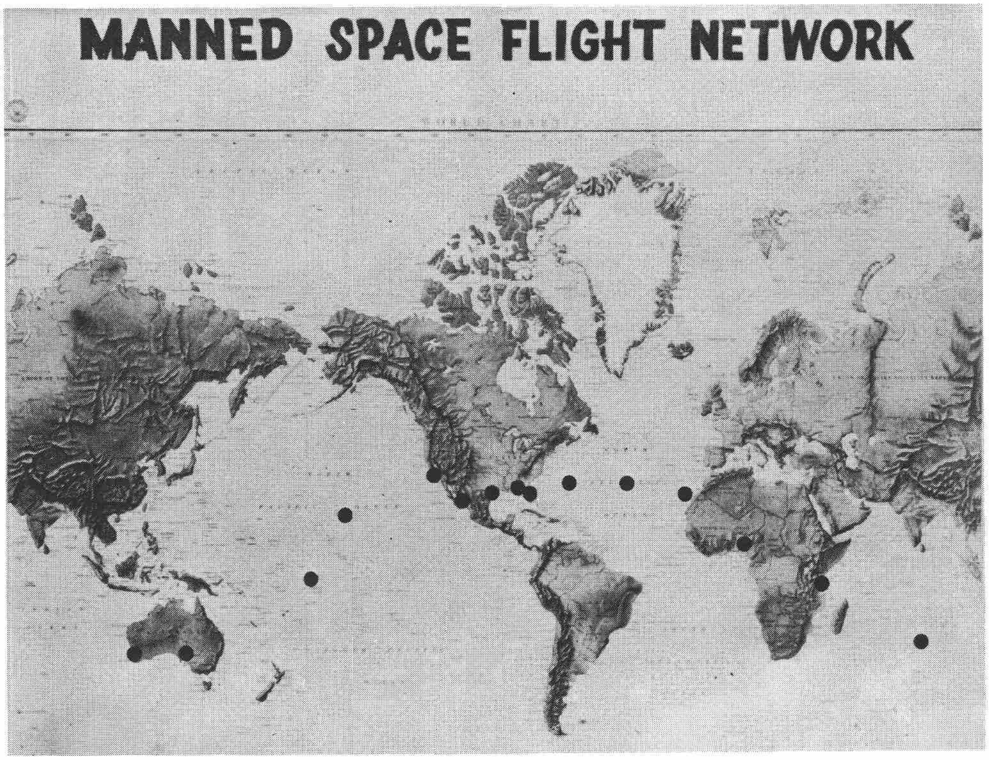

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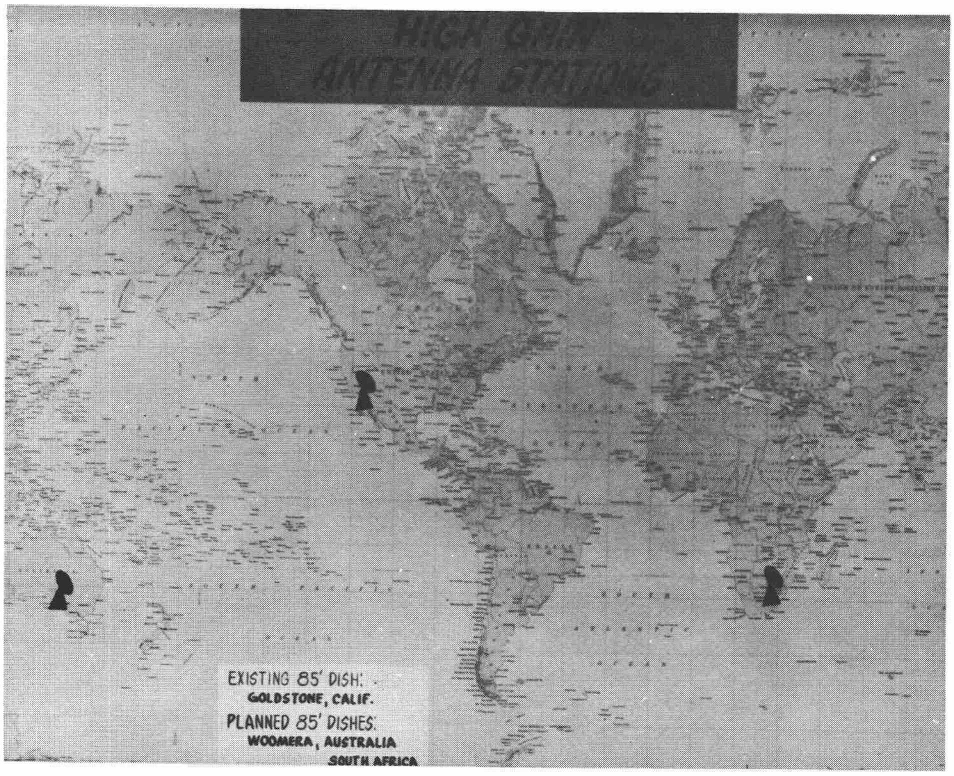

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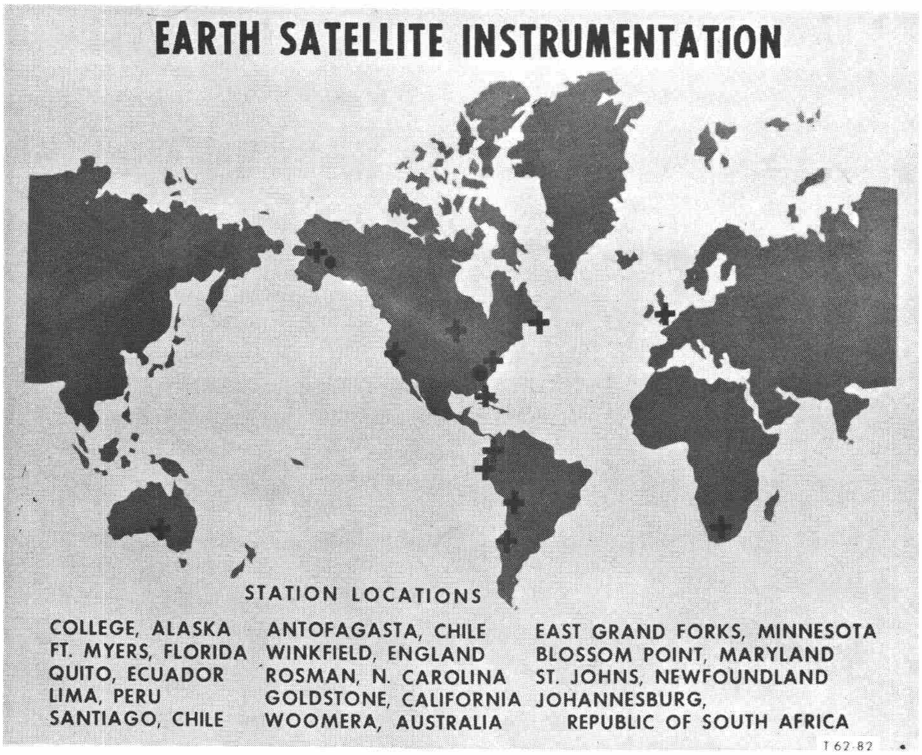


Figure 6

SCHEDULE FOR LUNAR AND PLANETARY EXPLORATION

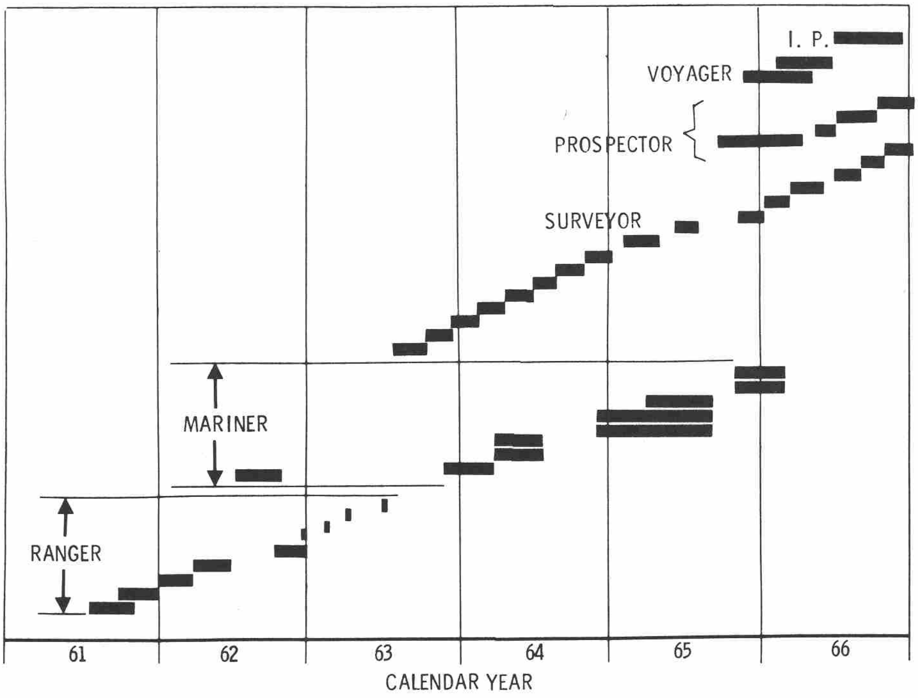


Figure 7

RANGER COVERAGE REQUIREMENTS

MISSION	MISSION PERIOD	DATA ACQUISITION CONTACT PER DAY
RA-3	0 - 90 DAYS	24 HOURS/DAY
RA-4 &5	0 - 15 DAYS 15 - 90 DAYS	24 HOURS/DAY 15 HOURS/DAY
RA-6, 7, 8, 9	0 - 5 DAYS	24 HOURS/DAY

Figure 8

MARINER COVERAGE REQUIREMENTS

MISSION PHASE	MISSION PERIOD	DATA ACQUISITION CONTACT/DAY
LAUNCH TO MIDCOURSE	0 - 5 DAYS	24 HOURS/DAY
CRUISE	5 - 85 DAYS	4 HOURS/DAY
ENCOUNTER	85 - 90 DAYS	24 HOURS/DAY
POST ENCOUNTER	95 - 120 DAYS	2 HOURS/DAY

Figure 9

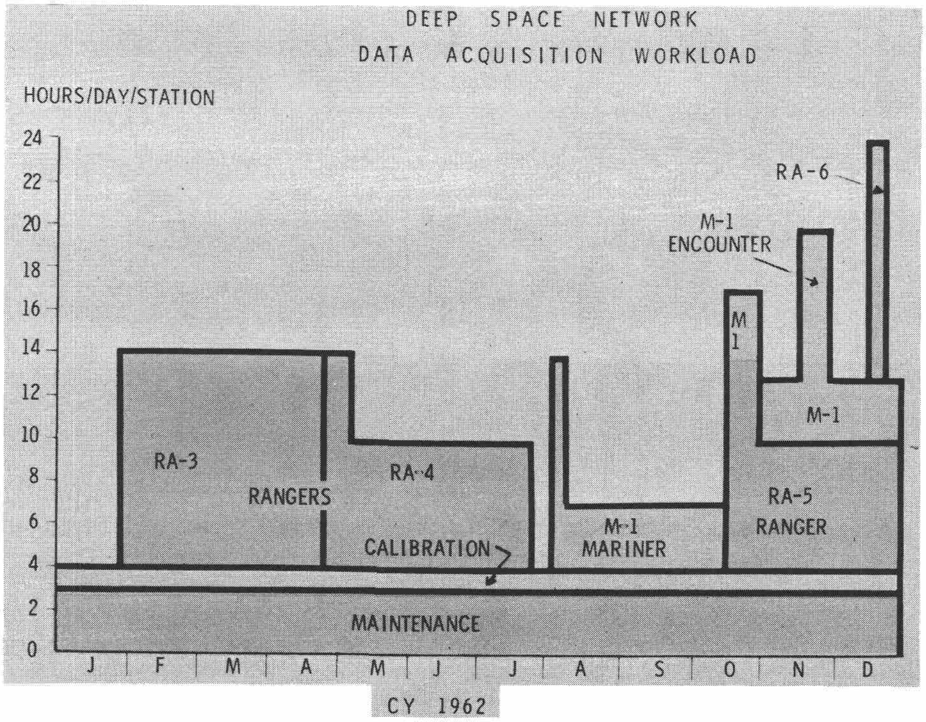
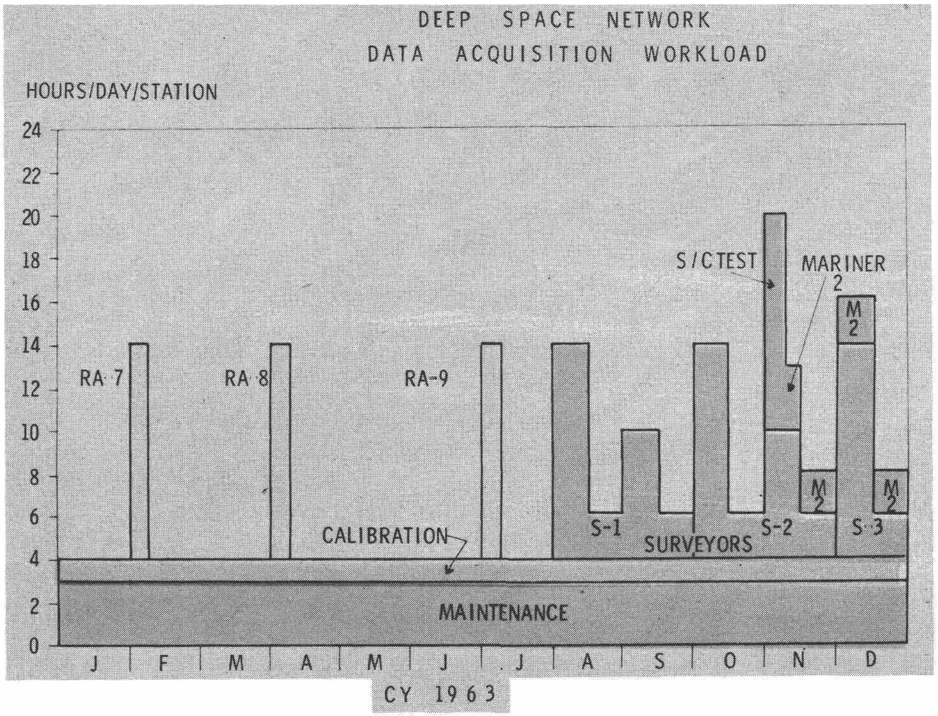


Figure 10



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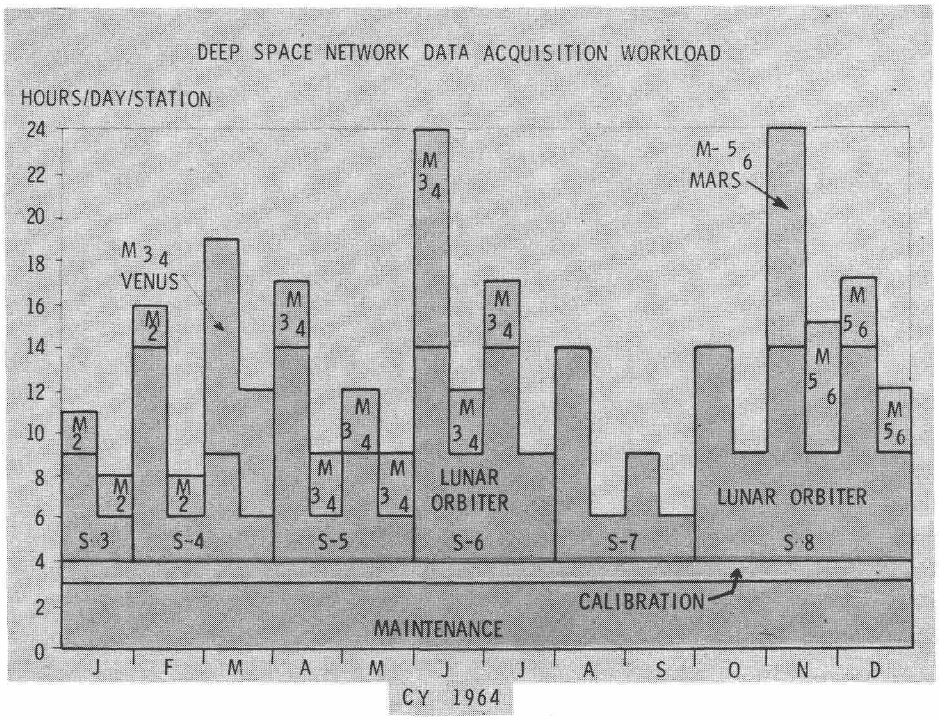


Figure 12

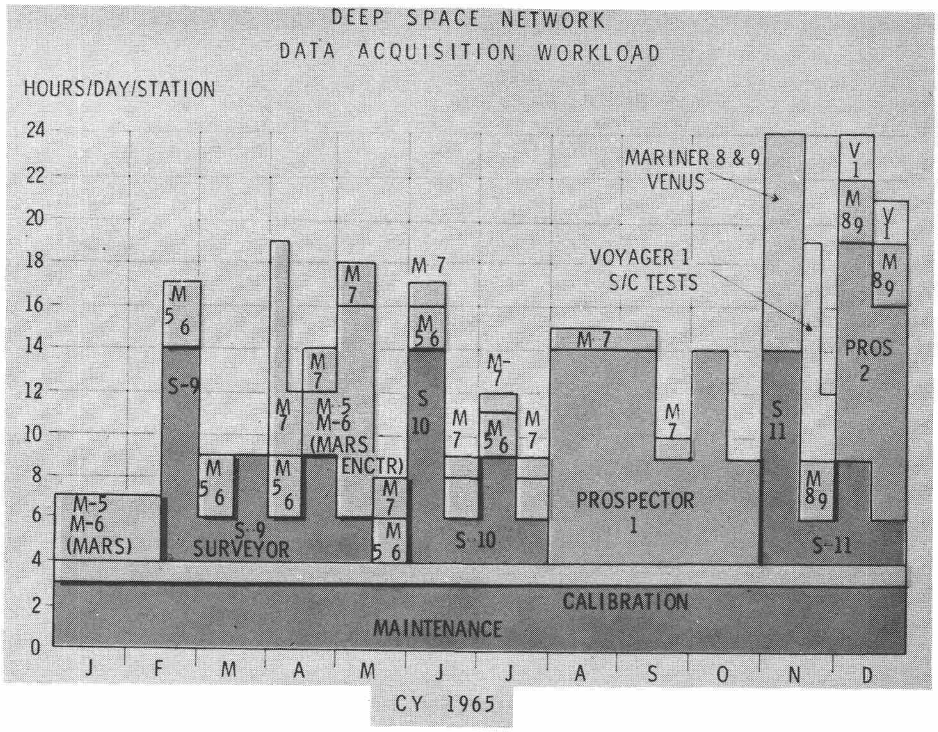


Figure 13

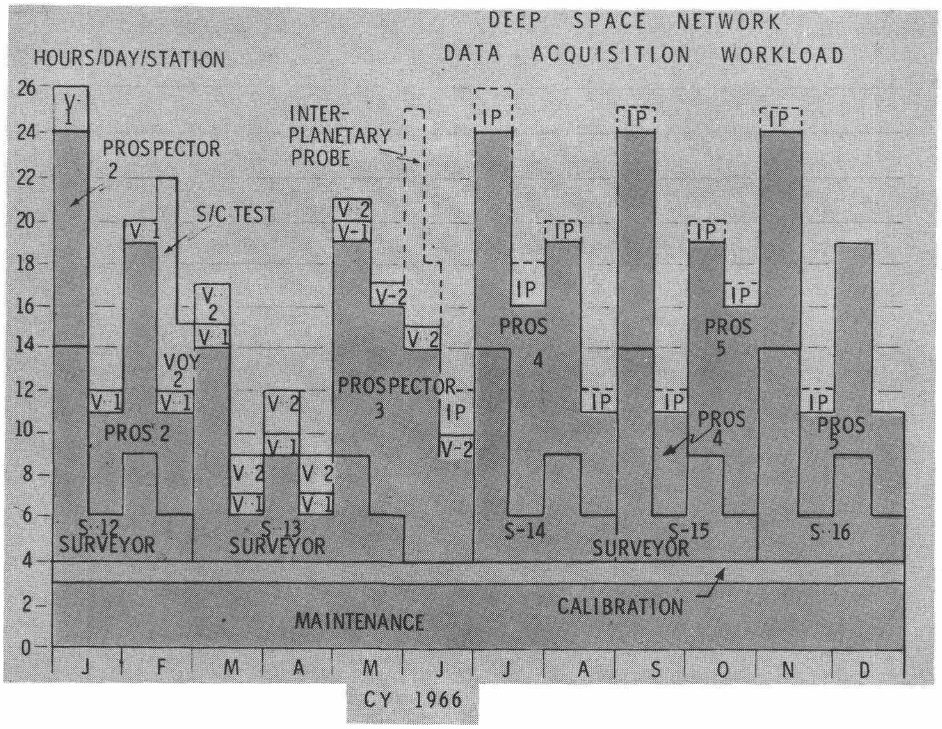


Figure 14

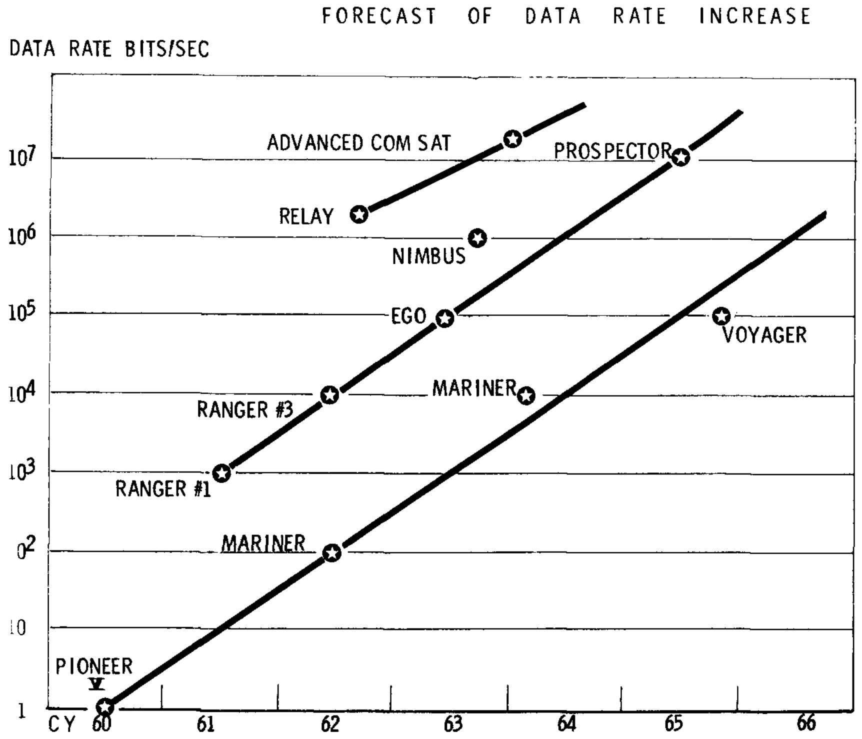


Figure 15

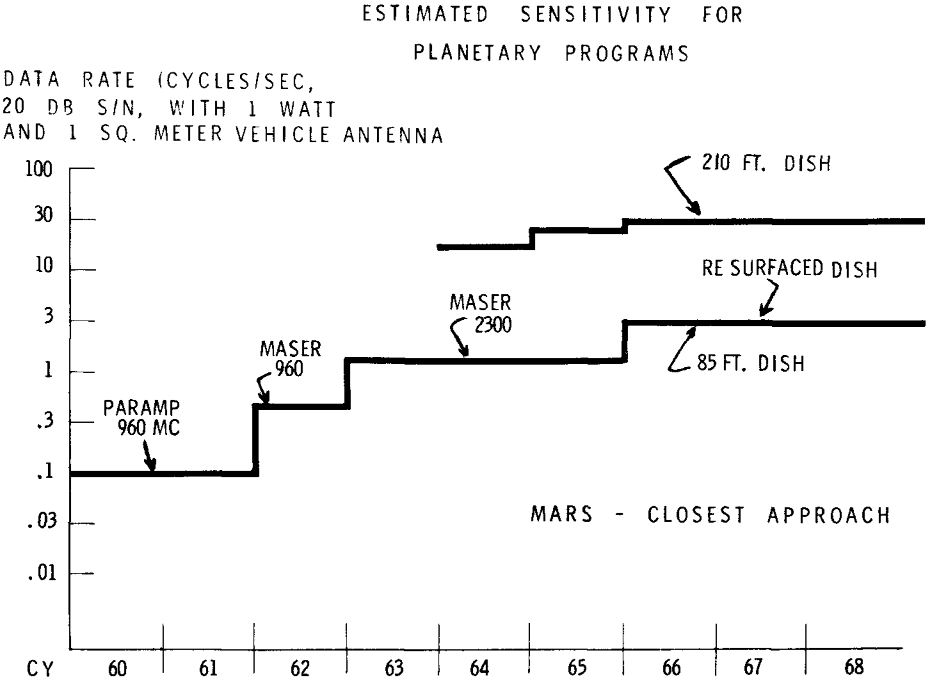
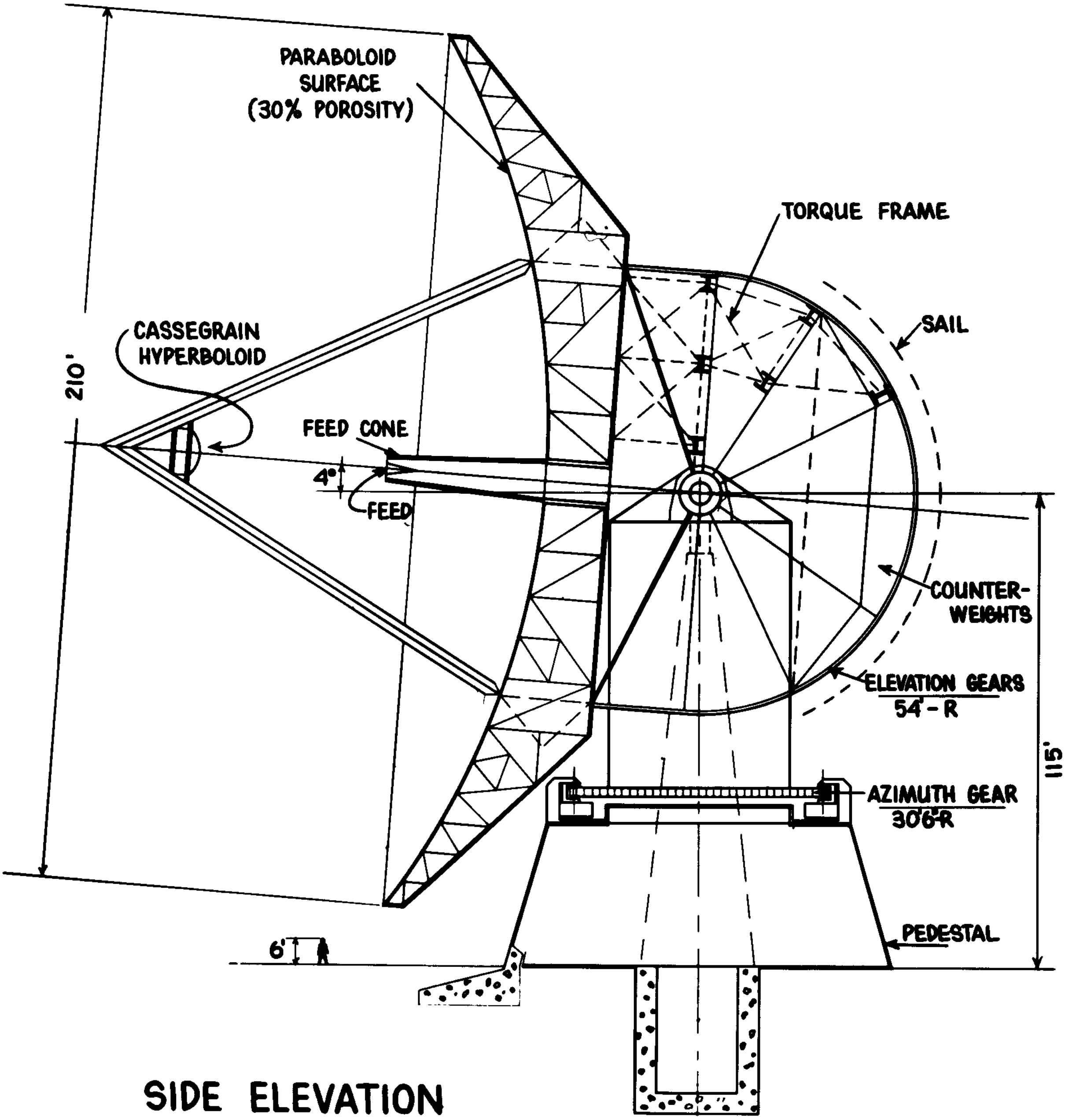


Figure 16

210 FOOT DIAMETER NASA ANTENNA



SIDE ELEVATION

Figure 17

PROJECTED DEEP SPACE TELEMETRY SYSTEM CAPACITY

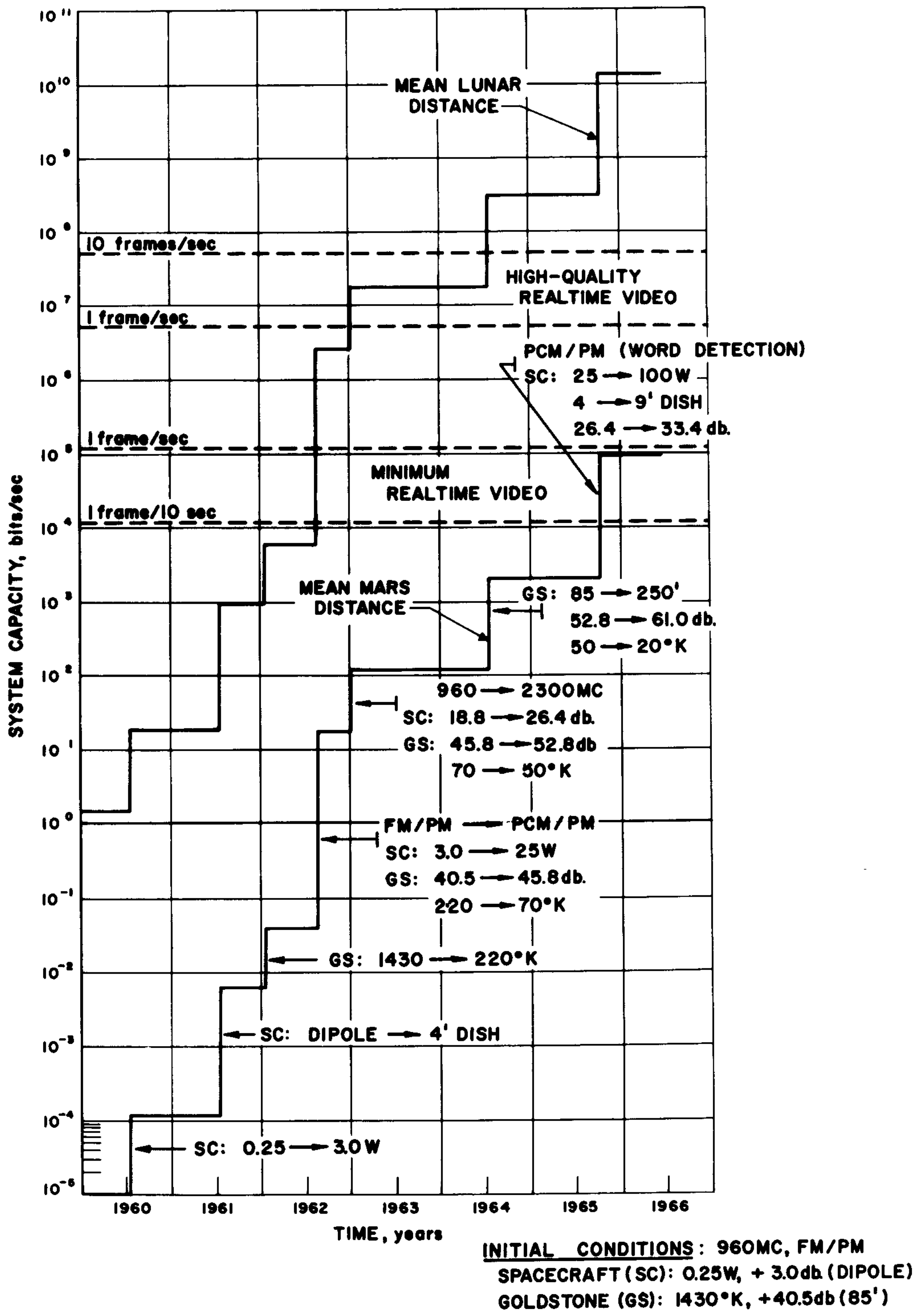


Figure 18

LIFE CYCLE OF A MAJOR PROJECT MANAGED BY GODDARD SPACE FLIGHT CENTER

By Harry J. Goett

Goddard Space Flight Center has been set up within the NASA specifically for the purpose of handling scientific satellite projects. In this capacity it is our job to adapt the requirements of the experimenter to the conditions imposed by the technological constraint of satellite operations.

In the past three years Goddard has been responsible for 18 satellites or space probes that have been successfully launched and have carried some 71 experiments into space. In addition, we have been involved in some 145 sounding-rocket launches, carrying experiments prepared both in-house and out-of-house.

Figure 1 indicates the makeup of the Goddard organization. The spectrum of capabilities required to do our task ranges all the way from scientific aspects to technological support and data acquisition. The functions in the Technological and Data columns represent the service functions that Goddard performs both for in-house and out-of-house experimenters. One capability that we have is payload fabrication which covers not only the spacecraft structure but also its power system, telemetry, encoders, thermal systems, and so forth. We have a group at Goddard that has fabricated the components for and assembled four satellites which have gone into orbit; two more will be launched in 1962.

A very important part of this work not shown in the figure is our sounding-rocket group. As previously mentioned, they have launched some 145 rockets during the past three years. In many of them our job was solely to see that the launching came off properly, to help the experimenter ready his experiment or payload and to make certain that it was going to survive the environment to which it would be exposed.

Another capability is the project-management task on larger projects, such as OAO, Ogo, and Nimbus. Although a contractor builds these spacecraft, the project-management task is a formidable one and will be the subject of the main part of this discussion.

Finally, there is the test and evaluation phase. Our experience, to date, on the 18 satellites with which Goddard has been involved, has been reassuring in that all the satellites that have achieved orbit have worked. This has been partly due to the rigorous test and evaluation regimen to which the satellite has been exposed prior to launch. We have thermal-vacuum chambers in which a complete "Delta-type" spacecraft and its experiments can be tested so that we can see whether it is going to work under the conditions to which it will be exposed in orbit. In anticipation of larger spacecraft such as the Orbiting Astronomical Observatory we have still larger thermal-vacuum chambers in the process of construction; there again, we will be able to subject the total spacecraft to the thermal and vacuum environment that it is going to have to survive in space.

Most of the publicity and most of the concern about satellites seems to involve the prelaunch preparation and the launch itself. Actually the satellite immediately after launch has just started on the useful part of its life, and a considerable amount of our effort is involved in the tracking and data acquisition and reduction (Fig. 1). We have some 30 tracking stations and data acquisition facilities in 14 different countries for this portion of the work. Goddard is connected through an extensive communications network with these stations. Then comes the task which is one of the less exotic but more important of the Space Age: taking the data tapes, digitizing them, and reducing them to a form in which the experimenter can start to analyze the data. In contrast to a ground-based experiment where the phenomena involved can be turned on and off, the data can be studied, and the experiment modified, a satellite experiment produces much more redundant data. On the Orbiting Solar Observatory, for instance, consider what would happen if during the interval when we had it turned off the biggest solar event of the past decade occurred. What we are confronted with is the necessity for acquiring essentially continuous data and then, later, processing it, and selecting the one per cent which is really meaningful. The digitized data are sent back to the experimenter, whether in-house or out-of-house, and on him falls the responsibility of selecting meaningful segments and analyzing them.

The scientific part of our work includes experiments and theory. There is a tendency, since a great deal of engineering and money are involved, that the science aspects will become submerged, and that the scientific objectives will be secondary to the engineering aspects. At Goddard it is the responsibility of scientists in the Space Science Division to see that this does not happen. These are the experimenters who not only spend a certain fraction of their time readying their own experiments for actual space launch, but also spend another important part of their time working with the outside experimenters, protecting the dominance of the scientific objective of the total mission, and representing the outside scientist to the project.

When we get results from a space experiment it is common that these results raise more questions than they answer. A typical example of this is the recent rocket observation that the ultraviolet emission from certain stars is deficient as compared with the theoretical model that had previously existed. It is easy to say that the experimenter himself should be sufficiently well rounded to look into all the implications of this observation. However, when this specific example is considered, it is realized that the experimenter must be able to go back and delve into the theoretical model of a stellar atmosphere; this is a task that deserves the attention of theoretical astrophysicists. There is, therefore, a bridge that must be built between the experimental results and the theoretician who will analyze them in depth. We have at Goddard an in-house competence in this area; we have also set up the Goddard Institute for Space Sciences. It is our hope that, through this group, theoreticians who perhaps might otherwise not become involved in the analysis of the results obtained from space experiments will focus their talents on the implications of these results. It is already evident that space science involves the interrelation between many scientific disciplines. It therefore requires special means to focus a variety of scientific talents on the analysis of the results.

The main topic of this report is project management. We set up, in our management of major projects at Goddard, a triumverate which consists of the manager himself, the project scientist, and a tracking scientist. This project scientist is the main contact between the outside experimenter and the Goddard project. He will work with the experimenter to the extent desired. When the experimenter is thoroughly familiar with the procedures he can operate to a great extent on his own and to a large degree it is his responsibility to deliver to Goddard a finished experiment which will survive the prototype tests. On the other hand, those who are unfamiliar with the procedures will receive such help as needed from the project scientist. They are available to criticize the approach and give the experimenter advice. We have tried to set this up in such a manner that, despite the fact that we are responsible for handling millions of dollars, there is a minimum of restriction on the scientific judgment of the experimenter.

Figures 2 to 5 outline the details of the major events encountered in any project, in this case, the Ogo. The upper bar in Figure 2 shows the major events involved in getting the spacecraft ready, and the lower bar indicates major events in the preparation of the experiment itself.

The concept of the major observatory type of spacecraft is to construct it in such a manner that there is a relatively standard interface between the experiment and the spacecraft; thus, it is possible to a degree to handle these two tasks in a relatively isolated fashion. NASA Headquarters and Goddard share the responsibility for the original formulation of the concept and a statement of general objectives.

The Ogo project was assigned to Goddard in February 1960. We were confronted then with the assembly of a project team, detailed feasibility studies, and the preparation of specifications, which were let out to industry in August 1960. In the ensuing five months, the contractors prepared their proposals and submitted them to Goddard where they were evaluated; there was a selection of a contractor in January 1961. Meanwhile, there were grants from NASA Headquarters for the development of various "breadboard" experiments. That is, the scientist receives a grant and says that he will work toward an agreed objective; he does not promise that he will come up with anything at any given time, just that he is applying his best effort. The incentive is the prospect of placing an experiment on a space vehicle.

Figure 3 continues this history. The Space Technology Laboratories was the recipient of this contract. The next several months were involved in the actual design study, preparation of the subsystem specifications, and initiation of long-lead-time projects. Meanwhile, interface documents and restraints were being developed, and the Goddard project scientist, with a knowledge of the experiments being prepared, was specifying to the contractor the requirements thereby imposed. Then Headquarters, as can be seen on the experiment bar, put out a request for proposals of experiments. There was a selection of so-called category 1 experiments. About 50 per cent more experiments were selected than we thought at the time could be carried onboard; our experience has been that there are always some experiments which prove unusable. The Headquarters requirement at this point was for what is called a "brass-box" experiment. This is not supposed to be an experiment that actually can survive the space environment; on the other hand, it is not an unassembled or breadboard experiment. This experiment generally resembles the weight and power characteristics of the final experiment. The various contending experimenters then make presentations to Headquarters for Steering Committee consideration. A final selection is then made of the experiments to go onboard the spacecraft. At this point the job is turned over to Goddard. By this time the experimenter really should know specifically what he can do. He should be able to produce a fairly specific work statement; he should be able to produce a cost estimate and a time estimate.

As can be seen in Figure 3, the experimenter has six months to put together his prototype experiment at which time it will go into the prototype spacecraft for test. Meanwhile, the component subassembly, fabrication, and testing is being completed on the spacecraft; then the contractor is ready to start putting together the prototype spacecraft.

Figure 4 shows the final portion of this project. There is the assembly of the prototype experiment, the integration of the prototype experiments into it, and the qualification testing of the prototype spacecraft with its experiments. Three months has been allotted for acceptance testing; the actual launching is scheduled for early 1963 at Cape Canaveral. Figure 5 is a summary of Figures 2 to 4 and shows the overall schedule milestones.

A breakdown of the cost of the Ogo is given in the following table:

Ogo Funding
(1 Prototype, 3 Flight Models)

			Millions of dollars
1. Program office (includes program management, materials testing, quality control, and reliability			2.8
2. Electrical integration and test			1.7
3. Stabilization and control subsystem			3.8
4. Electrical ground support equipment			1.8
5. Power supply subsystem			2.8
6. Communications subsystem			3.2
7. Data processing subsystem			2.5
8. Thermal control subsystem			1.0
9. Structure subsystem			2.6
10. Mechanical ground support equipment and integration			0.8
11. Backup development			4.3
Spacecraft and systems (total)			27.3
Experiments and Support	Experiments	7.2	11.0
	Test and Evaluation of experiments	0.7	
	Field Support	0.1	
	Tracking, data acquisition and ground equipment	3.0	
Launch vehicle (one Atlas-Agena B)			8.3
Data reduction and analysis (one year operation)			7.0
Total			53.6

Note that this budget includes three flightworthy models; if the estimates are accurate, each spacecraft will cost $9 million.

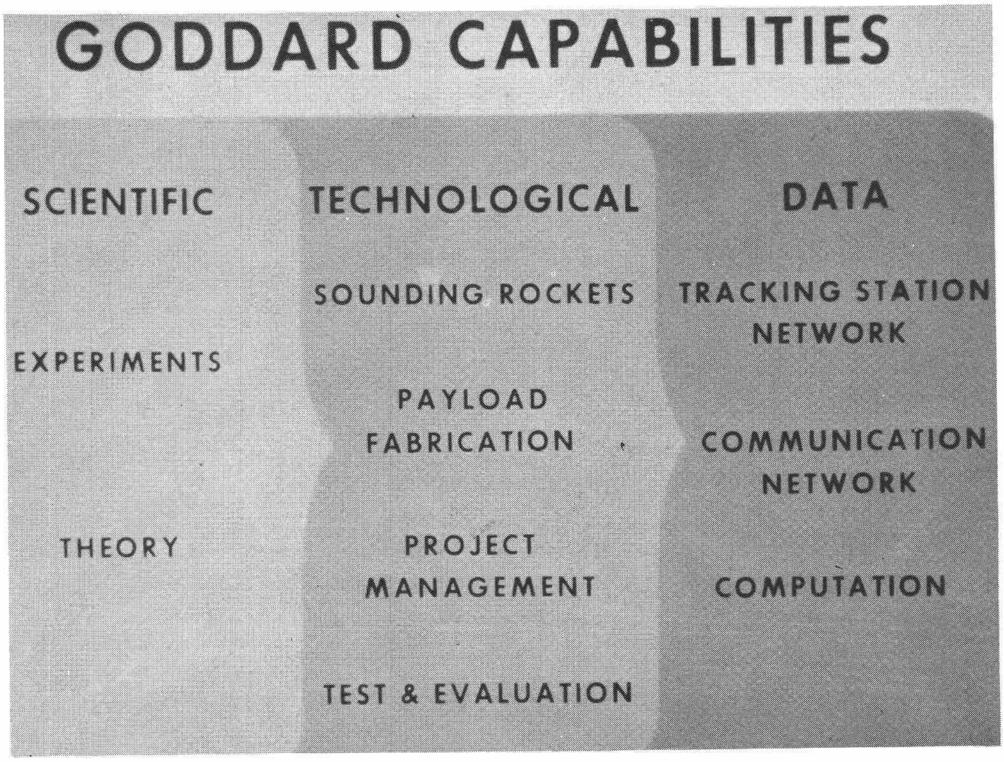
That, in brief, is the life cycle of a major project managed by Goddard. In conclusion, the following comments may be made. In the first place, we have tried to place the major burden for the experiment on the experimenter. Such help as he desires is available but, in general, we are putting the responsibility on the experimenter and trying to simplify the job by establishing fairly straightforward, uncomplicated interface requirements.

The second point is with reference to the question of small versus large spacecraft. The implication is that it is an either-or proposition. It is not. Insofar as the Earth satellites in which Goddard has been involved are concerned, our approach has been to start with the relatively simple spacecraft (which fall in the "small" category) and proceed to the more complicated. One degree of complication was added on the Orbiting Solar Observatory when we departed from simple spin stabilization; there are going to be further degrees of complication on the Orbiting Geophysical Observatory and the Orbiting Astronomical Observatory. There will be a continuing set of these relatively simple satellites; the Interplanetary Monitoring Probe will be one of these. For this probe we are exploiting the engineering experience obtained on Explorers X and XII (built at Goddard).

The third point concerns the merits of grants versus contracts. We have tried, by the procedure just outlined, to exploit the major virtues of both. We are trying, by direct work with our procurement staff, to relieve, to the extent that it is legally possible, the monthly reporting of costs and the like from the universities. We find that some universities, whose business organization is good, are capable of handling these reports easily. We find others for whom this is a major obstacle. We are trying to resolve this in the best manner that we can determine.

A major problem of the future is going to be the rate at which the Ogo, for example, will be able to produce data. It will take anywhere from two to ten IBM 7090 electronic data processing machines to keep up with this data output.

One final point concerns the release of data. It is rather commonly assumed that data are being withheld and not released. A check of the satellites programs in which Goddard has been involved shows a surprisingly quick release of data, sometimes almost too quick. There was an IGY meeting just six weeks after the Explorers X and XII were launched in which highlights of the data were presented. It is generally realized that the issuance of raw data can do more harm in many cases than it does good. I think that a detailed study of the time lag between satellite launch and the release of useful data will show that any lag is probably due to attempts by the experimenter to assure users of reliable data, data which will not have to be retracted or corrected at some future time because of incorrect calibration factor, wrong orbit, and so forth.



GODDARD CAPABILITIES

SCIENTIFIC

EXPERIMENTS

THEORY

TECHNOLOGICAL

SOUNDING ROCKETS

PAYLOAD FABRICATION

PROJECT MANAGEMENT

TEST & EVALUATION

DATA

TRACKING STATION NETWORK

COMMUNICATION NETWORK

COMPUTATION

Figure 1

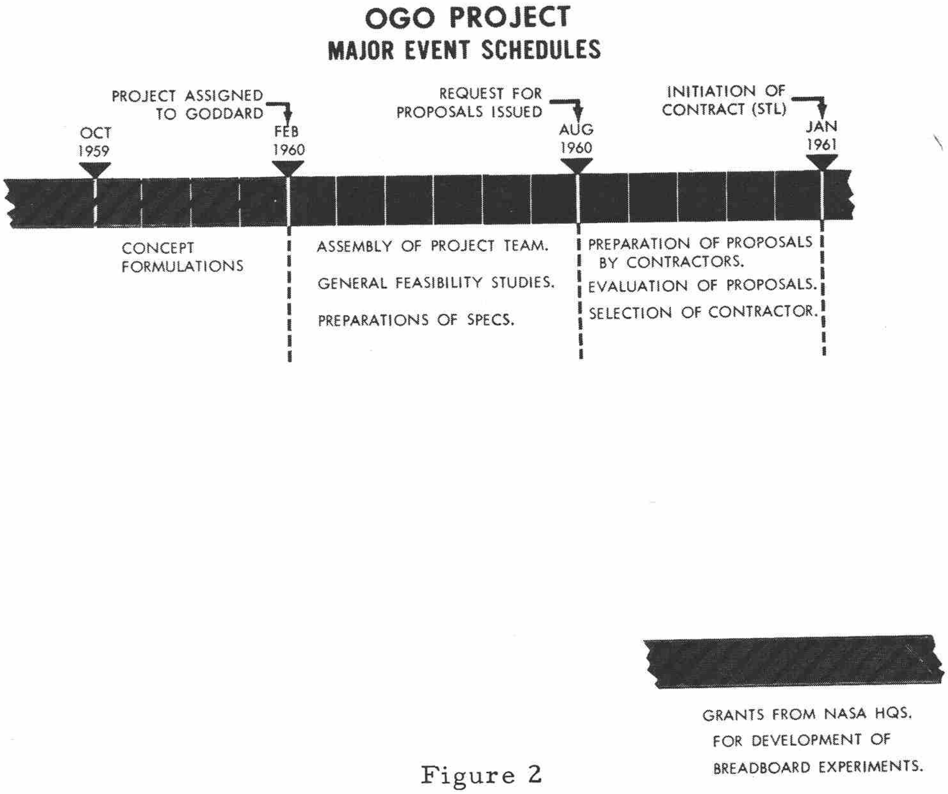


Figure 2

OGO PROJECT
MAJOR EVENT SCHEDULES
(continuation 1)

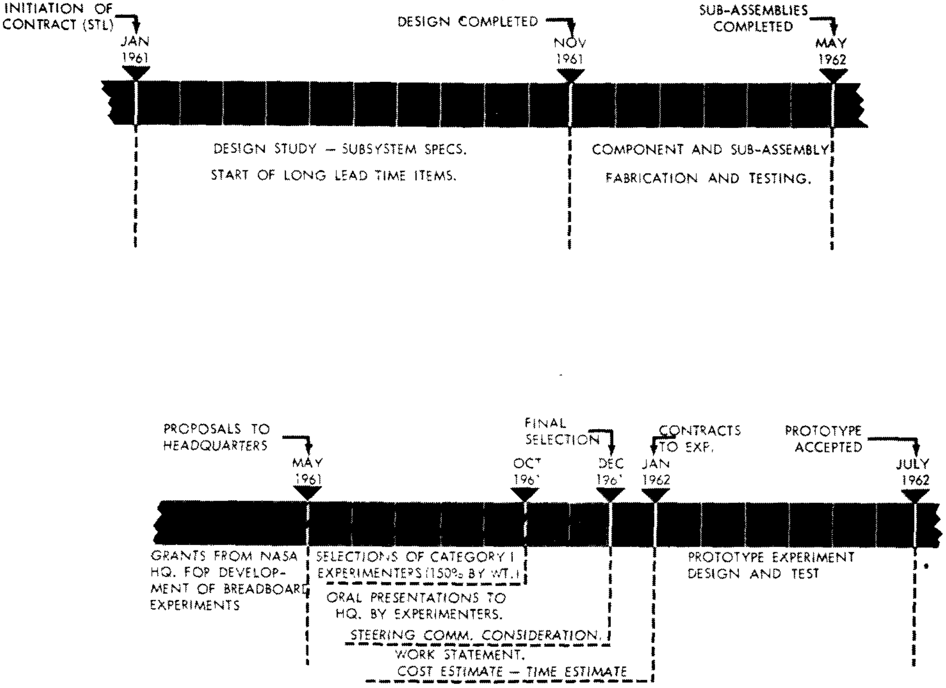


Figure 3

OGO PROJECT
MAJOR EVENT SCHEDULES
(continuation 2)

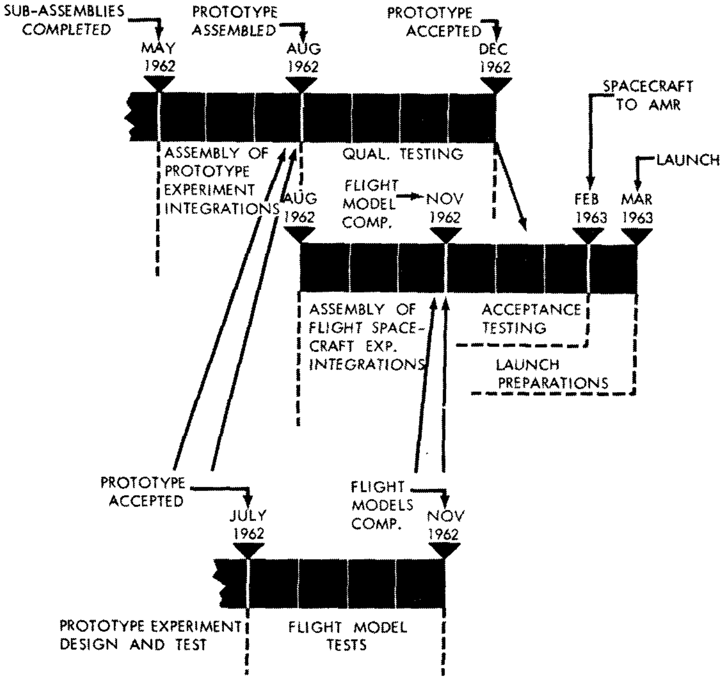


Figure 4

OGO PROJECT

MAJOR EVENT SCHEDULES

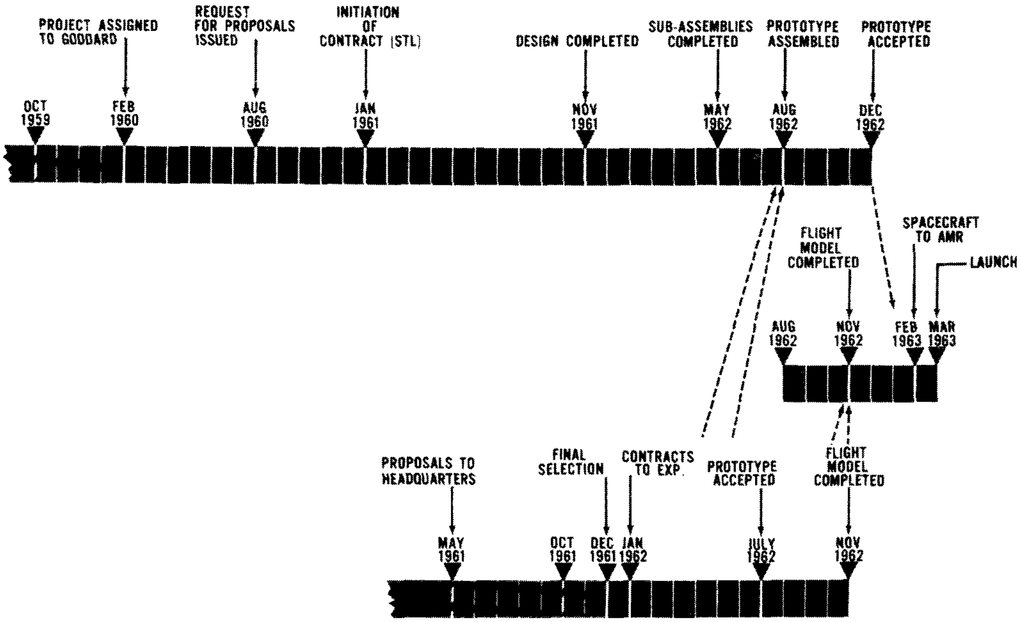


Figure 5

NATIONAL ACADEMY OF SCIENCES
NATIONAL RESEARCH COUNCIL

The National Academy of Sciences—National Research Council is a private nonprofit organization of scientists, dedicated to the furtherance of science and to its use for the general welfare.

The Academy itself was established in 1863 under a Congressional charter signed by President Lincoln. Empowered to provide for all activities appropriate to academies of science, it was also required by its charter to act as an adviser to the Federal Government in scientific matters. This provision accounts for the close ties that have always existed between the Academy and the Government, although the Academy is not a governmental agency.

The National Research Council was established by the Academy in 1916, at the request of President Wilson, to enable scientists generally to associate their efforts with those of the limited membership of the Academy in service to the nation, to society, and to science at home and abroad. Members of the National Research Council receive their appointments from the President of the Academy. They include representatives nominated by the major scientific and technical societies, representatives of the Federal Government, and a number of members-at-large. In addition, several thousand scientists and engineers take part in the activities of the Research Council through membership on its various boards and committees.

Receiving funds from both public and private sources, by contributions, grant, or contract, the Academy and its Research Council thus work to stimulate research and its applications, to survey the broad possibilities of science, to promote effective utilization of the scientific and technical resources of the country, to serve the Government, and to further the general interests of science.

The Space Science Board was established by the President of the Academy in 1958. Its primary purpose is to study scientific research opportunities and needs opened up by the advent of rockets and satellites as tools for research; to give advice and recommendations on space science to interested agencies and institutions; to stimulate research in the rocket and satellite fields; and to cooperate with scientists in these fields in other countries, particularly through the Committee on Space Research (COSPAR), established in 1958 by the International Council of Scientific Unions.