B-IV

Appendix B-IV

A P P E N D I X   T O   C H A P T E R   I V

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SPACE PLANS AND POSSIBILITIES

by

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INTRODUCTION

Ten years ago the Space Age began. Today, as we note the anniversaries of Sputnik, Explorer, and Vanguard, we do well to take stock of where we are and where we are going in Space, for after a decade of unparalleled success we find ourselves having to resell our fellow citizens this greatest adventure of mankind. Well may we ask, "What has gone wrong? Is the Vietnam War the whole reason, and will the growing disinterest reverse itself and become renewed interest and support when that conflict has been resolved? Or is there something deeper and more fundamental to combat, as is suggested by the widespread signs of

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Presented at the American Institute of Aeronautics and Astronautics 6th Aerospace Sciences Meeting Honors Night Banquet held at the Statler Hilton Hotel, New York, N. Y., January 23, 1968.

disenchantment with science in general?"

I believe that there is more to this matter than the Vietnam problem, and that those of us who pursue careers in science, technology, and advanced engineering have a lot of fence mending to do. In the years ahead of us we face the need to resell the values of our profession, in terms clear to the public, the legislator, and the administrator, in terms to make plain the worth to the public interest of what we are doing.

And we can't do that by talking only to ourselves! We have a lot to sell. It is not all Space, not even nearly all Space. But what this country has accomplished in its Space Program over the past ten years, and what it can accomplish in the future, are illustrative of our capabilities and our opportunities. So let us review the case for Space, bearing in mind the broader, rather than the narrower, context.

RETROSPECTION

Ten years ago at the start of the Space Age, our planning for the future included the requirement to establish the organizational and management as well as disciplinary and technical capability to achieve our objectives in science, technology, and applications. Now our planning for the future can be based on a well established and versatile capability in science, engineering, and administration. Reliable space vehicles are available ranging from small sounding rockets to the Titan and Saturn V class vehicles. The ability to use automated techniques successfully in space has been demonstrated by the Mariner, Ranger, Surveyor, Lunar Orbiter, Explorer, Geophysical and Solar Observatories, Tiros, Nimbus, Syncom, the Applications Technology Satellite, and many others. The capability of man to operate in space is emerging from the successes of Mercury and Gemini, and as Apollo proceeds.

The success of the first Saturn V, which was also the first launch from the new Apollo complex at Cape Kennedy, demonstrates the validity of the launch complex and most of the Apollo flight hardware, but even more it demonstrates the advance over the past decade in the capability to build and fly new large systems successfully. The combined technical and managerial skills which have been developed give us confidence that we can proceed to more exciting and complex tasks in the decade ahead.

Ten years ago, we could only assert from intuition and prophecy the value of space techniques to science. Today, we can point to a profound influence that space has had on the geosciences, is having on astronomy, and is beginning to have in life sciences.

Ten years ago, one could only point forward hopefully to space applications in meteorology, communications, navigation, and geodesy. Today, there are satellite systems and their earth-based components in operation for all of those areas, while still other possibilities are becoming apparent, for example, in earth resource surveys or data collection and dissemination.

Ten years ago there was serious question as to whether there would be adequate manpower or interest to carry out our planned space program. Today, there are substantial teams of highly competent scientists and engineers engaged in developing and applying our national space capability.

Ten years ago our space program was put together in something of a piecemeal fashion, bringing to bear separate capabilities as we could seize upon them, taking largely a survey approach in science, and an exploratory approach in applications. Today, we are in a position to plan and carry out a broad, integrated program to explore the solar system, including investigations of the earth and of sun-earth relationships. We are well on our way toward establishing the basis for investigating major areas of natural phenomena that characterize the universe in the places where measurements can best be made in space, with a power and an intensity hitherto unavailable to the researcher. If a breakthrough to long-range weather forecasting ever proves possible, it is clear that space will play a major role. A broad spectrum of communications applications for commercial, industrial, political, and military purposes is apparent and within reach.

Ten years ago it was apparent to many that space and aeronautical technologies would prove mutually contributory. Today it is patent that the two technologies are in many areas inseparable.

Ten years ago scientists and engineers schooled in the traditions and skills of the pre-Space Age era, had to turn to, learning and relearning as they went, to tackle the new problems ushered in by the Space Age. Today, we see entering the scene a new generation of researchers who, during their formative years, watched the Space Age unfold. These men and women have been caught up in the excitement that space represents. They may be expected to bring their enthusiasm to bear not only upon space, but also upon the great challenges today at home on our planet earth, such as the problems of oceanography, earth resources, the cities, transportation, population, pollution, and food. They are likely to oppose any national decision to sidestep <u>any</u> of these challenges.

Ten years ago the total of the world's space effort was encompassed in the beginning efforts of the United States and the USSR. Today space research and applications have become worldwide, with some eighty nations in addition to the US and USSR, taking part, many of them with sizable and substantial efforts of their own.

Ten years ago the atmosphere, the oceans, and space were more often thought of separately rather than as closely related media. Today these three are beginning to be viewed as inseparable media of human activity, engendering a broader and more cohesive view of our natural environment, and forcing a reassessment of the dimensions of our political environment.

Ten years ago there was grave concern about this country's ability to compete in space, and general agreement that we must not only compete but must lead. Today, there is no longer any grave concern about our ability to compete, but neither is there general agreement that we should continue a major program of space exploration. Rather, there is a feeling that the space program can be cut back, this in the face of the fact that we have just begun to explore and use space and in the face of increasing USSR efforts.

Ten years ago the space program was almost doubling in size each year, today the program has actually been shrinking since its peak in 1966.

Ten years ago any space mission was new and capable of bringing in exciting information. Then the required scientific experiments were simple, while today many of the pioneering experiments have been done. Now we are faced with the more difficult tasks of investigation, including the development of an understanding of the processes and dynamics of phenomena we have uncovered. Often the scientific experiments required to answer today's questions are large, complex, and costly.

Ten years ago a major problem was to find enough capable people to undertake a mission. Today, with a shrinking program there is an abundance of good people. NASA completed the Lunar Orbiter and Mariner V programs in 1967 and is right now winding up the Surveyor Program. More than $700 million and 20 to 30 thousand man years of effort were spent on the Ranger, Mariner, Orbiter and Surveyor projects through 1967. The elimination of the Mariner 71 and Voyager program from the Fiscal Year 1968 budget creates problem of what to do with these teams of highly skilled people. Some, but certainly not all, of the key people can be used in Apollo as it approaches the crucial period of manned operations and the landing on the moon.

Ten years ago mastering space technology was a major problem. Success was a rare and precious commodity. Today, success is routine and failures are rare.

Ten years ago we were caught up in a wave of enthusiasm for Space, and a frantic drive to compete with the Soviets who had stolen a long and psyche-shaking march with the launching of Sputnik I. Today, the values of space research, and indeed of all research, must be weighed against their contribution to national needs--the need to reduce expenditures, the need to solve the problems of the cities, of the pollution of our environment, and of over-population.

THE OUTLOOK

I think you will agree that our country can take satisfaction in, and rightfully be proud of, what it has accomplished so far in space. But truly, what has been done is only the merest beginning of what can be done.

The opportunities this nation, and indeed the world, have today in space are challenging and promising. In space applications one can look forward to the use of space communications for radio and television broadcasting, navigation, civilian ship and air navigation, air traffic safety and control, data collection and dissemination, and space communications, that is, the use of space systems for relaying radio communications from remote parts of the solar system to the earth. Future operational weather satellite systems could be expected to greatly improve on the capabilities of the present-day systems, and possibly to help furnish the basis for long-range weather forecasting, and to play a role in the development of techniques for weather modification and control. The use of satellite observations for earth resource surveys is highly promising. In science, one could expect to move from survey and exploratory measurements to investigation of the dynamics and underlying causes of phenomena in space; and from a broad collection of separate, loosely connected experiments to an integrated investigation of the solar system and of the universe. The creation of a large, versatile astronomical facility in space with the promise of a revolutionary impact on the science of astronomy is within our grasp. The biomedical results from Mercury and Gemini, and the startling results from the first successful Biosatellite hold promise of the usefulness of space techniques in biological and biomedical research.

But it is not enough to consider only opportunities. We must also assess the climate in which these opportunities exist, and note with concern a growing inclination to let those opportunities pass by unheeded and unused.

The outlook for the next two to three years, or at least until the situation in Vietnam has been resolved, must be for reduced budgets and for few new space missions to follow up and build on our completed projects. We plan to terminate the Orbiting Astronomical Observatory (OAO) in 1970 after OAO-C; the Orbiting Geophysical Observatory (OGO) project in 1969 after OGO-F; the Biosatellite in 1970 after Biosatellite E. We plan to continue the Mariner missions to the nearby planets but as of now there are no approved missions after the Mariner 69 missions to Mars. We plan to continue the Orbiting Solar Observatory (OSO) but there are no approved missions after OSO-H.

We have plans for continuing manned space flight programs after Apollo, as in Apollo Applications, but the support we receive is halting and questioning.

Obviously, as projects are phased out, with few if any new ones to take their place, we will lose some of our hard-won capability. Present plans call for reducing the size of the NASA work force by 1,704 people, from 34,126 to 32,422. The number of people in industry working on NASA contracts will be reduced from a peak of 420,000 in 1966 to 270,000 by 1 July 1968.

With such an outlook for the immediate future, it is important to assess accurately the prospects for the more distant future, for the mid to late 70's, for example. Our expectations are important because they help to determine the decisions we should make in these next crucial years. If we anticipate a permanent large-scale reduction in the resources allocated to space, that would imply one set of actions for NASA for the next few years; if, however, the outlook is for a temporary slowdown in the space program, brought on by the present need to reduce expenditures, to be followed, after that need is past, by a resumption of a vigorous exploration of space, that would imply quite a different set of actions by NASA. In the first case the primary concern would be the orderly completion of the projects underway and obtaining the maximum benefit from them. In the second case, actions would be required to minimize the loss of capability, particularly key people and in some cases essential teams of people. In addition, some of our scarce resources would be used to do the detailed planning and the design and laboratory work leading to new missions in the 70's. These steps are essential to maintain the elements of our spaceflight capability to enable the country to start new projects with a minimum amount of re-learning and retraining.

LONG-TERM OUTLOOK

We assume that the long term outlook for space exploration and research and for the application of space flight and techniques to the solutions of practical problems is good. Consequently, NASA is making the decisions and taking the actions required to preserve the nation's space capability and to lay the groundwork for a vigorous and exciting program in the 70's.

Why do we think the long-term outlook for the space program is good?

First and foremost, the intrinsic worth of the program merits the interest and support of the American people. The scientific results of the past decade demonstrate the power of the satellite and space probe as research tools. The economic value of the communications and meteorological satellites demonstrates the worth of space techniques when applied to practical problems.

Secondly, the vast numbers of babies born just after World War II are now college students and will in the 70's become a powerful force of intelligent, highly educated people looking for careers which are satisfying and which in their judgment will be historically significant. This generation, having grown up with the space program will, we think, want to continue to explore the moon and planets, to observe the stars and galaxies above the atmospere; and they will see the satellite--with its worldwide communications ability and its ability to gather data on a global basis--as a mechanism to help draw the peoples of the world together and to help solve their common problems.

Thirdly, we expect the USSR to continue a vigorous, expanding space program which cannot help but be a constant challenge that this country must meet.

While it is clear that expenditures must be guarded wisely to enable the nation to meet the challenge it faces in Vietnam, it is equally clear that Vietnam is not the only place in which this nation's way of life and this nation's ability to organize and manage large complex technological problems is being challenged. According to the report, "Review of the Soviet Space Program," prepared by the Science Policy Research Division, Legislative Reference Service, Library of Congress, Washington, D.C., the USSR has made 19 planetary attempts since 1960 while the U.S. has made 5. Clearly, the Soviets have assigned a high national priority to planetary exploration. It is also clear from their total launch rate that they are continuing to develop the technology of automated and manned flight systems. There is no evidence that they have retreated from their plan to demonstrate the supremacy of the Communist way of life by showing that they are the leaders of the world in the ability to conceive, design, develop and use space hardware. The long-term need on the part of this country to meet that challenge of the USSR and the emerging challenge of China is real and far-reaching in consequences, just as is the need to meet the immediate challenge in Vietnam.

Fourth, as this nation's economic growth continues, and hopefully, as our expenditures for military efforts decrease, we think there will be not only the resources available for a vigorous U.S. space program, but also a recognized need to maintain the industrial capacity represented by the space and space-related industries.

Finally, we are assuming that the public will grow is its understanding and appreciaton of the intrinsic worth of the space program. The space-oriented younger generation will help to bridge the present communications gap between our profession and the layman, but in the meantime we must continue our own efforts to communicate by using all the means at our disposal.

FUTURE POLICIES

There are several key policies that NASA plans to follow in the immediate future. First, as indicated above, NASA is planning for a major space program in the 70's. As we terminate or complete projects, we are reviewing which elements of those projects should be preserved, and how best to go about it. We are maintaining, and in some cases increasing, the level of funds for supporting research and technology. There are several reasons for doing this. There are large amounts of data coming in from the spacecraft which we have launched. Supporting research and technology funds can be used to analyze and interpret those data. Supporting research and technology funds can be used for laboratory work and theoretical studies required to design new experiments and instruments for the missions we think are likely in the 70's.

Secondly, we plan to seek approval and support from both the Executive Department and Congress, to initiate some new programs. While it is clear that many leaders in Congress feel that NASA should proceed with at least a reduced planetary program at this time, it is equally clear that there are others who oppose any further planetary flight program at this time.

In the planetary program the funding and the work of the agency will be structured so that we can proceed either along the line of a modest flight program, or with a much slower paced supporting research and technology program. Which path NASA will take will depend upon decisions of the President and the response of the Congress to the FY69 budget. The work is structured in such a way that the agency will not have to choose between these two alternatives until late in FY68 when Congress will have had an opportunity to review our proposals and indicate their reaction.

Another policy which we plan to follow is a greater involvement of the academic community in planning our future program as well as executing it. The universities have conducted the majority of the scientific research for our space program in the past; we plan to continue that policy. However, because of the complex nature of the spacecraft we will be designing and building, we need to involve the academic community more than ever in both the design and the execution of a mission.

We plan to increase emphasis on aeronautics as our growing space technology has an ever-increasing impact on the use of airspace as well as outer space.

NASA will continue to support the development of key advanced technologies, such as materials and structures, space power, electronics, and nuclear propulsion. We will continue with the Nerva I program as outlined to the Congress in last year's hearings.

At the same time, we plan to give more emphasis to the application and use of the space capability we have already developed, relative to the development of new capability.

There will be increased emphasis on the use of knowledge gained from space research and technology to the solution of practical problems on earth. We will give especial attention in our planning for the next decade to the interests of various user agencies such as the Departments of Interior, Commerce, Agriculture, Defense, and Transportation, for example.

As we continue the development of manned space flight, and move to the completion of the Apollo Program, we will be planning and laying the groundwork for such applications of the manned capability as the creation and use of orbiting astronomy facilities, space laboratories, and deep space missions. In the more immediate future, as a steppingstone to all of these possibilities, we will be giving special attention to where we go next in the development of orbiting space stations.

CONCLUSION

The continuing progress of our nation in its attacks on poverty, ignorance, and disease, cannot be achieved if we neglect the basic strength of our national economy--an advancing frontier of technology led by vigorous and innovative pioneers. Under the democratic system, there is no substitute for human judgment, participated in by large numbers of our citizens.

Until we of the scientific community have done our utmost to inform the citizen and increase his understanding, he cannot be a truly and fully effective participant in today's society. More than ever before, this nation needs men and women whose minds can draw the analogy between solving technological problems on the one hand, and solving sociological problems on the other.

Our resources, whether natural, monetary, or human, must be expended wisely and carefully in these difficult times. The values of our various programs, including the space program, must be considered carefully to determine how well they serve to answer our national needs. But in making the decisions, we must continually bear in mind that the same kind of innovative thinking now so clearly evident in the space program can be applied and carried over to earthbound problems of air and water pollution, urban housing and transportation, crime control, hunger, and all the other factors that weigh heavily on men's minds today.

I mentioned earlier in this discussion the young people now coming up through the school system, to take over from us--from you and me. They are growing up in the Space Age. They are imbued with an enthusiasm for what can be done today on many fronts. If you and I don't do some of these things sooner, they will take them up and do them later. History will record that they did them, and by implication, that we didn't.

In any case, sooner or later, just as we have come to think naturally of and accept fully the telephone, radio, television, the automobile and the airplane, the materials and products of modern chemistry, the miracles of modern medicine, and the whole range of scientific and technological benefits as part of our culture and everyday living, I firmly believe that one day space will be an accepted and <u>unquestioned</u> part of our everyday concepts and uses.

SPACE RESEARCH

DIRECTIONS FOR THE FUTURE

REPORT OF A STUDY
by the
SPACE SCIENCE BOARD

WOODS HOLE, MASSACHUSETTS
1965

PART ONE

SPACE SCIENCE BOARD
NATIONAL ACADEMY OF SCIENCES – NATIONAL RESEARCH COUNCIL
WASHINGTON, D.C. DECEMBER 1965

FOREWORD

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

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

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

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

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

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

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

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

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

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

Harry H. Hess, Chairman
Space Science Board

I

Planetary and Lunar Exploration

INTRODUCTION

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

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

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

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

Table 1

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

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

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

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

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

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

1. MAJOR RECOMMENDATIONS

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

STRATEGY OF LUNAR AND PLANETARY EXPLORATION

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

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

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

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

MANPOWER AND FACILITIES FOR LUNAR AND PLANETARY EXPLORATION

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

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

ADVISORY STRUCTURE OF NASA

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

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

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

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

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

SCIENTIFIC PRIORITIES

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

EARLY USE OF SATURN V ON MARS MISSIONS

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

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

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

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

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

EARLY VENUS PROBE

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

DEEP SPACE INFORMATION TRANSFER

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

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

ROCKET POINTING CONTROL

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

ORBITING PLANETARY OBSERVATORY

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

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

OPTICAL TELESCOPES AND THE SPACE PROGRAM

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

SCIENTIFIC PASSENGER FOR LUNAR EXPLORATION

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

THEORETICAL EFFORTS IN PLANETARY SCIENCE

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

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

SPACE RESEARCH

DIRECTIONS FOR THE FUTURE

REPORT OF A STUDY

by the

SPACE SCIENCE BOARD

WOODS HOLE, MASSACHUSETTS

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PART TWO

SPACE SCIENCE BOARD

NATIONAL ACADEMY OF SCIENCES – NATIONAL RESEARCH COUNCIL

WASHINGTON, D.C. JANUARY 1966

II

Optical Astronomy

1. SUMMARY AND RECOMMENDATIONS

The Working Group on Optical Astronomy was organized "to examine the future needs of optical astronomy for large-aperture orbiting telescopes of a generation beyond the orbiting astronomical instruments which are now being readied for launching." The Group interpreted this charge to include the space program for optical astronomy generally, since consideration of large instruments requires study of the scientific data as well as engineering experience gained with small instruments. As applied to the Working Group's area of concern, optical astronomy in space was defined to include all astronomical research carried out with reflecting telescopes in space at wavelengths from 800 Å to 1 mm, excluding solar studies. In terms of the instruments used, this definition is logical, since a conventional optical telescope with near-normal-incidence reflecting optics can be used for a wide variety of observational studies in this wavelength range. At the lower wavelength limit, somewhat shorter than 912 Å, mirror reflectivities tend to be low, and stellar radiation is probably completely cut off by the interstellar hydrogen absorption. Above the upper limit of 1 mm, the atmosphere becomes transparent and larger radio telescopes on the Earth's surface are more effective. (Solar research, with different problems of thermal control and guidance requirements, needs different types of telescopes from those used for observing stars, stellar systems, nebulae, and planets, and was therefore the subject of study by a different Working Group.)

The space astronomy discussions at Woods Hole in 1965 were in some ways a continuation of earlier discussions by the Astronomy Working Group at the Iowa Summer Study Group in 1962 (" A Review of Space Research", Publication No. 1079 of the National Academy of Sciences—National Research Council). During the three-year interval since that earlier study, great strides in space technology have been made. Large rocket boosters have placed tons of equipment in orbit, and the Gemini flights in the spring of

1965 have shown that man can operate effectively in space, even outside the spacecraft. The progress of optical space astronomy in the study of objects other than the Sun has been impeded by the difficult pointing requirements, but the accumulating data on ultraviolet stellar spectra obtained with sounding rockets (including a recent spectrogram with 1 A resolution), and the progress made in fabricating and testing Orbiting Astronomical Observatories, suggest that rapid progress in this field can now be expected. The Woods Hole discussions naturally reflect the confidence resulting from these developments.

The present report is designed primarily to present the recommendations made by the Working Group, together with enough background material to explain the chief reasons underlying each specific recommendation. Many of the auxiliary points discussed by the Group are not mentioned here. To provide general background information, Section 2 presents a brief discussion of some of the most important and striking research objectives of astronomy in general and of optical astronomy in space in particular. Section 3 discusses the short-range program in optical space astronomy, including flights planned during the next ten years, and related programs in astronomical instrumentation, optical design, and ground-based research generally. Section 4 is devoted to the longer-range goal of a large space telescope. Section 5 comprises three appendixes—the working papers of the Group.

RECOMMENDATIONS

The Working Group on Optical Astronomy has considered the possibilities for studying stars, star systems, nebulae, and planets by means of telescopes in space sensitive to electromagnetic radiation at wavelengths between 800 Å and 1 mm. For the short-range program (1965-1975), the following recommendations (all summarized here) have been made:

(1) The number of coarse-pointing sounding rockets available each year for optical space astronomy should be increased to twice the present level.

(2) Two or more telescopes having apertures of 40 inches or larger should be included in the Apollo Extension Systems (AES) program. The Orbiting Astronomical Observatory (OAO) program should be continued until AES launchings are definitely scheduled.

(3) Development of various detectors required in space telescopes should be supported by NASA.

(4) Development of improved gratings would be of central importance in the space astronomy program.

(5) Development of optical interferometers should be pressed, with probable initial operation on the ground.

(6) Research and development concerned with problems of space-

telescope optics, especially with the primary mirror, should be supported by NASA.

(7) Support of ground-based astronomy should be increased, as such support is urgently needed for the continuing healthy growth of astronomy in general and of space astronomy in particular.

With regard to the long-range program (after 1975), the Working Group has concluded that the focus of the national effort in optical space astronomy generally should be toward, and in the context of, a very large orbital telescope to be used with a wide variety of astronomical instrumentation. To help pursue this objective the following recommendation (given in full here) was adopted:

(8) We conclude that a space telescope of very large diameter, with a resolution corresponding to an aperture of at least 120 inches, detecting radiation between 800 Å and 1 mm, and requiring the capability of man in space, is becoming technically feasible and will be uniquely important to the solution of the central astronomical problems of our era. We recommend that the Space Science Board of the National Academy of Sciences appoint an *ad hoc* panel to work toward this Large Orbital Telescope and to encourage studies of those critical areas where particular research and development is required in the near future to further this program.

4. THE LARGE ORBITAL TELESCOPE

It seems clear that during the next ten to twenty years the U.S. space program will have an increasing potential for launching complex scientific equipment into space, and maintaining it there. The Working Group considered in some detail what the program in optical astronomy should be during this long-range interval. The possibility of launching a single very large, diffraction-limited telescope into space has been discussed previously, for example, at the Iowa Summer Study in 1962 and in various NASA planning documents. Consideration of such a space telescope as a goal for the long-range program raises the following two major questions:

(1) Would a large telescope of conventional optical design be a vitally important research tool several decades hence?

(2) Would a single large telescope be preferable to many smaller ones?

These questions were discussed in detail and at length by the Working Group.

The first question is of particular importance because of the long lead time, between one and two decades, for launching a large space telescope. However, it seems reasonable to assume that so long as astronomy is of interest to man, the measurement of electromagnetic radiation from the stars will continue to be an important objective of astronomy, and the use of a reflecting telescope seems to be much the simplest method of achieving this objective over a wide range of wavelengths. It is conceptually possible that methods might be developed for circumventing the varying atmospheric refraction of starlight (bad seeing), and also the distortion of large mirrors under gravity (flexure), and thus achieving high-resolution pictures from the Earth's surface. There is no simple physical reason why such a technological development should be impossible, though clearly it would be difficult. However, until such technology has been shown to be possible, either by analysis or by experiment, this possibility must be regarded as remote. In any case, there seems to be no possibility whatever that radiation below 2900 Å, and in certain wavelength bands in the infrared, can ever be observed from the Earth's surface. On the basis of these considerations, one can conclude that a large diffraction-limited telescope in space would certainly be of very vital importance in astronomical research for many years to come, though for high-resolution pictures, such a telescope might conceivably in time have some competition from ground-based instruments, but only in the event that a major technological advance is made in ground-based instrumentation.

With regard to the question of whether a single large telescope would be preferable to many smaller ones, there are strong reasons, peculiar to space astronomy, for favoring the single large instrument. On the assumption that diffraction-limited images can be obtained, a single large instrument clearly has a capability that cannot be matched by several smaller instruments with the same surface area; while the use of interferometry and aperture synthesis could in theory provide equivalent resolution, use of several small telescopes in this way could not yield the same signal-to-noise ratio as a single large telescope, and could not, therefore, reach the same limiting magnitude. The great advantage of manned supervision and maintenance of this complex equipment to ensure its reliable operation is a further argument in favor of a single large instrument. Moreover, in view of the enormous cost and effort required to maintain man in space, it would be difficult to justify manned supervision of many small instruments, even if these were all located near a single space station; a single large instrument would presumably require fewer man hours of maintenance per year than many small instruments, each with its own pointing system, power supplies, and communication systems. A single large telescope, maintained at regular intervals, could have a useful scientific life of decades, as compared with about a year or less for complex unmanned equipment at present.

The great flexibility of an optical space telescope, which could be used for most types of astronomical research at wavelengths from about 10^{-4} to 1 mm, is an argument for concentrating the long-range program on a single instrument, with the largest possible aperture. Among other types of instruments that might be needed for optical space astronomy, the Working Group considered the three listed below.

Survey telescope. An all-reflecting Schmidt telescope might be used to find objects that radiate only in the infrared or ultraviolet and therefore cannot be found on plates taken in the visible or photographic regions of the spectrum. It is not clear whether such objects exist, and at present there is no wide-field imaging sensor available for wavelengths longer than 1.2 microns. The ultraviolet surveys planned for the OAO program (Smithsonian experiment) and for the Apollo program (University of Arizona) will indicate whether a larger ultraviolet survey telescope would be useful. For a diffraction-limited survey, a Schmidt telescope is not suitable because the focal ratio must be large if the size of the image on the sensor is to exceed the wavelength of light.

Infrared telescope. A telescope designed to be diffraction-limited at a wavelength somewhere between 10 and 100 microns might conceivably be made very much larger than an instrument designed for ideal optical performance at 0.5 micron. Until this field of research has been explored more fully from the ground and from space, the value of such a specialized instrument cannot be assessed.

Interferometer. The beam interferometer, designed to achieve very high resolution on particular objects, would be a useful instrument in optical astronomy. Current efforts to use this technique from the ground have been discussed above, and further information is required before the need for interferometric equipment in space can be evaluated.

After study of these various points the Working Group concluded that at present the long-range program in optical astronomy should be concentrated on a single general-purpose telescope, though special-purpose instruments might be included at a later date, when and if a clear demonstration of their value can be made.

Following considerable discussion the Working Group adopted the following recommendation:

Recommendation 8

We conclude that a space telescope of very large diameter, with a resolution corresponding to an aperture of at least 120 inches, detecting radiation between 800 Å and 1 mm, and requiring the capability of man in space, is becoming technically feasible and will be uniquely important to the solution of the central astronomical problems of our era. We recommend that the Space Science Board of the National Academy of Sciences appoint an ad hoc panel to work toward this Large Orbital Telescope and to encourage studies of those critical areas where particular research and development is required in the near future to further this program. (See p. 2-21 for considerations leading to the last part of this Recommendation.)

Confidence in the technical feasibility of a diffraction-limited 120-inch space telescope was based on the various technical studies carried out for NASA directly or indirectly by various groups (Boeing, American Optical, Perkin-Elmer); the engineering problems of such a large instrument were discussed only briefly by the Group. The design goal of a 120-inch aperture was adopted in the belief that a long-range instrument should be a very significant advance over the insturements used in the Stratoscope and OAO programs, whose apertures are in the 30 to 40 inch category. The aperture could well be greater than 120 inches, if that proves technologically feasible (see page 17).

It was the conviction of the Group that this large instrument could provide a dramatic central focus for the optical space astronomy program, and that it would be an appropriate major space program for the nation. It was to help emphasize the central character of this instrument in the national space effort that the name "Large Orbital Telescope" (LOT) was proposed. While the term "orbital" was used for this large-span telescope, the possibility of a lunar location was not strongly excluded.

Clearly, adoption of the LOT program would have a significant impact on the short-range program in optical space astronomy. While the short-range program discussed in Section 3 is designed primarily to obtain significant scientific results, the data obtained and experience gained would be absolutely essential for the LOT effort. In particular, the AES effort could be an important forerunner of the manned high-resolution LOT. In general, considerable expansion of much of the short-range program might be required if the LOT were to be effectively used within the time scale outlined below.

The subsequent sections discuss the possible design parameters for the Large Orbital Telescope, a time schedule that may be visualized for its construction, and some administrative problems that might be associated with this enterprise.

DESIGN PARAMETERS

The general characteristics of a large space telescope, discussed in earlier sections of this report, apply to the LOT as well. Thus, this large telescope would be a general-purpose instrument, focusing electromagnetic radiation in the wavelength range from about 800 Å to 1 mm. The Group discussed briefly the engineering problems of this telescope and the design parameters that might be chosen in view both of these problems and of the scientific objectives. While no recommendations were adopted on most of these items, the conclusions are summarized here for reference.

Aperture

For reasons already outlined, the goal of designing a diffraction-limited 120-inch telescope was adopted by the Group. The actual diameter of the instrument would depend, of course, on the technical situation at the time the instrument was designed. One possibility discussed by the Group was that the actual diameter might substantially exceed 120 inches, but with the image size corresponding to a diffraction-limited 120-inch mirror. Such an increase in light-gathering power would be desirable for many researches and might be technically feasible if a corresponding decrease in angular image diameter were not required. (If the Saturn V were used to place the LOT in orbit, and the primary were a single mirror, the diameter could not exceed 250 inches; without doubt, other engineering considerations would limit the diameter to a substantially smaller figure.)

Role of Man

It was generally agreed that the LOT should be usable for many decades, with occasional changes and improvements in the instrumentation provided at the focal plane. This requirement can presumably not be met unless a man is intimately involved in maintaining and repairing the equipment, and presumably a man will also be required for the initial adjustment and operation. The design of the LOT should provide for ease in trouble shooting, for access to all parts of the telescope, and for replacement of defective modules. The extent to which a man should actually operate the telescope is a matter of debate, and it is not excluded that the entire system should be completely automated. Guidance on stars will presumably be automatic, and, during this time, man should probably not be coupled to the instrument. However, guidance by man might prove useful for observations of a rotating planet, for which automatic guidance would be difficult. Similarly, in a crowded star field, acquisition of the desired object by a man might be useful, though this could be done through use of a television camera rather than by looking through the telescope. There was agreement that the instrument should be completely controllable at will, either by equipment on the ground or by a man nearby. There was some discussion of the likelihood of failures resulting from human error.

Location

After reviewing the recommendations of the Report on Lunar Exploration Systems after Apollo (LESA, North American, 1965), the Group discussed the relative advantages of the following three different locations for a large space telescope: low orbit (below the Van Allen belts), at 400 km altitude or less; high orbit (above the Van Allen belts), at 30,000 km altitude or more; and on the Moon. Most of the considerations examined would appear to favor the high orbit. As compared with location on the lunar surface, the advantages of a high orbit include no gravitational flexure, no secondary micrometeorites, and lower cost. A possible major disadvantage of the high orbit is greater risk of exposure of equipment and men to high-energy radiation from solar flares, though evidence presented to the Group suggests that adequate shielding is no problem. Objects close to the Sun, however, might be more difficult to observe from a high orbit than from the lunar surface. As compared with a low orbit, the advantages of the high orbit are: negligible occultation of objects by the Earth regarded the schedule as optimistic, others were of the opinion that a substantial acceleration would be possible.

Table 1. Proposed Schedule— Large Orbital Telescope and Related Programs

Calendar Year	Run'g Years	Stage of Development	Launch of OAO	Manned Operation Gemini/Apollo	Corresponding dates for 40 - 80 inch AES telescopes
1966	0	Application for funds, at least for the early phases	A1		1966
1967	1	Preliminary design studies	B A2	Henize: ultraviolet, small unstabilized camera/Gemini	1966.5
1968	2	Detailed design; begin breadboarding, etc.	C		1967
1969	3		D*	U. Arizona: 6-inch ultraviolet Schmidt/Apollo	
1970	4	Start construction of prototype	E** F**		1968
1971	5		G** H**		
1972	6	Deliver first of two blanks for primary mirror†	I**		
1973	7	Figuring, testing mirrors			
1974	8	Figuring, testing mirrors			
1975	9	Figuring, testing mirrors			

Table 1, continued

Calendar Year	Run'g Years	Stage of Development	Launch of OAO	Manned Operation Gemini/Apollo	Corresponding dates for 40 - 80 inch AES telescopes
1976	10	Prototype completed			1970
1977	11				
1978	12	Flight-model completed			1971.5
1979	13	Launch on Saturn V			1972.5

* Approved by NASA, but not funded.

** In present long-range planning only, not approved.

† Optical work on primary and secondary mirrors of main optics can proceed independently of the main telescope construction, at least in part. Figuring and testing of flight-model mirrors can continue until completion of flight model.

ADMINISTRATIVE PROBLEMS

Three different phases of the program were considered: (a) preliminary phase, (b) design and construction, and (c) post-launch operation. As entirely different administrative problems would be encountered in each of these phases, they are discussed separately here.

Preliminary phase

Such a major astronomical effort as the LOT should not be undertaken until a majority of the astronomical community supports the program with enthusiasm. It appears to the Working Group that progress in space research generally, and in space astronomy particularly, combined with increasing awareness of the close interdependence of space astronomy and ground-based astronomy, may help in generating enthusiasm for the LOT among U.S. astronomers.

To help in explaining LOT plans to their colleagues, and in pressing for the program generally, the Working Group concluded in effect that the Group as a whole, or a representative fraction of it, should continue in existence, as an *ad hoc* panel, and requested the National Academy of

Sciences to endorse a proposal to this end as contained in Recommendation 8, page 3). The purpose of the panel would be:

(i) To attempt to broaden the base of support, for (a) the space astronomy program in general, and for (b) an eventual launching of a large astronomical instrument in particular. By discussion with their colleagues, they would hope to clarify the issues involved and to stimulate the interest of astronomers who are at present unfamiliar with the aims of the space program.

(ii) To begin an orderly examination of some of the technical problems that will arise in the design of a large orbiting telescope, anticipating that more permanent arrangements will be made later.

(iii) To implement these two aims by holding fairly frequent informal meetings, preparing discussions of specific subjects, inviting the participation of other astronomers, and generally to keep alive the idea of working toward a large orbital telescope.

Design and construction phase

In the initial organization of the program and during all successive stages until launch, there must be close and effective contact between NASA and its engineering contractors, on the one hand, and the astronomical community on the other. How this contact can best be maintained and integrated into the vast administrative structure required for such a large program is a question that deserves careful study. Perhaps a group of astronomers might be organized to carry out detailed design studies, with advice from engineers and optical experts; such a group might then serve in an advisory capacity during the engineering design phase that would follow. Perhaps a committee under the National Academy of Sciences, with representatives from various interested groups, might serve a useful function in this context, and might help to provide a bridge between the NASA organization for the LOT and the scientific community. Further exploration of these and other possibilities is desirable.

Operations phase

Clearly, the LOT would be a truly national facility, and should be administered as one. The plan should be workable from the standpoint of NASA's internal administration, since the situation would be complicated by the fact that flights would be involved. The Working Group visualizes that the detailed program for operating the LOT (allotment of observing time, expeditious recovery of data, proposals to place auxiliary instruments of newer design on board, etc.) would need to be managed in a way analogous to present ground-based national facilities. Responsibility for detailed scheduling must be defined, as it would depend not only on the scientific program but also on such factors as the relative position of the telescope, the Earth, the Sun, the object to be observed, communications, etc. Experience with the OAO-D program, in which two-thirds of the observing time will be allotted to guest investigators (i.e., investigators other than the principal investigator, who is responsible for the experiment), may help to reveal some of the administrative problems in these areas.

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WASHINGTON, D.C. FEBRUARY 1966

THE ROLE OF MAN

The Biology Working Group limited its consideration of this topic to those factors that bear directly on the performance of fundamental biological research in space.

The limitations imposed by the space program in weight and volume require that decisions be made repeatedly as to the relative effectiveness of experiments or sampling performed by instruments and by man. The advantages of using man increase with the need for interpretive or value judgments during the course of a mission.

The basic roles of man in the space-research program are (1) pilot, (2) scientific technician, and (3) observer-experimenter.

(1) The need for highly trained pilots or astronauts is evident, and an important part of biomedical research is concerned with the well-being of astronauts in the space environment. However, fundamental biological studies are of a different order and type from those concerned with astronaut safety. In addition, experience has shown that pilot-astronauts cannot be expected to have much time for scientific observation.

(2) The title "scientific technician" implies the performance of preprogrammed observations. Many kinds of measurement can be made automatically and telemetered to Earth, and others can be stored for later recovery. To make other measurements it may be more economical in terms of volume and design to use a human observer in accordance with a specified program that may be established before flight or altered according to the measurements resulting during flight. A technician-astronaut must be carefully trained and he must have sufficient motivation.

(3) A third role of man for scientific observation in space is to

perform tasks that cannot be adequately anticipated or automated. By trying to preprogram all details, a rigidity can result that could limit the scientific yield of the mission.

In some missions, as in the exploration of the Moon or of Mars, observational skill will be paramount; in others, as in an extended Apollo or MOL program, experimental skill is more important. Ideally, the two competences can be combined, maintaining both at high levels in the same scientist-astronaut. He must also be trained to understand the role of the pilot-astronaut and, if only in a limited way, to be able to substitute for him in emergencies. Missions should be so designed however, that the scientist can devote his principal attention to scientific duties. Pilot-astronauts must regard such scientists as working passengers who cannot be expected to take on many crew responsibilities.

Types of Biological Operations Requiring Man

There is a broad overlap in what can be done by instruments, by pilot-technician, and by scientist-astronaut. In general, automated devices will be employed unless weight and complexity indicate that it will be more feasible to use man. Examples of situations in which man is preferable are:

A. Exploratory Observations. Because conditions on Mars may permit some type of biochemistry that differs from that on Earth, keen observational skill will be needed to discover it. It cannot be predicted what may be found on the Moon and Mars that might have biological relevance, and, although much information can be obtained from fly-bys and unmanned landings, the unexpected can be reported only by trained human observers.

B. Experiments on Man. Mainly for practical reasons, but partly also because of more fundamental considerations, the need is anticipated for a continuing program of physiological and psychological studies on man in space. These involve the sum of space stresses, particularly weightlessness over long periods of time. Areas of physiological interest, such as those listed below, are mainly the same as those for shorter periods: (i) cardiovascular physiology, particularly vasomotor tone, venous return, and cardiac output; (ii) optomotor responses, subjective orientation, visual discrimination; (iii) postural reflexes, effects on the central nervous system of deficit of proprioceptive input; (iv) metabolic alterations such as those that can be studied best by repeated blood and urine analysis; and (v) functions dependent on cell division, such as hematopoiesis in which a synergism between weightlessness and radiation is possible. The scientist-astronaut (passenger) is the best choice as the subject of physiological experiments because of motivation, experience, and freedom from competing duties. Since the number of available subjects is necessarily small, crew members other than the scientist should also be employed for this purpose. Special training of the astronauts will be necessary, as will full appreciation by scientists of those several factors that limit the usefulness of astronauts as experimental subjects.

C. Experiments on Organisms Other than Man. As knowledge increases, the kinds of study made in the space environment will become more sophisticated and the need for trained human experimenters will increase. Specific areas of experimentation that will utilize vehicles to study basic biological problems were treated above, in the section on environmental biology. The following discussion reviews the scientific reasons for the use of scientist-astronauts for experiments on animals and plants:

1. Experiments on Mammals Other than Man. Long-term tolerance studies will be difficult to interpret and of relatively little significance unless numerous physiological measurements are made during the state of weightlessness. These measurements will include blood and urine tests (chemical), microscopic examination of blood, and function tests that cannot be satisfactorily automated. Also, it may be expected that effects on growth and development will be found that cannot be seen in adult animals; hence, it will be necessary that animals be bred and reared in space. Breeding and maintenance of even a small colony of rodents can best be done by a scientist-astronaut, who can observe unexpected effects and can provide care as needed. Complex measurements of circulatory and muscular function that cannot be done safely on man, especially those requiring catheterization, should be done on mammals by scientist-astronauts.

2. Experiments on Nonmammalian Organisms. Simple manipulations, such as fixation of cells for later cytological study and photography of growing plants, can be done by instruments. However, microscopic observation in space vehicles requires preparing and mounting the objects, focusing, and visually identifying structure. Microscopy has never yet been fully automated on Earth. In some microscopic preparations fixation and staining will also be needed. It is probable that in the weightless state, more basic information will be obtained on cellular functions in developing organisms than in adults. Germination of seeds and fertilization of sea-urchin or frog eggs, for example, can be automated, but sophisticated observations on the course of their development require the judgment of a trained biologist. Another type of experiment for which man will be particularly useful is in biorhythm tests requiring manipulations on the organism. For example, transplantation in space of organs such as the corpora allata in cockroaches may be needed to localize environmental effects on rhythmic phenomena at the tissue or organ level.

3. Martian Observations. After a manned landing on the Martian surface, the elaborate automated laboratory technology that ought to have been brought to an advanced state by that time will be used by man rather

than replaced by him. Man's greatest contributions will be to shorten the control loop by being closer to the Martian laboratory and to aid in the general problem of sampling.

Vehicle Size

The programs now visualized of experiments on man himself, or on other animals and plants, can be accomplished with only a modest increase in vehicular capability. The principal need is for more space. A very large manned orbiting laboratory (with crew of 10 or more) could be employed but would not be required for the studies envisioned. The Extended Apollo system seems to be all that is necessary for these biological experiments involving scientist-astronauts.

Criteria of Choice Between Automation and Manned Observation

The question of where to place man in the framework of scientific observation has no general answer, and the final decision in each situation must be based on the balance of numerous criteria. Size of instruments and power requirements for automation must be balanced against the size of a man and his life-support system. The complexity of the experiment and nature of the information desired must be balanced against the flexibility, motivation, and judgment of a man.

In recording, storing, and transmitting bits of information, man is clearly inferior to machines. For certain kinds of preprogrammed observations such as photography, optical measurements, pressure determinations, and some chemical measurements, and also for simple manipulations such as histological fixation and animal feeding, instruments are superior or equal to man.

At the level of complexity above mere information transfer, remote control devices can be perfected, although they may be limited by the telecommunication system. One proposal of the 1962 study has not been explored adequately for space application, namely, the development of a telepuppet. This is an extension of remote handling of equipment: it is a machine that mimics the actions of man. It is controlled by a human operator whose movements are reproduced by the telepuppet in real time.

Design of an experiment or observation so that it can be conducted by automation may impose such restrictions that the biologist is discouraged from making important proposals for flight programs. Most scientists are accustomed to designing experiments to be performed at the laboratory bench rather than remotely; for space research, however, it is desirable that flight experiments be designed so that man, whenever possible, can carry them out using more or less familiar procedures.

Insistence on minimal packaging may provide economy of payload, but it may be unrealistic if it should reduce the possible number of measurements such that the value of the experiments is degraded and the potential of the investigator is not fully exploited. Considerations of weight and volume may have to be balanced against scientific excellence. Closer cooperation between the scientific investigator and the engineer, and between the investigator and the astronaut, will be needed. It is patently important that both the engineers and the members of the astronaut team clearly understand the meaning of each scientific experiment on a given mission.

Selection and Training of Scientist-Astronauts

A first assumption is that most scientific missions in space, whether of exploration or experimentation, will be designed by scientists who do not themselves make the flight. Hence, it is necessary, when a decision to use human observers is made, to have men of the highest scientific ability. Perceptiveness, ingenuity, and scientific originality are more important qualities than quick thinking and short reaction times for trained responses. Motivation and breadth of scientific background are equally important. The scientist-astronaut must be of outstanding caliber, at the Ph.D. or M.D. level.

The selection program should be broadened to include the scientist-passenger. Physical requirements should be consistent with the performance of the required tasks, but sufficiently elastic to include men of superior ability even though they may not meet the standard requirements for astronaut selection, such as those for height and vision. Intellectual and psychological qualifications are more important than physical qualifications except, of course, for those minimum physical requirements related to resistance to the stresses of space flight.

SCIENTIST-PASSENGERS

Scientist-astronauts—persons combining the skills of pilot and scientist—are already engaged in the manned-space-flight program and are expected to participate in the later Apollo missions. Substantial demands on their time and energy will be made in operating the spacecraft. Thus they may not be able, even with the aid of pilot-astronauts, to carry out the full-scale biomedical program discussed above. Therefore, scientist-passengers, able to devote their entire time and concentration to scientific investigations, should be included as crew members of future missions.

ASTRONAUT SELECTION AND TRAINING

As the space program evolves toward multimanned crews and longer flights, problems of crew structure and compatibility, task allocation and competence for emergency procedures, and crew time available for experimentation will emerge as important elements. Thus crew selection, training, tasks, and composition must be continually evaluated and adjusted to meet changing requirements. For example, physical and medical standards for crew selection should in the future depend upon the state of spaceflight technology, upon specific mission requirements, and upon the specialized types of astronauts sought. The imposition of the same minimum physical standards for scientist-passengers as for pilot-astronauts may prove unnecessarily stringent and could result in an excessive limitation on scientist-passenger candidates at the expense of needed scientific qualifications.

Specific types of medical evaluation during the selection process may warrant greater emphasis; for example, predisposition to nephrolithiasis, sensitivity to labyrinthine stimulation, and individual nutritional requirements. Criteria must be developed also for the assessment of individuals for group leadership, for the evaluation of personal compatibilities, and for the selection and composition of crews in terms of best sustained group performance. Training programs for scientist-astronauts and scientist-passengers should omit teaching of unnecessary engineering and piloting skills. Training programs must, however, maintain and augment the scientific interest and competence of participating scientists.

XI
Role of Man in Space Research

INTRODUCTION AND CONCLUSIONS

The Working Group on the Role of Man in Space Research was drawn from participants in each of the other Working Groups of the 1965 Summer Study at Woods Hole. Meeting on July 6-7, it discussed, in broad terms, the limitations and advantages of man as an experimenter in space, the nature of the role or roles to be played, the relationship of manned and unmanned space research, and the selection and training of flight personnel. Also discussed were the scientific experiments planned for the current Gemini and Apollo programs, and the scientific importance of large manned orbital laboratories capable of flights of extended duration. The Working Group was not designed to study these questions exhaustively but rather to assess generally and informally, on the basis of the disciplines and experience represented by those participating, the scientific role of man in space research.

The general conclusions of the Working Group are as follows:*

1. Scientifically satisfying studies of the planets will require the presence of scientists, preferably on the planetary surface where they can make direct observations. If that is not feasible they should at least be in a spacecraft orbiting closely enough to the planet so that communication time delay and power bandwidth considerations will not seriously limit the performance of remotely controlled instrumented vehicles on the planet. It is clear that here man is essential.

2. There are many ways in which men can be usefully employed in space for scientific research: as observers; for the assembly, placement, repair, and operation of scientific instruments; for preliminary analysis, screening, sampling, data collection, storage, and retrieval.

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*Each of the Working Groups of the 1965 Summer Study gave consideration to man's role in space research; specific comments may be found in their individual chapters.

3. Few if any scientists would attempt to justify the entire cost of developing manned space flight solely on the basis of its "scientific value;" however, most scientists would agree that this capability, when developed, should be utilized for scientific purposes whenever it seems possible to do so.

4. There appear to be no a priori reasons why the physiological and behavioral stresses imposed on man by the space environment should prevent him from surviving and functioning effectively in space during missions at least of Apollo duration.* Such was the judgment of the Summer Study's Working Group on Medicine and Physiology (see Chapter X). The Working Group on the Role of Man in Space Research also believes that most physiological constraints will yield to work now in progress, and that man may in the future embark on longer missions with reasonable confidence that he can perform meaningful scientific studies. It was clearly recognized by all participants, however, that ground-based research must be carried out on those human systems likely to be affected by the space environment (e.g., cardiovascular, respiratory, renal, and digestive).

The experience of the United States and the Soviet Union in increasingly longer manned flights, and in extravehicular activity, together with the progress that has been made in preparation for manned lunar landings, has stimulated increased interest in directly using man's abilities to explore space—from within spacecraft, in appropriate pressure suits outside the spacecraft, and on the surface of the Moon or planets. Certainly many questions regarding man's adaptability to the space environment remain unanswered, but given the opportunity to progress in a scientific and orderly way to successively longer and more complex missions, it is expected that spacecraft and life support developments will emerge that would enable him to live in space for protracted periods and to accomplish almost any desired task. As a result, scientists will become progressively interested in the ways that men can be used to advance scientific research in space—as scientific observers, as highly skilled technicians, and as the subjects of experiments themselves.

The Working Group visualized four immediate types of programs utilizing man for scientific studies in space: (1) The current Gemini and Apollo programs, which can carry modest scientific experiments to be performed by the astronauts, but where scientific objectives are secondary to engineering and human survival; (2) later manned orbiting laboratory flights, such as the Manned Orbiting Laboratory (MOL), Apollo Extension Systems (AES), or the Large Orbiting Research Laboratory (LORL), and their modifications, where an important objective of the mission is to carry out relatively demanding scientific measurements and observations; (3) large, complex, special-purpose space assemblies, such as optical or

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*Note added in proof: Astronauts Borman and Lovell in GT-7 recently completed a 2-week orbital flight, with no apparent ill effects.

radio-astronomical observatories that are designed specifically to be assembled, aligned, serviced, or operated by men in space; and (4) scientific operations on lunar or planetary surfaces.

ROLE OF MAN IN SPACE RESEARCH

In order that the greatest scientific returns be achieved by including man in space research, those capacities that make him superior to automatic or remotely operated equipment must be fully utilized.* For example, man can make substantial contributions in tasks that require scientific judgment, e.g., studying data and evaluating preliminary results; making a complete record of an experiment or group of experiments being done simultaneously; making decisions as to which scientific results are significant and should therefore receive further study; planning a program of observations on the basis of results as they are obtained; operating complicated instruments and equipment so that maximum and most significant results are obtained; utilizing his ability to reflect, theorize, and make and test hypotheses in order that full advantage may be taken of every opportunity; surveying a large field of view from an orbiter in order to select specific areas and times of interest for photographic recording and to supplement such records with descriptions of significant phenomena.

While the foregoing deals with the role of man as a scientist, there are other roles that require a high level of intelligence, ingenuity, and technical skill. In this category may be included such tasks as the return of samples or data in physical form; alignment and adjustment of precision equipment; placement of complex apparatus, especially that which requires protective cover during launch and flight; removal of worn-out or nonfunctioning subsystems; and equipment maintenance and servicing. (One has only to reflect on the extreme difficulty of maintaining complex precision equipment on Earth to realize the importance of maintenance in space.)

RELATIONSHIP OF MANNED AND UNMANNED RESEARCH

In deciding whether to use man as part of a data collection and analysis system, or to have it completely automated, scientists would certainly

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*For a discussion of this subject, see Appendix 8 of Chapter X.

favor using man if (1) it is the only way to obtain the necessary data or (2) it is the most economical way in the broader sense, i.e., in terms of expenditure of time and effort per unit result. Finally, if the presence of man in the system were already considered essential, the scientist would assign many scientific tasks to man because of the greater reliability and flexibility he would bring to the system.

An unmanned system has only an estimated statistical probability that it will continue to operate reliably for its designed lifetime; manned intervention increases reliability through the possibility of extending the lifetime of spaceborne equipment almost indefinitely by means of repair and replacement. Intervention by man greatly increases the flexibility of the system, for he can decide not only the most interesting or profitable way to use an instrument at a particular time but also can make alterations and improvements in the instrument itself. Data transmission can, of course, be virtually eliminated for manned experiments, and the design of an instrument can be simplified if every operation does not have to be automated. Furthermore, several lines of investigation may be carried on simultaneously. Most importantly, man can select significant items from a large field of view and notice relationships among phenomena that no preprogrammed instrument can discern.

Since such abilities make the presence of man very attractive for most investigations, the Working Group felt it necessary to address itself to the questions: (1) At what point does it become justifiable to utilize man if he is not essential? (2) Is such use ever justified when the economic trade-off is not clear-cut? This section will summarize some of the Working Group discussions that occurred.

MONITORING

Unmanned spacecraft should, in general, be used for the monitoring, continuous measurement, or sampling of physical variables (e.g., solar radiations of various kinds, particle fluxes and magnetic fields, components of the neutral and ionized atmosphere). The physical variables of interest in such researches are known; the data are routine and in themsleves not very exciting from minute to minute; the data gathering process requires no exercise of judgment and there is generally no reason why a man should be present while it is going on. However, when it is necessary to monitor simultaneously a large number of critical variables in order to gain a complete picture of some typical process, a man can be useful. An example can be found in the study of the physics and large-scale behavior of the upper atmosphere of a planet—meteorology, in the broad sense of the word. In such a situation each instrument can be automated easily enough and have a statistical probability of working for the required interval, but it may be much more difficult to achieve satisfactory over-all reliability for the complete system.

SEQUENCE OF MANNED AND UNMANNED INVESTIGATIONS

Manned and unmanned investigations can and should be arranged so that each complements the other most effectively. Unmanned investigation of hostile environments is obviously desirable in advance of manned exploration. Thus, the programs of unmanned lunar investigations—the Rangers, Surveyors, and Lunar Orbiters—in preparation for Apollo are wise, and unmanned exploration of Mars should be done in the same way.

Examples of projects which should be manned before being completely automated can be drawn from other chapters of the Summer Study report, for example, in Chapter II. Here, the Working Group on Optical Astronomy cites justifiable misgivings on whether a very large diffraction-limited telescope mirror can be figured on the ground under normal gravity and still be made to retain its figure under weightless conditions. That Working Group also expresses concern with the technical problem of exact optical alignment after the telescope has suffered the stresses of launch. There is a desire to see whether these problems can be solved with a man present to carry out the trial-and-error approach that has always been found essential on the ground. A comparable procedure will doubtless be proposed for the Apollo Extension Systems. The current Gemini and Apollo programs can conduct simple photographic surveys to "see what is there" in order to optimize the design of, and to plan observing programs for, larger and more complex instruments.

PREPARATION FOR LONG-DURATION MANNED SPACE MISSIONS

If manned expeditions are to go someday to a planet (e.g., Mars), a manned space station clearly becomes essential for the gathering of the medical, physiological, and behavioral data needed to determine how man may survive in space in reasonable comfort for periods of 2 or 3 years.

COMPLEX SCIENTIFIC RESEARCH PROGRAMS

An orbiting station will probably be necessary to carry out the biological experiments proposed by life scientists, such as those requiring very detailed and selective manipulation of biological specimens, examination and recording of selected events under a microscope, the rearing in space of generations of advanced life-forms and their use in experiments, and surgical or similar procedures using plant forms. Large systems like the large orbiting telescopes proposed by the astronomers (Chapters II-V) are somewhat less complex: it is possible to contemplate automating all the steps in their operation so that such systems could be kept running efficiently between occasional manned visits for maintenance. A manned space station within easy reach of a large orbiting astronomical facility would permit the design and management of that facility to be greatly simplified. Astronomers have been considering the advantages of a system comprising a telescope and a space station (near by or very loosely coupled to the telescope) so that man can get aboard quickly and frequently when needed but not be present while the telescope is operating in the fine-pointing mode.

SELECTION AND TRAINING OF SCIENTISTS FOR SPACE MISSIONS

The Working Group examined the relative merits of training of scientists as astronauts or training astronauts as scientists. For some tasks—those requiring scientific insight—it would seem better to have a scientist possessing judgment, experience, and imagination and to train him as an astronaut to the extent required. However, for duties other than these, it would probably be preferable to train astronauts to carry out scientific activities or at least to work as technicians and observers. Thus, an experienced field geologist would almost certainly have greater grasp of and insight into what he observed and sampled on the lunar surface than would an astronaut, regardless of high motivation or a technical background sufficient to learn basic geology in short order. But it also seems clear that for maintenance, repair, or almost any task where manipulation rather than scientific observation is important, an astronaut is probably to be preferred to a scientist, who may or may not be familiar with such tasks. Furthermore, in a situation where a variety of straightforward scientific tasks in quite different fields must be performed, there is no reason to believe that the competent scientific specialist will be more expert outside his own field than a highly motivated and well-trained astronaut starting with a good technical or general scientific background.

The Working Group further agreed that (1) it is timely to consider ways in which "scientist-passengers"* can be incorporated into space crews. As scientist-astronauts, older, more experienced scientists might be disqualified on age or medical grounds; these requirements should now be re-examined. For example, in situations requiring visual scientific observation, experience in interpretation is much to be preferred over acute naked eye vision. It would appear that medical requirements for the

*See A Review of Space Research, National Academy of Sciences—National Research Council Publication 1079 (1962), Chapter 11.

scientist-passenger should be progressively relaxed to the point where a candidate would be disqualified only for defects that would make him a hazard to the mission. (2) Although the pilot-astronaut will be essential for a long time to come, training of a scientist-astronaut as a jet pilot might be reduced and perhaps finally eliminated. Experience in simulators suggests that the control of a spacecraft (attitude control, rendezvous, docking, re-entry maneuvers) is not intrinsically too difficult for a reasonably well-coordinated person to learn. However, jet and test pilot training contributes one valuable ingredient: it cultivates a continual, almost subconscious awareness of the possibility that an emergency may occur at any moment and, by constant practice in dealing with simulated and real emergencies, gives confidence in one's ability to deal with them.* Ways should be sought by which this process can be accelerated and done more simply.

In summary, the Working Group suggests that the astronaut selection and training program take into account progress in manned space flight. Successes so far may be viewed as evidence that it may be possible to relax the present high physical standards at a pace faster than has yet been contemplated.

GEMINI, APOLLO, AND SCIENTIFIC FLEXIBILITY

In reviewing the presently planned Gemini and Apollo scientific program (see Appendix 2), the Working Group affirmed its value as an exploratory program and commended the use of Gemini and Apollo to try out simple ideas and experiments, especially those that might be regarded as forerunners of more ambitious projects for the Apollo Extension Systems and later systems. This may be the most valuable opportunity for science in these early manned flights.

The Working Group recognized that engineering and mission constraints prevent the derivation of maximum potential scientific benefit from the Gemini and Apollo programs. The primary purpose of these flights is to develop systems able to support man in space for limited durations and to assure his safety, within reasonable limits, during such flights; scientific activities must be carried out on a noninterference basis, subject to cancellation. Despite these constraints, it is likely that numerous opportunities will exist, up to within a few weeks of launching, to place modest

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*The performance of the crew during the abort of the October 25, 1965, launch of GT-6 provides a good illustration.

experiments on manned space flights. The Working Group suggests that no such opportunity be lost and encourages NASA to examine ways to exploit them.

While the Working Group subscribed to the existing goals of the Gemini and Apollo programs, and recognized that the predominance of scientific objectives must be deferred until those goals are met, it wished to emphasize the following point: In order to attract many competent research scientists to the type of space experimentation that involves man, it is desirable to create a reasonable level of assurance that the time and effort they invest in planning and preparing a manned flight experiment will lead to the accomplishment of that experiment. Some risk must, of course, be accepted, but it is important that an effort be made to minimize it. Such considerations may suggest that some adjustments in procedures and attitude toward the experimenter may be necessary to encourage the scientific community to use this new capability in space research.

B-V

APPENDIX TO CHAPTER V

RESEARCH AND DEVELOPMENT: AERONAUTICS AND SPACE

TABLE OF CONTENTS

RESEARCH AND DEVELOPMENT FUNDING BY PROGRAM

Extracted from NASA Pocket Statistics, September-October 1968, pp. 68-69

R&D FUNDING BY PROGRAM

(In Millions of Dollars) As of 31 August 1968

	FY 1969 BUDGET REQUEST	PROGRAM PLAN FY 1968	FY 1967	FY 1966	FY 1965	FY 1964	FY 1963	FY 1962	FY 1961	FY 1960	FY 1959
MSF											
Apollo Applications	439.6	251.1	80.0	26.4	—	—	—	—	—	—	—
Mercury	—	—	—	—	—	—	.1	15.1	105.4	83.1	38.8
One Day Mission	—	—	—	—	—	—	16.6	16.5	—	—	—
Gemini	—	—	15.2	197.3	308.3	418.8	289.1	55.0	—	—	—
Apollo	2,038.8	2,537.9	2,922.6	2,941.0	2,628.9	2,277.9	1,135.0	451.6	190.9	36.3	10.9
Spacecraft	(--)	(1,096.4)	(1,310.6)	(1,234.1)	(1,024.1)	(877.2)	(363.0)	(73.4)	(--)	(--)	(--)
Saturn I	(--)	(--)	(--)	(.4)	(37.7)	(186.1)	(254.8)	(192.7)	(98.5)	(9.6)	(--)
Saturn IB	(--)	(139.1)	(225.5)	(273.3)	(254.9)	(147.1)	(18.4)	(--)	(--)	(--)	(--)
Saturn V	(--)	(1,007.9)	(1,098.0)	(1,134.9)	(967.5)	(763.3)	(340.2)	(57.4)	(--)	(--)	(--)
Engine Dev.	(--)	(20.5)	(49.8)	(133.2)	(166.3)	(166.0)	(136.3)	(103.9)	(77.8)	(26.7)	(10.9)
Mission Support	(--)	(274.0)	(238.7)	(165.0)	(168.3)	(138.2)	(72.3)	(24.1)	(14.6)	(--)	(--)
Ad. Manned Missions	5.0	--	6.2	10.0	23.9	21.2	11.2	--	—	—	--
TOTAL MSF	2,483.4	2,739.0	3,024.0	3,174.3	2,951.0	2,717.9	1,532.0	538.0	296.3	119.4	49.7
SSA											
Physics & Astronomy	141.9	138.5	122.9	140.8	133.7	145.8	112.5	79.3	34.9	20.6	6.2
Lunar & Pl. Expl.	107.3	146.4	191.0	251.3	202.6	204.2	170.2	111.6	61.2	19.7	12.2
Manned Flight Exp.	--	—	7.9	--	9.0	5.6	—	—	—	—	—
Sustaining Univ. Prog.	—	—	—	--	--	—	30.6	13.1	—	—	—
Launch Veh. Dev.	—	—	31.2	58.4	96.5	111.7	100.2	83.7	77.0	52.1	24.5
Veh. Proc./Unman.Mis.	128.3	123.6	117.6	178.1	154.5	130.0	115.7	97.8	38.2	8.2	3.5
Bioscience	48.5	41.5	42.8	36.7	27.7	21.2	13.5	3.0	3.6	—	—
(Meteorological Sats.) ‡	--	--	(41.6)	(1.2)	(30.7)	(61.8)	42.4	27.1	14.4	7.9	1.3*
Space Applications	112.2	98.8	71.3	78.4	63.8	88.2	34.9	22.6	17.9	3.6	3.6
Other Completed Mis.**	—	--	—	—	--	—	--	--	12.8	35.8	77.2
TOTAL SSA	533.2	548.9	583.8	744.9	637.8	706.8	620.1	438.4	259.8	147.9	128.5

‡ Met. Sats. now Included in Space Applications which also includes Communications and ATS.

* Does not Include DoD Tiros Funds.

R&D FUNDING BY PROGRAM

(In Millions of Dollars) As of 31 August 1968

	FY 1969 BUDGET REQUEST	PROGRAM PLAN FY 1968	FY 1967	FY 1966	FY 1965	FY 1964	FY 1963	FY 1962	FY 1961	FY 1960	FY 1959
ART											
Basic Research	22.0	21.3	22.0	22.0	21.2	22.5	17.7	7.7	2.6	—	—
Space Vehicle Systems	35.3	33.8	34.0	35.0	44.2	45.7	37.5	8.4	3.0	1.5	—
Electronic Systems	39.4	37.8	33.6	32.4	25.6	28.7	17.0	5.4	3.0	1.5	—
Human Factor Systems	21.7	19.7	16.4	14.9	13.3	13.2	9.6	5.3	.5	—	—
Sp. Pwr. & Elec. Prop. Sys.	44.8	43.5	40.6	45.2	58.2	60.0	47.3	24.1	14.0	6.6	.8
Nuclear Rockets	60.0	53.6	53.0	58.0	57.0	79.1	69.4	27.0	6.7	4.0	3.2
Chemical Propulsion	36.7	36.7	33.6	39.7	78.3	46.0	49.6	23.0	1.4	3.3	6.6
Aeronautics	76.9	66.3	35.9	41.7	35.2	21.8	15.9	2.0	.6	4.9	3.0
TOTAL ART	336.8	312.8	269.1	288.9	333.0	317.1	263.9	102.8	31.9	21.8	13.6
TDA											
TOTAL TDA	304.8	275.2	270.9	231.1	253.2	191.4	121.5	65.6	29.7	16.2	3.5
OFC. OF ADMIN. TOTAL	—	27.2	—	—	—	—	—	—	—	—	—
TECH. UTILIZATION											
TOTAL TU	4.0	4.0	5.0	4.8	4.8	3.5	2.2	.3	—	—	—
UNIVERSITY AFFAIRS											
TOTAL UA	10.0	9.9	31.0	46.0	46.0	40.0	—	—	—	—	—
TOTAL	3,677.2	3,967.0	4,191.0	4,490.0	4,285.8	3,976.7	2,307.7	1,145.2	617.7	305.3	195.3
Approp. Transfers & Adjustments		42.0	54.0	41.0	77.8	50.7	50.5	157.3	54.4	42.2	1.3
Approp. and Availability Total ‡		3,925.0	4,245.0	4,531.0	4,363.6	3,925.0	2,457.2	1,302.5	672.1	347.6	196.6

** Not the responsibility of the present Program Offices. Includes Vanguard.

† Largely funded from Operations through FY 1962.

‡ See "Appropriations Summary" page.

NUCLEAR ROCKET PROGRAM ACTIVITIES 1959-1968

INTRODUCTION

As the KIWI-A series of reactor tests was being completed (1960), steps were being taken to expand the nuclear rocket program into a new chapter which involved, in addition to reactor research and development, rocket engine development and plans for flight testing. Four contractors began studies to define problems connected with flight testing a nuclear rocket under the RIFT (Reactor In Flight Test) project. Another important step was the establishment in 1960 of the joint AEC-NASA Space Nuclear Propulsion Office for management of all nuclear rocket propulsive activities in both agencies.

In May 1961, President John F. Kennedy, in an announcement that established the Apollo program as a national goal, recommended development of nuclear rocket propulsion technology and requested Congress to add $30 million to the FY 1962 ROVER program budget. The NERVA project was born and NASA and AEC awarded a contract to Aerojet-General and Westinghouse for the development of the Los Alamos KIWI-B reactor into a NERVA nuclear rocket engine for flight applications. The NERVA engine of that period was defined in part by a power specification of 1100 megawatts which would provide a thrust level of approximately 55,000 pounds. An engine systems specific impulse of 760 seconds was predicted for operation of the engine in space.

Late in 1961 the first of a second (KIWI-1B) series of reactors was tested at the Nevada Test Site by LASL. This series of reactors was intended to examine, more specifically, design features that would be required in a flight system. Another first was the use of a regeneratively-cooled nozzle on a reactor test. This test was operated at 300 megawatts, a limitation imposed by gaseous hydrogen cooling of the nozzle. Operation with liquid hydrogen was yet to come.

The year 1962 witnessed expansion of the nuclear rocket program when NASA, through Marshall Space Flight Center, awarded a contract to Lockheed for development and flight testing of the RIFT stage with a NERVA propulsion system. Serious troubles, however, were to be encountered during reactor testing.

Although a second KIWI-B reactor test, KIWI-B1B, showed that rocket reactors could be stably operated with liquid hydrogen at high pressures, high-power levels and high-flow rates, this test was followed by a test of a flight-type reactor designated KIWI-B4A which suffered a structural failure later proven to be caused by mechanical vibrations induced by the flow of hydrogen in the reactor core. Further "hot" reactor testing was postponed until solutions to these structural problems were found as demonstrated by the recent history of the nuclear rocket program.

GROUND TECHNOLOGY PHASE

In 1963 the third chapter in the nuclear program came to a close when the NERVA project objective was changed from the development of an engine to be flight tested in the RIFT stage to the accumulation of technology for nuclear rockets via ground-based systems tests. The RIFT project was cancelled because it was proceeding much in advance of potential mission needs. Utilizing reactor and engine hardware designed for 1100 megawatts, the NERVA project assumed a technology rather than engine development orientation and objectives.

Los Alamos was to continue graphite reactor R&D to establish the basic soundness of the reactor design concept as revised through extensive analysis and component testing and then began work on advanced reactor technology under the Phoebus project. Phoebus was to lead to higher-temperature, higher power level reactors than the KIWI reactor. A specific objective--to design and test 5000 megawatt reactors, designated Phoebus-2--was established as a necessary step in the development of high-power nuclear rocket engines in the future. A thrust level of 230,000 pounds would be produced in a nuclear rocket engine rated at 5000 megawatts of reactor power. In addition, LASL planned to test several KIWI-sized reactors, labelled Phoebus-1, to aid in the general advancement of rocket reactor technology.

The contractor team of Aerojet and Westinghouse continued 1) design and development on NERVA experimental reactors (NRX) to assist in solving structural problems and to extend operating lifetime of the reactor to 60 minutes, and 2) development of a turbopump, thrust nozzle, propellant valves, turbine power control valve, control system and other components and subsystems necessary to test a complete experimental engine system (XE). However, flight requirements were eliminated and engine component development was limited to those activities essential to the conduct of systems tests or to the advancement of component technology.

NASA, through its Marshall and Lewis centers, continued some technology efforts related to nuclear rockets. Lewis Research Center examined the problems of radiation heating of hydrogen during operation of nuclear rockets. Marshall Space Flight Center conducted generalized mission studies, performed design analyses, evolved the propulsion module concept and experimented with electronic components and hydrogen-tank insulation to determine radiation effects.

RECENT TECHNOLOGY PROGRAM RESULTS

The ground technology chapter of the nuclear rocket program history is now nearing completion and has recorded much dramatic progress in recent years.

Evidence of this progress is dramatized by the list of graphite tests completed since the beginning of 1964:

1) February 13, 1964--KIWI-B4D Cold Flow Reactor (no fissionable uranium in core) demonstrated structural soundness of KIWI-B4 design under cold-flow (no power generation) conditions.

2) April 16, 1964--NERVA NRX-A1 Cold Flow Reactor qualified Westinghouse reactor design for subsequent power tests.

3) May 13, 1964--KIWI-B4D reactor tests at full design power for more than 60 seconds.

4) August 28, 1964--KIWI-B4E reactor was tested successfully at high-power conditions for more than 8 minutes. September 10, 1964, this reactor was restarted and operated a seoncd time at high power conditions in the first demonstration of a restart capability.

5) September 24, 1964--NERVA NRX-A2 reactor tested at high power for 6 minutes and demonstrated an equivalent vacuum specific impulse of 760 seconds. NRX-A2 was restarted October 15, 1964 and operated to investigate off-design performance and control margins in the low-flow, low-power operating regime.

6) April 23, 1965--NERVA NRX-A3 reactor operated at full power for $3\frac{1}{2}$ minutes. May 20, 1965 this reactor accumulated another 13 minutes offull-power operation for a total of more than $16\frac{1}{2}$ minutes endurance. NRX-A3 was restarted May 28, 1965 and operated for 45 minutes to explore operating limits in the low-and-medium-power ranges.

7) June 25, 1965--Phoebus-1A reactor tested to evaluate several detailed design improvements for extending the endurance capability of rocket reactors. After operation at rated power for more than 10 minutes, the reactor was damaged during shutdown when the liquid hydrogen supply was unexpectedly exhausted. However, substantial data on reactor design was obtained in a post-mortem examination made difficult by the damage sustained by the reactor.

8) February 3, 1966--NERVA NRX/EST (a breadboard Engine System Test) was first bootstrapped to power operation February 3, 1966 and the test series continued through March 25, 1966 in a highly successful demonstration of nuclear system characteristics. Operating time at full power was approximately 28 minutes; the total time spent in power operation was one hour, 50 minutes.

9) June 8, 1966--NERVA NRX-A5 reactor was operated at full power for 15½ minutes. An additional 14½ minutes of full power NRX-A5 operating time was accumulated June 23, 1966.

10) February 23, 1967--Phoebus-1B reactor was tested to demonstrate high power-density capability and several specific design features. The KIWI-size core was operated at a power level approaching 1500 megawatts. It operated for 30 minutes at power levels above 1250 megawatts.

11) December 15, 1967--NRX-A6 reactor was operated at full power for 60-minutes--twice as long as any previous reactor had been operated since the inception of the program.

12) April 26, 1968--XECF engine--the first down firing engine--was successfully run through a series of cold-flow experiments in Engine Test Stand No. 1.

13) June 26, 1968--Phoebus-2A reactor, the most powerful nuclear rocket reactor ever developed, was operated over a range of power for approximately 32 minutes, about 12 minutes of which was at a power level above 4000 megawatts. The peak power reached was about 4200 megawatts.

On July 18, 1968, the reactor was restarted and operated for approximately 30-minutes over a range of power levels up to 3670 megawatts.

A primary goal in the reactor technology portion of the nuclear rocket program was to demonstrate high-specific impulse operation for 60-minutes in a 1100 megawatt reactor. This goal was achieved through the testing of the NRX-A6 reactor by the Aerojet-General and Westinghouse Electric Corporations. The NRX-A6 test was the last and most ambitious reactor test in the NERVA technology phase. Specific test objectives were to explore and demonstrate the long-time operation of the nuclear rocket reactor, particularly from the standpoint of the effects of corrosion of the graphite by the hot hydrogen propellant. The performance limiting factor in this system is the corrosion reaction. Corrosion affects the lifetime capability, the temperature, and therefore, the specific impulse capability and power density capability. Taken in its entirety, the 60-minute NRX-A6 reactor full-power test illustrated the substantial system competence that had been developed in the program.

A second major goal in the reactor portion of the program was to explore increased specific impulse and power density and to scale-up the reactor system to higher power levels. This goal was achieved through the testing of the 1500 megawatt Phoebus-1B reactor in February, 1967 and through the testing of the 5000 megawatt Phoebus-2A reactor in July and August, 1968. Both tests were conducted by the Los Alamos Scientific Laboratory.

In the engine system technology portion of the program, the major goals are to determine engine operating characteristics during start-up, full-power operation and shut down, to evaluate control concepts and to qualify engine test stand operation. These goals were partially achieved through the breadboard engine tests conducted in 1966, and "cold-flow" tests of the XECF engine conducted in 1967.

In the breadboard engine test program the nuclear rocket engine was operated as a self-contained, self-starting system in all the modes and operating regions required of a versatile propulsion system. It was started and brought to power ten times, and proved to be stable over an unusually broad range of operating conditions.

The XECF engine test series was conducted to verify that the engine test stand (Engine Test Stand No. 1) was ready for hot engine testing, and to investigate engine startup in the test stand under simulated altitude conditions. Other objectives included the checking of operating procedures that had not been demonstrated in previous tests and the investigation of engine malfunctions under simulated altitude conditions. The engine system and test facility operated as planned, and the test results provided the basis for proceeding with the "hot" testing of ground-experimental engines in the fall of 1968. The objectives established for these engine tests are to examine further engine startup characteristics, gain data on system interactions and control at altitude conditions and to provide a variety of engine system data to supplement those data obtained from the breadboard engine and XECF engine tests. An additional and very important aspect of the tests is to gain experience in operating the complex engine test stand system.

NERVA ENGINE DEVELOPMENT PHASE

As a reult of the substantial progress made in the development and demonstration of nuclear rocket propulsion technology, President Johnson requested the Congress in FY 1968 to appropriate funds to initiate the development of a NERVA engine rated approximately at 200,000 pounds of thrust. It had been determined that a single engine of this size could perform all of the missions for which nuclear rockets appeared advantageous--unmanned deep space missions, direct flight manned lunar landing missions, and manned planetary flyby and landing missions.

FY 1968 budgetary constraints, however, forced a reassessment of the future space program. This reassessment necessitated a re-examination of the goals of the nuclear rocket program and led to the decision to reduce the thrust level of the NERVA engine from the 200,000 pounds originally proposed to 75,000 pounds. This reduction in thrust level was made as a result of the NASA decision to defer the manned planetary missions into the distant, undetermined future. The 75,000 pound thrust engine is substantially off-optimum for manned planetary exploration but quite suitable for all other missions for which nuclear rockets are being considered. The lower thrust engine will use hardware of the class used in the technology program and test facilities basically available. Development costs, therefore, will be less than the 200,000 pound thrust engine, particularly over the next several years.

During FY 1968 and FY 1969, preliminary planning in preparation for the development of the 75,000 pound thrust engine was completed and a development schedule was defined which indicated preliminary flight tests based on current NERVA and Phoebus reactor technology could be accomplished in seven to eight years. Work also proceeded on the preliminary design of the NERVA reactor and critical engine hardware, i.e., the nozzle and nozzle hot-bleed port, reactor pressure vessel and turbopump. Concurrently, work was initiated on the preliminary design of modifications required to test the NERVA engine in Engine Test Stand No. 1 at full power and under simulated flight conditions.

AERONAUTICS AND SPACE ENGINEERING BOARD

OF THE

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NATIONAL AERONAUTICS AND SPACE ADMINISTRATION
WASHINGTON, D.C. 20546

TELS. WO 2-4155
WO 3-6925

FOR RELEASE: TUESDAY P.M.
January 9, 1968

RELEASE NO: 68-4

NEW NASA ADVISORY COUNCIL

A new Research and Technology Advisory Council has been created to assist the National Aeronautics and Space Administration in planning and evaluation of research and technology for aeronautics and space.

The council will assess and render judgments on the relative importance of ongoing research, suggest additional work that should be undertaken and advise on the methods for further developing the nation's resources.

The council will be headed by Dr. Raymond L. Bisplinghoff, Head of the Department of Aeronautics and Astronautics of the Massachusetts Institute of Technology. It will be supported by committees representing seven major technology areas and committee members will be leading specialists in these subjects from industry, universities and other government agencies.

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A major function of the council will be to advise the Associate Administrator for Advanced Research and Technology, Dr. Mac C. Adams, on research to fulfill NASA and national objectives.

Members of the council are: Dr. David Altman, United Technology Center, Chairman of the Chemical Rocket Propulsion Committee; Dr. Allen V. Astin, Director, National Bureau of Standards; Dr. Loren D. Carlson, School of Medicine, University of California, Chairman of Biotechnology and Human Research Committee; Dr. Howard W. Emmons, Gordon McKay Professor of Mechanical Engineering, Harvard University; Gen. James Ferguson, Commander, Air Force Systems Command; Dr. Nicholas J. Hoff, Head, Dept. of Aeronautics and Astronautics, Stanford University; Dr. Hans W. Liepmann, Professor of Aeronautics, California Institute of Technology, Chairman of Basic Research Committee.

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Also, Dr. John Linvill, Head, Dept. of Electrical Engineering, Stanford University, Chairman of Electronics Committee; Matthias E. Lukens, Deputy Executive Director, The Port of New York Authority; Dr. Ronald Smelt, Vice President and Chief Scientist, Lockheed Aircraft Corp., Chairman of Space Vehicles Committee; Dr. Chauncey V. Starr, Dean, The Engineering School, University of California, Chairman of Space Power and Electric Propulsion Committee; and Edward C. Wells, Vice President, Product Development, The Boeing Co., Chairman of Aeronautics Committee.

-end-

NMI 1156.1C

July 1, 1968
Effective date

Management Instruction

SUBJECT: RESEARCH AND TECHNOLOGY ADVISORY COUNCIL, COMMITTEES, SUBCOMMITTEES AND PANELS

1. PURPOSE

This Instruction continues in existence the NASA Research and Technology Advisory Council, Committees, Subcommittees and Panels that were established on July 1, 1967, and sets forth their functions and areas of interest.

2. AUTHORITY

a. Section 203(b) (7) of the National Aeronautics and Space Act of 1958 (42 U.S.C. 2473(b) (7)).

b. Executive Order 11007.

c. NMI 1150.2.

3. OBJECTIVES

The general objectives of the NASA Research and Technology Advisory Council, Committees, Subcommittees and Panels are:

a. To provide advice on advanced research and technology goals in aeronautics and space and critical problem areas associated with these activities.

b. To provide an independent means whereby advanced research and technology work in progress and planned can be reviewed and assessed both in depth and from an integrated point of view, considering the relation to other work in the same area as well as the technical value of the results to advanced research and technology goals.

4. ESTABLISHMENT

a. Pursuant to Executive Order 11007 and paragraph 6 of NMI 1150.2, it is determined that the continuation of the NASA Research and Technology Advisory Council, Committees, Subcommittees and Panels listed in Attachment A to provide NASA with technical advice and assistance in the specific areas of scientific interest, as set forth in Attachment B, is in the public interest. Therefore, such Council, Committees, Subcommittees, and Panels are hereby continued in existence.

b. The committees listed in Attachment A are not "industry advisory committees" within the meaning of paragraph 5b of NMI 1150.2.

5. FUNCTIONS

NASA Research and Technology Advisory Council, Committees, Subcommittees and Panels shall be used only for the purposes of consultation and advice. The determination as to any actions to be taken with respect to advice or recommendations that have been rendered will be made by appropriate full-time officers or employees of NASA. Functions of the Council, its Committees, Subcommittees and Panels are prescribed in Attachment B.

6. RELATIONSHIPS

a. The Council reports to the Deputy Administrator, but the Associate Administrator for Advanced Research and Technology is the the principal NASA point-of-contact in detailed dealings with the Council.

b. The Deputy Administrator, with advice from the Associate Administrator for Advanced Research and Technology and other senior officials, will determine NASA actions on recommendations by the Council and will authorize any major assignments NASA may ask the Council to undertake.

c. The flow of technical information from each Subcommittee or Panel will be lateral to the cognizant Branch Chief and upward to the cognizant Committee or Subcommittee.

d. The flow of technical information from the Committee will be lateral to the cognizant Division Director and upward to the Council.

e. The Council will integrate the information flow from the Committees, Subcommittees, and Panels and advise the Associate Administrator for Advanced Research and Technology and the Deputy Administrator thereon.

7. MEMBERSHIP

a. The Associate Administrator for Advanced Research and Technology, with the concurrence of the Deputy Administrator and the Administrator, will appoint the Chairman and members of the Council, which includes the Chairman of the Committees. In addition, he alone will appoint all other members of the Committees and Chairmen and members of the Subcommittees and Panels. Selection of members shall be based upon the individual's professional or scientific competence and recommendations from OART Division Directors. Appointments will be made on a fiscal year basis for terms of one year.

b. The names of the Chairmen, Vice-Chairmen (when designated), Executive Secretaries, and all members of the Council, Committees, Subcommittees and Panels will be set forth in Attachment C each fiscal year.

8. SUBGROUPS

The Chairmen of the Council, Committees, Subcommittees and Panels are authorized to establish, with the approval of the Associate Administrator for Advanced Research and Technology or his designees (who shall be full-time salaried employees of NASA), such ad hoc task forces as they may consider appropriate and necessary to carry out their assigned functions. Members of such task forces will be selected from the existing membership of the Council, Committees, Subcommittees and Panels.

9. AVOIDANCE OF CONFLICTS OF INTEREST

a. Nongovernmental members of the Council, Committees, Subcommittees and Panels will be retained as government consultants or advisers, and will therefore be expected to observe NASA regulations (NHB 1900.2A) pertaining to the avoidance of conflicts of interest and the observance of ethical standards of conduct. A copy of such regulations and related instructions will be furnished to each nongovernmental member at the time of his appointment as a NASA adviser or consultant.

b. Nongovernmental members of the Council, Committees, Subcommittees and Panels must have completed and forwarded to NASA a "NASA Special Government Employees Confidential Statement of Employment and Financial Interests" (NASA Form 1271) prior to participating in any meeting.

10. MEETINGS

a. Meetings will be held at the call of or with the advance approval of the Associate Administrator for Advanced Research and Technology, or his designees, who shall be full-time salaried employees of NASA. The agenda for the Committees and Subcommittees will be formulated or approved by the cognizant OART Division Director. The agenda for the Council will be formulated and approved by the Associate Administrator for Advanced Research and Technology.

b. All meetings will be conducted in the presence of the Executive Secretary of the respective Council, Committee, Subcommittee or Panel, or another designated full-time salaried NASA employee. Such employee will be authorized to adjourn any meeting when he considers adjournment to be in the public interest. When such action is taken, it will be reported to the Administrator.

c. Nonmember attendance at Council meetings by individuals who are not full-time officers or employees of NASA may be permitted with the approval of the Deputy Administrator or the Associate Administrator for Advanced Research and Technology.

d. Records will be kept of each Council, Committee, Subcommittee or Panel meeting which shall, as a minimum, contain:

(1) A record of persons present, and

(2) A description of matters discussed and conclusions reached, recommendations formalized, and copies of all reports received, issued, or approved by the Council, Committee, Subcommittee or Panel. The accuracy of all minutes will be certified by a full-time salaried officer or employee of NASA present during the meeting.

11. RECORDS AND REPORTS

a. The Council will submit its reports and recommendations to the Deputy Administrator.

b. All records and files of the Council, Committees, Subcommittees and Panels including agenda, transcripts or notes of meetings, studies, analyses, reports or other data compilations or working papers, made available to or prepared by or for the Council, Committees, Subcommittees and Panels, will be retained by the cognizant division of the Office of Advanced Research and Technology. These will be made available upon request by the Attorney General or his duly authorized representatives, subject to such security restrictions as may be properly imposed on the materials involved.

12. SUPPORTING SERVICES

Necessary staff and support services for the Council, Committees, Subcommittees and Panels will be provided by the Office of Advanced Research and Technology.

13. DURATION OF COMMITTEES

The Council, Committees, Subcommittees and Panels will be in existence from July 1, 1968 to June 30, 1970, unless sooner terminated, or unless extended pursuant to the provisions of NMI 1150.2.

14. CANCELLATION

NMI 1156.1B, July 1, 1967.

James E. Webb
Administrator

ATTACHMENT:
A. NASA Research & Technology Advisory Council, Committees, Subcommittees and Panels
B. NASA Research and Technology Advisory Council, Committees, Subcommittees and Panels Functions and Typical Areas of Technical Interest.
C. FY 1969 Membership--Research & Technology Advisory Council, Committees, Subcommittees and Panels.

DISTRIBUTION:
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NASA RESEARCH AND TECHNOLOGY ADVISORY COUNCIL, COMMITTEES, SUBCOMMITTEES AND PANELS

Research and Technology Advisory Council

- Committee on Aeronautics
  - Subcommittee on Aircraft Aerodynamics
  - Subcommittee on Aircraft Structures
    - Panel on Fatigue
  - Subcommittee on Airbreathing Propulsion
  - Subcommittee on Aircraft Operating Problems
  - Subcommittee on Aircraft Flight Dynamics
- Committee on Space Vehicles
- Committee on Chemical Rocket Propulsion
- Committee on Space Power and Electric Propulsion
- Committee on Biotechnology
- Committee on Electronics
  - Subcommittee on Control, Guidance and Navigation
  - Subcommittee on Communication and Tracking
  - Subcommittee on Instrumentation and Data Processing
- Committee on Basic Research
  - Subcommittee on Materials
    - Panel on Materials for Aircraft Engines
  - Subcommittee on Fluid Mechanics
  - Subcommittee on Electrophysics

NASA RESEARCH AND TECHNOLOGY ADVISORY COUNCIL, COMMITTEES, SUBCOMMITTEES AND PANELS
FUNCTIONS AND TYPICAL AREAS OF TECHNICAL INTEREST

Research and Technology Advisory Council

Makes recommendations and provides NASA with an integrated technical input on program goals, trends, content, scope, and technical balance in aeronautical and space research and technology.

Reviews, integrates, assesses, and balances the technical inputs and recommendations of the Advisory Committees.

Committee on Aeronautics

Reports its recommendations to the Council and provides the Director, Aeronautics Division, with a technical input on program goals, trends, content, scope, and technical balance in aeronautics.

Reviews, integrates, assesses, and balances the technical inputs and recommendations of the Aeronautics Subcommittees.

Subcommittee on Aircraft Aerodynamics

Reports its recommendations to the Committee on Aeronautics and provides the Chief, Aerodynamics Branch, with technical input in relevant areas of interest including:

- Aerodynamic and overall flight efficiency of aeronautical vehicles.
- Aerodynamic stability and control of aeronautical vehicles.
- Aerodynamic heating of aeronautical vehicles.
- Aerodynamic flow fields, shock wave generation and propagation, boundary-layer development and transition, shock wave boundary layer interactions, separation, heat transfer and skin friction of aeronautical vehicles.
- Aerodynamic problems related to aircraft-propulsion system dynamics, particularly the interaction of inlet-engine-nozzle, and aircraft-engine control systems.
- Aerodynamic problems peculiar to the low-speed operation of aeronautical vehicles including those of high-speed aircraft, vertical and short takeoff and landing aircraft (VTOL/STOL), and rotary-wing aircraft.
- The need for advanced aerodynamic vehicle ground-based research facilities and evaluation of the technology necessary for their timely construction.
- The need for aerodynamic flight research to obtain critical information not otherwise obtainable and to validate or extend the results of ground-based research on aeronautical vehicles.

Subcommittee on Aircraft Operating Problems

Reports its recommendations to the Committee on Aeronautics and provides the Chief, Operating Environment and System Dynamics Branch, with technical input in relevant areas of interest including:

- Characteristics and operational effects of atmospheric variables such as composition, electrical and magnetic phenomena, winds, weather, temperature, density, and pressure.
- Flight safety and operational factors, such as ditching, thrust reversal, instrumentation, altimetry, pilot procedures and techniques, collisions and operating statistics.
- Crew and vehicle protection techniques for efficient operation and survival including escape, restraint, fire, and explosion.
- Crew performance problems as imposed by the vehicle and the internal and external environments such as acceleration, thermal stress, noise, vibration, and task saturation.
- Boundary-layer and power-plant noise measurements, mechanisms and characteristics, including generation, propagation, and alleviation.
- Problems related to the need for supporting facilities and an evaluation of the technology necessary for their timely construction.

Subcommittee on Aircraft Structures

Reports its recommendations to the Committee on Aeronautics and provides the Chief, Loads and Structures Branch, with technical input in relevant areas of interest including:

- Structure environment and design conditions of aircraft, including evaluation of missions and operations, critical ground and flight loads and load distributions, and thermal loads.
- Characteristics of aircraft structures, including strength, stiffness, vibration modes and frequencies, thermal resistance, and structural efficiency.
- Structural behavior and responses, including failure modes such as fatigue, aerothermoelastic and instability phenomena and forced vibrations.
- Safety and reliability as determined by combined effects of environment, structural characteristics and responses.
- Problems related to the need for supporting facilities, and an evaluation of the technology necessary for their timely construction.

Panel on Fatigue

Reports its recommendations to the Subcommittee on Aircraft Structures and provides the Chief, Loads and Structures Branch, with technical input in relevant areas of interest including:

- Research on the mechanism of fatigue, including environmental materials and structural aspects.
- Design aspects of fatigue including safe-life, fail-safe considerations and economic limitations of potential design principles.
- Design criteria or minimum requirements for ensuring satisfactory probability of structural survival.

Subcommittee on Aircraft Flight Dynamics

Reports its recommendations to the Committee on Aeronautics and provides the Chief, Operating Environment and System Dynamics Branch, with technical input in relevant areas of interest including:

- Handling qualities of conventional, V/STOL, and rotary-wing aircraft.
- Analysis, simulation, and flight test techniques relating to flight dynamics investigations.
- Methods of achieving attitude and flight path control for manned aeronautical vehicles.
- Dynamic interaction of stability augmentation, artificial feel, flight path command and display systems on aircraft flight dynamics.
- Problems related to the need for supporting facilities and an evaluation of the technology necessary for their timely construction.

Subcommittee on Airbreathing Propulsion

Reports its recommendations to the Committee on Aeronautics and provides the Chief, Propulsion Branch, with technical input in relevant areas of interest including:

- Performance, reliability, durability, safety, and economics of operation of airbreathing propulsion systems for manned and unmanned flight vehicles at subsonic, transonic, supersonic and hypersonic speeds for military and/or commercial applications.
- Research on complete engines and engine components, including inlets, fans, compressors, combustors, turbines, augmentors, exhaust nozzles and thrust deflection systems, controls, propellers, power transfer systems, auxiliary power systems, seals, bearings, thrust reversers, and engine noise suppressors.
- Aircraft propulsion system dynamics, particularly the interaction of inlet-engine-nozzle and aircraft-engine control systems.
- Engine materials, fuels, lubricants, and hydraulic fluids.
- Mechanisms of engine noise generation and alleviation techniques.
- Problems related to the need for supporting test facilities and an evaluation of the technology necessary for their timely construction.

Committee on Space Vehicles

Reports its recommendations to the Council and provides the Director, Space Vehicle Research and Technology Division, with a technical input on program goals, trends, content, scope, and technical balance in all phases of space flight including launch; operation in space; operation in regions of the moon and planets including to, on, and from their surfaces; reentry flight into the earth's atmosphere; and post-reentry flight within the earth's atmosphere including landing and recovery. The vehicle research and technology programs concern both launch vehicles and spacecraft, and concern primarily the broad technology areas of space environmental factors, structures, and aerodynamics. Some specific technology areas of concern are:

- The meteoroid environment in space and research on meteoroid impact phenomena and concepts of space vehicle design to protect against the meteoroid hazard.
- Fluid slosh dynamics and damping during powered flight, fluid behavior and control in very low gravity space flight environment, and cryogenic containment for both short- and long-term space flight.
- Launch vehicle and spacecraft configuration concepts and performance including static and dynamic stability (lift, drag, moments) and associated structural loads and response.
- Structural dynamics of space vehicle configurations and unsteady aerodynamic forces or pressures including bending and vibrational modes, acoustic pressures, [illegible]ting, flutter, and other aeroelastic phenomena.
- Space radiation effects on space vehicles and radiation shielding.
- Thermal vacuum effects and temperature control of vehicles in space.
- Reentry heating and heat protection systems.
- Launch vehicle base heating and heat protection.
- Flight operation, performance, and structural characteristics of landing and recovery systems including elements such as parachute and impact alleviation devices.
- Problems related to the need for supporting facilities and an evaluation of the technology necessary for their timely construction.

Committee on Chemical Rocket Propulsion

Reports its recommendations to the Council and provides the Director, Chemical Propulsion Division, with a technical input on program goals, trends, content, scope, and technical balance in all types of chemical rocket propulsion. These include liquid, solid, and hybrid chemical reactants in a wide range of propulsion systems for launch vehicles and spacecraft. Some specific technology areas of concern are:

- Fluid mechanics of propellants from tanks to the combustion chamber.
- Materials, insulation, heat transfer, and cooling.
- Controls including thrust vector control, variable thrust, multiple start and stop, etc.
- Engine components including turbopumps, valves, thrust chambers, motor cases, nozzles, etc.
- Engine systems including control and dynamic responses.
- Gas dynamics during combustion and expansion.
- Ignition and combustion.
- Propellants including properties, manufacturing, handling, and storage, hazards, etc.
- Durability and reliability.
- Problems related to the need for supporting facilities and an evaluation of the technology necessary for their timely construction.

Committee on Space Power & Electric Propulsion

Reports its recommendations to the Council and provides the Director, Space Power and Electric Propulsion Division, with a technical input on program goals, trends, content, scope, and technical balance in all types of space power systems and electric propulsion systems. Some specific areas of interest are:

- Conversion of solar energy to electrical energy through solar cells, thermionic diodes, or heat cycle engines.
- Conversion of chemical energy to electrical energy through batteries, fuel cells, and heat engines.
- Conversion of nuclear energy to electrical energy by means of nuclear and isotope reactors, thermionic diodes, and heat engines.
- Electric control and distribution systems.
- Complete electric power systems.
- Electric propulsion by plasma, electromagnetic, electrostatic, and thermal techniques.
- Complete electric propulsion systems including electric power components.
- Problems related to the need for supporting facilities and an evaluation of the technology for their timely construction.

Committee on Biotechnology

Reports its recommendations to the Council and provides the Director, Biotechnology and Human Research Division, with a technical input on program goals, trends, content, scope, and technical balance in biotechnology and human factors in aeronautics and space. Specific areas of interest include:

- Human research to obtain a better understanding of man and to determine his best utilization in advanced aerospace systems and methods of assessing the status of homeostasis.
- Biotechnology of life support and man-machine information systems including control, and protective systems which guarantee an environment adequate to maintain maximum operating efficiency during advanced aerospace missions.
- Human engineering for man-system integration into the design of advanced aeronautical and space systems, extravehicular technology, and man-machine information systems.
- Problems related to the need for supporting facilities and an evaluation of the technology necessary for their timely construction.

Committee on Electronics

Reports its recommendations to the Council and provides the Director, Electronics and Control Division, with a technical input on the program goals, trends, content, scope, and technical balance in electronics for aeronautics and space including guidance, control, navigation, communication, tracking, instrumentation, and data processing.

Reviews, integrates, assesses, and balances the technical input and recommendations of the Electronics Subcommittees.

Subcommittee on Control, Guidance and Navigation

Reports its recommendations to the Committee on Electronics and provides the Chiefs, Control and Stability Branch, and Guidance and Navigation Branch, with technical input in relevant areas of interest in aeronautics and space such as:

- Techniques and systems for guidance, navigation, and flight path control of manned and unmanned vehicles.
- Techniques and systems for attitude control and stabilization of manned and unmanned flight vehicles.
- Integration of manned and unmanned guidance and control systems with flight vehicles.
- Advanced control and guidance theory and trajectory analysis.
- Components related to control, guidance, and navigation systems.
- Problems related to the need for supporting facilities and an evaluation of the technology necessary for their timely construction.

Subcommittee on Communication and Tracking

Reports its recommendations to the Committee on Electronics and provides the Chief, Communications and Tracking Branch, with technical input in relevant areas of interest in aeronautics and space such as:

- Radio and optical communication and tracking.
- Information theory and other disciplines related to communication and tracking.
- Optical technology for space applications.
- Problems related to the need for supporting facilities and an evaluation of the technology for their timely construction.

Subcommittee on Instrumentation and Data Processing

Reports its recommendations to the Committee on Electronics and provides the Chief, Instrumentation and Data Processing Branch, with technical input in relevant areas of interest in aeronautics and space such as:

- Instrumentation techniques and devices for detection and measurement of environmental, engineering, and bioelectronic parameters.
- Aerospace data processing and computing techniques and devices.
- Problems related to the need for supporting facilities and an evaluation of the technology necessary for their timely construction.

Committee on Basic Research

Reports its recommendations to the Council and provides the Director, Research Division, with a technical input on program goals, trends, content, scope, and technical balance in the physical disciplines of fluid physics, electrophysics, materials, and applied mathematics as relevant to the advancement of aeronautics and space projects.

Reviews, integrates, assesses, and balances the technical input and recommendations of the Basic Research Subcommittees.

Subcommittee on Materials

Reports its recommendations to the Committee on Basic Research and provides the Chief, Materials Engineering Branch, with technical input in relevant areas of interest including:

- Physics of solids, and fundamental metallurgy, chemistry, and ceramics, in support of development of new materials and improved materials for aircraft and spacecraft structures, power plants, electronic and magnetic devices, and auxiliary equipment.
- Characterization of structural and power plant materials with emphasis on properties under conditions that will be encountered by aircraft and spacecraft.
- Bearings, lubricants, and seals.
- Problems related to the need for supporting facilities, and an evaluation of the technology necessary for their timely construction.

Panel on Materials for Aircraft Engines

Reports its recommendations to the Subcommittee on Materials and provides the Chief, Materials Engineering Branch, with technical input in relevant areas of interest including:

- Definition of requirements of materials for advanced aircraft engines.
- Identification of present and future requirements for engine materials that are not satisfied or will not be satisfied by available materials.
- Recommendation of research and development that is required to have materials that will meet these requirements.

Subcommittee on Electrophysics

Reports its recommendations to the Committee on Basic Research and provides the Chief, Electrophysics Branch, with technical input in relevant areas of interest including:

- Atomic and molecular behavior in pulsed and steady gaseous discharges such as corona, glow, spark and arc, as affected by induced and applied static and dynamic electric and magnetic fields.
- The generation, modulation, detection, and amplification of electromagnetic wave energy in the range from gamma rays to the millimeter wave-lengths by stimulated emission and the propagation of such electromagnetic waves through liquids, solids, and gases.
- The behavior of charged and uncharged particles due to internal and external static and dynamic electric and magnetic fields, nuclear fields, and other short- and long-range force fields.
- Theoretical physics research concerned with electrodynamics, gravity and relativity, wave coherence, magnetism and superconductivity, nuclear fields, and structure and other topics in classical and quantum physics.
- Problems related to the need for supporting facilities and an evaluation of the technology necessary for their timely construction.

Subcommittee on Fluid Mechanics

Reports its recommendations to the Committee on Basic Research and provides the Chief, Fluid Dynamics Branch, with technical input in relevant areas of interest including:

- Composition, properties and chemical kinetics of fluids, including atmospheric gases, combustion products, and propellant fluids in the liquid and gas phase.
- Magnetogasdynamics requiring research which involves the flow of electrically conducting fluids under the influence of electric and magnetic fields. Also, research in plasma physics, including the generation, the dynamics, and the diagnostics of plasmas.
- Flow of gases at high energies and Mach numbers corresponding to entry into the atmospheres of earth and other planets including the study of physical processes, chemical reactions, flow configurations, radiative heating, and highly rarefied gases.
- Boundary layer phenomena, including transition from laminar to turbulent flow, skin friction, mass transfer, and heat transfer, interaction of boundary layer with shock waves and separated flows.
- Internal fluid mechanics including two-phase flow, flow of cryogenic and non-Newtonian fluids. Study of real gas nozzle flows, detonation, and combustion processes.
- Problems related to the need for supporting facilities, and an evaluation of the technology necessary for their timely construction.

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