NASA SP-213

# A LONG-RANGE PROGRAM IN SPACE ASTRONOMY

## Position Paper of the Astronomy Missions Board

July 1969

NATIONAL AERONAUTICS AND SPACE ADMINISTRATION

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# PREFACE

The Astronomy Missions Board was established by the National Aeronautics and Space Administration by charter in September 1967 to assist in an advisory capacity in the planning and conduct of all NASA missions to create and operate astronomical experiments in space. The scope of the Board's activities includes: development and review of the scientific objectives and general strategy for space astronomy and associated ground-based astronomy; the formulation of guidelines and specific recommendations for the design of space astronomy missions, and for the various experiments and auxiliary equipment to be developed and used on these missions; the continuing examination of policies relating to the operation of these space observatories once they have been made operational and are available for observations by the scientific community. The work of the Board encompasses the many aspects of space astronomy including direct observations of electromagnetic radiation from astronomical sources, cosmic-ray particles and the supporting research that is necessary, but its scope does not include the study of the Moon and planets from close vantage point or study of the Earth.

The Astronomy Missions Board is presently composed of 18 members of the scientific community with a wide diversity of interests and experience. They are drawn largely from universities, but include members from national laboratories (see appendix for a list of members of the Board and its panels). The Board's activities are supported and supplemented by seven panels and two ad hoc working groups to whom specific areas of responsibility are assigned. The panel compositions are similar to that of the Board itself and involve an additional 31 scientists. This wide membership provides a broad representation of current thought in space astronomy both directly through its membership and from the wider astronomical community by means of letters and discussions.

The activity of the Board has been intensive. With few exceptions, it has met monthly for 2 days at locations appropriate to its current activities. In addition to extensive deliberations and discussions, the meetings have included reports and résumés from NASA personnel about matters such as the current status of projects then underway, present NASA plans for the future, technical reports on areas of special relevance, and budgetary aspects of current and planned programs. The panels have met several times during the past year and have taken the opportunities for obtaining firsthand information about the activities in space astronomy at various NASA centers relevant to their particular fields of interest. Again, briefings as to technical capabilities and current planning were obtained and the panels prepared detailed programs and recommendations for activities in their areas.

An important continuing activity of the Board is the presentation of specific recommendations to the Associate Administrator of NASA. Many of these recommendations have been ad hoc answers to questions raised by NASA, while others have been of a more general nature and have, in most cases, been incorporated into the body of this report. Many of these ad hoc recommendations were for the purpose of assisting NASA to optimize a low-level program, and should not be construed as approval of such a program by the Board or the scientific community.

The Board has created a long-range national program for space astronomy—including discussions of the major problems of astronomy and astrophysics, an observing program describing the next important measurements from space, and examples of the instruments, spacecraft, and missions needed to make those measurements. Specific mission descriptions are not intended as concrete definitions of future missions, but as part of an exemplary program which is used to establish the best current balance between the subdisciplines. The plan contains sufficient mission priorities and interdependences on which to base AMB advice to NASA at various foreseeable levels of effort, and should enable NASA management to assess the impact on scientific progress of the various future options available to them. The purpose of this position paper is to describe the long-range plan as it appears in July 1969.

Past experience has shown that astronomy is a field full of surprises and the unexpected, and it would be extremely shortsighted to expect this report to remain up to date for very long. This report is not intended to be a static document. It is, rather, a working paper to be updated and altered continuously by the Board as technical capabilities change and scientific opportunities and priorities evolve. Nevertheless, it seems appropriate to publish this version of the position paper, just as it was submitted to NASA as part of the fiscal year 1971 budget planning cycle, in order to acquaint a wide community of astronomers, astrophysicists, physicists, and other interested scientists with the workings of the Astronomy Missions Board, as well as with the national space astronomy program. NASA and the Astronomy Missions Board hope in this way to continue to improve the mechanisms by which the NASA space astronomy program can get the best assistance from, and give the most help to, the entire community of astronomers and space physicists. From time to time, as the extent of the revisions makes a major part of this work obsolete, the Board will again publish an updated position paper.

The detailed reports on the subdisciplines of space astronomy, authored by the panels and endorsed in substance by the Board, will be found in Part II. Part III describes how the panels' programs were evaluated, and how parts of them were combined into long-range plans at two levels of effort—a minimum balanced program and an optimum program—both of which do not attempt simply to do everything suggested by the subdisciplines, but rather emphasize research on those problems judged astrophysically most important by the greatest consensus of the Board.

A summary of the position paper and key features of the long-range plan will be found in Part VII.

# FOREWORD

The Astronomy Missions Board advises the National Aeronautics and Space Administration (NASA) through the Associate Administrator, Dr. Homer E. Newell, on the present and future of the national space astronomy program. The Board has developed a position paper which recommends to NASA an integrated space astronomy program for the Seventies. The position paper was received by NASA on July 5, 1969. Because of current widespread interest this paper is being published in its original form without any evaluation or comment by NASA beyond this statement. While NASA will be guided by the recommendations in this paper, publication of this document by NASA is in no sense either an endorsement of its contents or a commitment on the part of NASA to undertake to carry out all or any part of the proposed astronomy program.

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# I

# THE UNIQUE CONTRIBUTION OF SPACE RESEARCH TO THE MAJOR PROBLEMS OF ASTRONOMY AND ASTROPHYSICS

# INTRODUCTION

The ability to mount astronomical instruments on observing platforms high above the Earth's atmosphere is the latest and most decisive in a series of technological developments that have stimulated the recent fantastic growth of astronomy. Astronomers are now able to observe the sky in ways they could only dream of less than 30 years ago. In those days, the known spectrum of radiation reaching the Earth's surface was only slightly broader in wavelength than the visible spectrum, 4000–7000 Å, because the Earth's atmosphere blocks out most of the shortwave radiation emitted by the Sun and the stars, and a good part of the infrared as well. Moreover, there was little reason to suspect that any significant number of astronomical bodies were radio emitters, and Jansky's early apparent observations of radio waves from the Milky Way aroused little interest among astronomers.

With observations restricted to a small fraction of the electromagnetic spectrum, a large part of astronomical research was concerned with the collection of data that could not be well understood. Astronomers could hope to deduce the physical nature only of objects emitting purely thermal radiation at a temperature of a few thousand degrees so that most of their radiated energy was radiated in the form of visible light. Very hot or very cold objects or those that gave off exotic, nonthermal radiation were beyond understanding. Then, one by one, the other regions of the spectrum began to be exposed. First the cosmic rays provided the first clue to high-energy processes in the universe. Next radio astronomy came to fruition, revealing both high-energy and nonthermal phenomena on a grand scale, notably the quasars and pulsars. Finally, the space program is bringing into view the ultraviolet region of the spectrum between 3000 and 300 Å, and X-rays and gamma rays at still shorter wavelengths.

No longer may theories be proposed that cannot be tested because key parts of the spectrum are inaccessible. On the contrary, it is now possible to devise and carry out decisive experiments to test almost any hypothesis in astrophysics, which is one of the reasons why more and more physicists now look upon astrophysics as the most interesting and exciting branch of physics.

Space astronomy offers two kinds of challenges. First, a great many well-defined scientific problems can now be solved by multiwavelength experimental approaches, and second, many unexpected discoveries are sure to be made as they always are when a new region of the spectrum is first explored or when a new instrument of unprecedented power is put into operation. The recent history of astronomy is full of examples of such unexpected discoveries. For example, the first radio and X-ray sources were both discovered accidentally and many of the recent discoveries of strong emitters of infrared radiation could not have been predicted in advance.

Telescopes in space have other important advantages beyond their capacity to intercept radiation that cannot penetrate the atmosphere. Thus, Earth-based telescopes must look through columns of turbulent air which severely degrade the images they produce. A telescope of 120-inch aperture above the Earth's atmosphere has 10 times the resolving power of the 200-inch telescope on Mount Palomar operating under the best atmospheric conditions. Because of the very small image sizes that are possible with telescopes in orbiting observatories, a space telescope 120 inches in diameter should be able to detect stars 100 times fainter than the faintest detectable from the Earth. Data on such faint objects are critical for settling major questions in cosmology, such as whether the universe is infinite or not.

To fully appreciate the unique contribution of space research in reaching otherwise inaccessible information about our universe, we should survey the great problems before us in modern astronomy, and determine for each case just what observations ought to be made next. This is how each of the reports of the subdisciplines (Part II) begins. Among the problems they examine are:

> The quasars and the violent explosive events in the nuclei of galaxies which share many properties with those most distant objects known to man;
>
> the strange problem of the million-degree temperatures in the solar corona surrounding the—astronomically speaking—cool surface of the Sun (a few thousand degrees) when, as every schoolboy knows, heat flows from hot to cold places and not the other way around;
>
> the possibility that astronomers may be witnessing in some clouds of dust surrounding a certain star the formation of a system of planets like our own;
>
> the mysterious pulsars, whose unnatural sounding rhythm led the first astronomers detecting them to catalog them as LGM 1, LGM 2, etc., where the LGM stood for "little green men";

the puzzling situation in the interstellar medium where OH molecules (two-thirds of a water molecule), at temperatures hundreds of degrees below freezing (on Earth), are sending us brilliant maser beams of radiation, brighter than the radiation from any normal thermal source with a temperature of trillions of degrees; and

the most recent discovery of organic molecules existing in cold interstellar space—perhaps the simplest building blocks of life were not formed in early geological evolution but were created as part of the same process that formed the stars.

We conclude here with brief discussions of two challenging problems in modern astrophysics, and especially note the interplay between new techniques in widely separate subdisciplines.

One of these problems, the microwave background, is concerned with beginnings, with the cosmological question of the ultimate origin and fundamental forces that govern the evolution of the universe. The second example, the Crab Nebula, is related to endings, to the termination of the life of a star which apparently dies—contrary to T. S. Eliot's despairing poetic prediction—with a bang, not a whimper.

## THE CRAB NEBULA: SUBJECT OF ALL DISCIPLINES

The Crab Nebula is now recognized as one of the most remarkable objects in the entire sky, combining the attentions of nearly every modern astronomical discipline, both spaceborne and ground based, observational and theoretical. The scribes of the Sung dynasty described it as a "guest star" that appeared on July 4, 1054, and in recent times the combined efforts of astronomers and scholars of Chinese history finally showed that the nebulosity known as M1 (object no. 1 in Messier's catalog of 1742) was indeed the remnant of the stellar explosion of 1054. The event was no ordinary nova (for "new" star) outburst, but an example of the much rarer supernova explosion, only three of which have been observed in our galaxy in the last thousand years. As a star exhausts its nuclear fuel, it cannot keep from collapsing under its own gravity. A complex series of events, whose details are still not completely understood, leads to a rapid collapse, followed by a violent explosion, during which the supernova releases more energy in 1 year—$10^{52}$ ergs—than it had given off in its entire lifetime as a star. Theorists have predicted for many years that the residue might include a neutron star—a star so compressed that atoms lose their individuality, their nuclei and electrons merge, and the resulting state is best described as continuous nuclear matter. These neutron stars would be only 10 km or so in diameter, but would be extremely dense, with 1 cc weighing a billion tons or so. This corresponds approximately to the entire material of Manhattan Island, rock, buildings, and all, compressed into the volume of a thimble.

The combined efforts of radio, optical, and X-ray astronomers over the past 20 years have shown that the remnants of this incredible energetic explosion provide fascinating new ideas of the complexity of our universe. The optical astronomer sees a faintly glowing nebulosity, interlaced by a delicate network of red filaments that are expanding at a rate of 1000 km per second. In the center is a faint star, unlike any known stellar types. The light from the continuous part of the nebulosity was found to be strongly polarized, and by combining this information with the intense radiation seen by the radio astronomers, it became clear that the nebulosity is not just a glowing mass of hot gas, but a relativistic gas, composed of electrons whose energies exceeded 100 billion electron volts (and with velocities near the speed of light, the relativistic limit), an energy greater than that produced by any accelerator in operation on the Earth. The electrons emit radiation by the process known as synchrotron emission (like that given off by synchrotron accelerators), which requires that the relativistic gas be permeated by a magnetic field weak by terrestrial standards, but fabulously energetic when it extends over a region light-years in extent.

The advent of space technology brought forth the new field of X-ray astronomy, and after the initial discovery of Scorpio XR–1, the first X-ray star, the next object to be observed clearly as an X-ray emitter was the Crab Nebula. The X-rays might be generated by a "cool" gas at a temperature of a million degrees Kelvin—a substrate mingled with the relativistic gas—or by the relativistic electrons themselves. The evidence now favors the latter choice, but this implies many more energetic electrons, and at energies of 10 000 billion to a hundred thousand billion electron volts, which radiate so strongly that a new supply must be furnished continuously.

New surprises were in store for the astronomers, however, when an entirely new class of object—the pulsar—was discovered by radio astronomers two years ago. The first of these remarkable objects was observed to emit sharp radio pulses every second or so, with clocklike regularity. One of the suggested mechanisms that would explain the observations invoked the rotation of a neutron star as the means of controlling the pulse rate, although the means of producing the radio pulses themselves remained unclear.

Within a year, a pulsar was discovered within the Crab Nebula, and in many respects the Crab Pulsar has proven to be the most promising key to the puzzle. Its repetition rate is 30 times per second—much faster than the typical pulsar—and the rapid rate can best be understood, it appears, by assuming that we are indeed observing a rapidly rotating neutron star. The optical astronomers quickly discovered that the peculiar star at the center of the Crab was indeed the pulsar, flashing in the visible as well as in the radio part of the spectrum. Space astronomy has now further extended the observations, for the X-ray astronomers have shown that approximately five percent of the X-rays from the Crab are pulsed in synchronism with the light and radio pulses.

The space observations have special significance because of the need for powerful energy sources to explain the source of X-ray emission. A hundred times more energy is emitted in the X-ray spectrum than in the visible, and the total rate of energy radiated by the Crab Pulsar must be over 100 times the rate of energy radiated by the Sun.

The combined X-ray, radio, and optical observations all support the model of the Crab Nebula being energized by a rotating neutron star. According to one theory, the rotation of the neutron star at 30 times per second results from the collapse of the original star, and the conservation of angular momentum. The collapse also compressed the normal magnetic field previously present in the star, to a value of a trillion gauss. Calculation then indicates that the huge electric field induced by the rapidly rotating magnetized star accelerates particles that drag the magnetic field with them, emitting synchrotron radiation as they go. In this way one accounts for the magnetic fields and relativistic particles observed in the nebula. Moreover, the process extracts energy and angular momentum from the star at a calculable rate, which agrees with the observation that the Crab Pulsar period is lengthening on a time scale of a thousand years.

Thus the discovery of a neutron star at the center of the Crab may solve several problems concerned with the energization of the nebula itself. More important, perhaps, it demonstrates the close relationship between the formation of neutron star and the explosion of a supernova. It will be of great interest to see whether all supernovae produce a pulsar (neutron star) to play a clocklike dirge during the final stages of evolution—the magnificent death throes of a star, or whether some lead to a relativistic collapse to a gravitational singularity, a lump of matter so highly condensed that no radiation of any kind (and hence no information) can escape from its gravitational pull—"black holes" of the universe into which things may enter, but from which nothing ever returns.

## THE COSMIC MICROWAVE BACKGROUND

The abundance of the chemical elements, and an unexplained discrepancy in the performance of a satellite communication system, unexpectedly prove to be closely related to the deep question of the origin and evolution of the universe. George Gamow and his coworkers showed theoretically 20 years ago that if the universe began as an initially compact mass of hot matter (the "big bang" theory of cosmology), they could explain some of the observed element abundances. A further consequence of the theory, not entirely appreciated at the time, was the lingering effect of the blast of gamma rays that would have been present in the initial fireball.

After the elements stopped forming, at a time when the universe would have been about 3 days old in the simplest cosmological model, the radiation began to degrade in energy, and should still be observable today. Gamow's colleagues estimated in 1949 that the cosmological red shift would have transformed the gamma rays in energy all the way through the electromagnetic spectrum to radiofrequencies, and the sky should today appear to have a uniform brightness of a "blackbody radiator" at a temperature of about five degrees Kelvin (°K).

Four years ago two radio astronomers, Penzias and Wilson, showed that a previously discarded discrepancy in the observed noise from a very-high-sensitivity receiving system at the Bell Laboratories was indeed real and was observable no matter what direction in the sky they pointed their antenna. The apparent brightness of the sky at 7-centimeter (microwave) wavelength, when all corrections for the Earth, the atmosphere, and the galaxy had been applied, was measured to be about 3.5° K. Since then, measurements at many wavelengths between 3 mm and 70 cm have established that the spectrum is very accurately that of a blackbody radiating at a temperature of 2.7° K, and measurements in many directions confirm that it is a true isotopic cosmic background. Thus, the prediction of Gamow's theory was verified, and the range of acceptable cosmological models was greatly reduced. The steady-state theory, which envisioned a universe whose appearance never changes, with new galaxies constantly forming from new matter in the void left by the expanding system of old galaxies, cannot explain the new observation without additional hypotheses, such as a new class of radio sources far exceeding the number of galaxies. The radio evidence at present surely favors the big bang theory, but cosmological problems are notoriously slippery, and new observations are certainly needed to cross-check the new ideas.

If this cosmic microwave background radiation is a true "blackbody" at 2.7° K, it should have its peak intensity at a wavelength of one millimeter and fall off rapidly at shorter wavelengths. Unfortunately, the Earth's atmosphere is opaque at this and shorter wavelengths, so it is difficult to verify the predicted decrease in intensity by ground-based measurements. Upper limits have been deduced at several wavelengths near one millimeter by observations of the state of excitation of interstellar molecules, and these suggest a fall in intensity, but the method is indirect. Several groups are therefore undertaking direct observations at millimeter wavelengths from balloons or rockets. One preliminary rocket observation had indicated that, to the contrary, there is a component of radiation at one millimeter which is almost 100 times stronger than a 2.7° K blackbody. If the observations of interstellar molecules are correct, the radiation observed by rockets must be locally produced.

The question of the ultimate origin and grand scheme of the universe is so important that one should be driven by only the utmost decisive evidence to acceptance of a particular cosmological model. The evidence for the "big bang" is mounting, but the final evidence is not in hand. Only through a combination of all the threads of observational evidence can the scientist be convinced. The choice of a particular theory for the origin of our universe has tremendous implications for many other astrophysical problems at the frontier of study, such as the nature and distribution of quasars, the origins, distribution, and dynamics of the high-energy cosmic rays, the abundance of the elements, and in particular the relative amounts of different isotopes. Those abundances must be determined more accurately in a variety of different astronomical objects. To be sure, our present best measurements of the helium abundance in the oldest stars support the cosmologies that are consistent with the cosmic microwave background, but the abundance determinations, in many instances, are still subject to large uncertainties.

Moreover, because of its great energy density, which exceeds that of other known sources, the microwave background may have had profound effects on the evolution of galaxies. For example, density fluctuations which would otherwise have collapsed gravitationally to form galaxies may have been prevented from doing so by radiation pressure, and relativistic electrons ejected by distant quasars are rapidly decelerated by Compton collisions with microwave background photons. Examples such as these emphasize the importance of this new cosmological phenomenon.

The quest for the solution of the basic cosmological problem remains a difficult one, but the discovery of the cosmic microwave background has added new impetus and excitement. It permits us to look directly at radiation created when the universe was just a few days old, and allows us to surmise conditions in this most early history of the universe.

# II

# REPORTS ON THE SUBDISCIPLINES OF ASTRONOMY

## THE CHARGE TO THE PANELS

In June 1968, at its seventh meeting, the Astronomy Missions Board (AMB) charged each of its subdiscipline panels with the responsibility of preparing a logical, orderly, long-range space astronomy program for the subdiscipline. This chapter contains the most recent drafts (some of these reports have been revised several times) of each subdiscipline report, in each case an in-depth analysis of the current status and future needs of the subdiscipline. The Board suggested that all reports have a certain overall structure and development, and that taken together they achieve a certain completeness. The wide range in working styles of the panels and especially the panel chairmen, and the enormous differences in the state of science and technology in these frontier subjects, may occasionally mask the underlying structure, but each report contains essentially the following elements:

(1) A discussion of what appears to the panel to be the most exciting problems in astronomy and astrophysics.

(2) An estimate of the space observations, and complementary ground-based observations, needed to make progress on the solution of the major problems over the next 10 years, or as far as can be predicted with reasonable assumptions.

(3) A determination of the types and numbers of instruments needed to carry out this long-range observing program, assuming reasonable advances in the state of technology. Many of the panels had specific briefings by NASA representatives on the Supporting Research and Technology and Advanced Research and Technology Programs. Each panel was urged to identify any critical requirements for specific instrumentation needing technological development, or theoretical research which should be started now in order to make future missions possible.

NASA planners requested that the panels define their subdiscipline objectives primarily in terms of the observing programs, i.e., the acquisition of certain kinds of astrophysical data of a given quality and quantity, and at a specified rate, and leave to NASA the problem of defining hardware requirements (spacecraft, number of missions, etc.). However, the Board subsequently requested the panels to use their experience with the space program to translate these observation and instrument requirements into spacecraft needs—by grouping required instruments into exemplary compatible payloads—and finally into mission schedules—which could be interpreted as implicit priority assignments (timing, level of effort, etc., all included) for the various space experiments. The primary purpose of these flight schedules was to allow the Board to have a common set of inputs from each panel on which to base the Board's own assignment of priorities between the subdisciplines, as described in part III. In most cases these mission schedules were subsequently incorporated in revisions of the panel reports.

## THE ORGANIZATIONAL STRUCTURE OF THE PANELS

A deliberate amount of redundancy and overlap was introduced into the panel assignments to avoid the tendency for important issues to "fall between the cracks" of the organizational structure (both within the Board and within NASA). This difficulty, which has plagued past advisory reports on space astronomy, usually arises when the choice is made to divide the study into either technique-oriented sections or problem-oriented sections. The Board attempted to overcome this by having panels divided in both ways, though usually with one or the other emphasis. Thus there are four primarily technique-oriented panels arranged by wavelength regimes: X and $\gamma$-ray, Optical ultraviolet, Infrared, and Long Wave Radio. In each of these reports, discussions will be found on all types of astronomical problems—Extragalactic, Galactic Stellar and Interstellar, Solar, Planetary and Interplanetary Research. However, these four panels, considered from the problem orientation, were primarily concerned with Extragalactic, Stellar and Interstellar Research and their principal contacts within NASA at the program level are with the program chief for Astronomy. (We should note that initially Optical UV and IR were a single panel, and the Optical report, which was taking shape as the IR panel was formed, still contains considerable discussion of IR problems.) The Board created two problem-oriented panels primarily for solar-system research, a Solar Panel whose contacts within NASA are with the program chief for Solar Physics, and a Planetary Panel whose NASA contact is the Chief of the NASA Planetary Astronomy Program. But it will be seen that each of these panels gave deliberate consideration to the applicability of various techniques from X and $\gamma$-ray to Radio. Finally, the Board has a panel on Particles and Fields Astronomy, which as a technique-oriented panel concerns itself with measurements of energetic particles and magnetic fields. As a problem-oriented panel, it is the one with a primary interest in the interplanetary medium, but through the particle fluxes from the solar wind, and the high energy cosmic rays of galactic and extragalactic sources, it is also interested in the rest of the universe. This panel works primarily with the NASA program chief for Space Physics but its experiments are supported by both the Physics and Astronomy programs and the Lunar and Planetary programs within NASA, as are those of the Planetary Panel.

## ENDORSEMENT OF THE PANEL REPORTS BY THE AMB

Since the length of the several reports effectively prohibited every Board member from reading every report in the short time between meetings, and since the desire was to make these reports available to the planning groups within NASA as soon as possible after they had been received by the Board, the following procedure was adopted so that two consecutive meetings would be the longest time needed to endorse a report or return it to the panel.

For each report a reading subcommittee was appointed with a chairman and at least two other readers, all chosen from outside the subdiscipline area of the report, and all Board members had at least one assignment. This served to deepen the Board members' knowledge of the overall program. The outside readers criticized (essentially refereed) the reports and suggested many editorial as well as some substantial changes which, in many cases, were made in future revisions. The chairman of each reading subcommittee had the responsibility to move AMB endorsement of the report.

Eventually all reports were endorsed in substance by the AMB (it is expected that many editorial changes may be made by the panels without altering the substance of the programs), with the following general qualification:

> A panel report correctly describes only relative priorities between missions in the given subdiscipline—priorities between subdisciplines are to be found in the AMB long-range plan (pt. III of the AMB position paper). Specifically, precise timing and dates of particular missions, as well as expenditure levels, mentioned in this panel report may have been modified in incorporating this program into the overall long-range plan as described in part III.

*Report of the X-Ray and Gamma-Ray Panel*
*September 1968*

# Recommended Program in High-Energy Astronomy

WILLIAM L. KRAUSHAAR, *Chairman;* GEOFFREY BURBIDGE, GIOVANNI G. FAZIO, WILLIAM A. FOWLER, HERBERT FRIEDMAN, RICCARDO GIACCONI, LAURENCE E. PETERSON, NANCY G. ROMAN (NASA contact)

## INTRODUCTION

Astronomy in the high-energy region of the electromagnetic spectrum has made important advances in the last several years. It is now established as an important new branch of astronomy capable of yielding information of great significance in the solution of cosmological problems.

In the X-ray region of the spectrum, galactic and extragalactic single objects have been observed with totally unsuspected properties. In some of these objects, X-ray emission exceeds by orders of magnitude all other radiative energy losses.

A diffuse background which is believed to be of extragalactic origin has been observed first in the X-ray region, then in the low-energy gamma region, and most recently in the gamma-ray region over 100 MeV. The values of the intensity and spectral composition of this background radiation (in the X-ray region) set significant bounds on the temperature and density of the intergalactic gas and (in the gamma-ray region) on the density of energetic electrons in intergalactic space. These matters bear directly on the question of a closed versus open universe.

X-ray astronomy offers the possibility in the not-too-distant future of extending the range of observable objects to extreme cosmological distances. The potential angular resolution exceeds that obtainable in the visible region by several orders of magnitude.

In the low-energy gamma-ray region, measurements of the extended energy spectrum from known X-ray sources have already been of singular value in understanding emission mechanisms. The detection of nuclear transition gamma rays from the so-called *r*-process chemical elements will offer possibly the only direct evidence on the place of origin of these elements.

High-energy cosmic-gamma rays have only recently been detected for the first time. The high-energy particles responsible for these gamma rays are either the same as or closely related to the high-energy particles responsible for the radiation from the many classes of remarkable radio sources. For radio production, the charged particles interact with magnetic fields, while for gamma-ray production the charged particles interact with gas or ambient radiation fields.

In the wavelength region over 1 Å, X-ray imaging and photon gathering are possible with grazing incidence reflectors. X-ray photographs of the Sun have already demonstrated the great promise of this method and there is little question that the method is destined to be the ultimate tool for high-resolution, spectral and polarization studies in the soft X-ray region. There is a set of well-defined technical problems which need laboratory study and development in order that this promising device can be used optimally when facilities for its flight become available.

In the longer studied regions of the astronomical energy spectrum, detection methods are well established and background problems are understood. In the high-energy region this is much less so. In the past year, for example, two methods have been developed (pulse rise time discrimination and wire-wall proportional counters) both of which accomplish significant decreases in background for mechanically collimated X-ray detection in the 2- to 10-Å region. In a young and developing field, order-of-magnitude improvement in sensitivity or background rejection is not unusual and is to be expected, provided the field remains vigorous and has broad participation. Continued availability of support and facilities for laboratory, balloon, rocket, and small satellite investigations as well as realistic plans and supporting development for observatory-type instruments is essential to the healthy development of high-energy astronomy.

While it is perhaps natural that the six decades in the portion of the electromagnetic-energy spectrum discussed here be grouped and called "High-Energy Astronomy," the diversity of production mechanisms, detection methods, and background problems is nearly as great as the diversity over six decades in any other part of the spectrum. For this reason, our report has been divided into three parts:

I. X-rays $<$ 15 keV
II. Gamma rays $<$ 10 MeV
III. Gamma rays $>$ 10 MeV

This grouping corresponds generally to identified interest groups and similarity in production mechanisms and detection techniques.

Figures 1 and 2 summarize the energy spectral information that is available. Figure 1 concerns the diffuse and presumably intergalactic intensity measurements, while figure 2 concerns Taurus A.

There follows a brief summary of our recommended program and a listing of the associated recommendations. Further details and explanatory material will be found in the body of the report.

## RECOMMENDATIONS

1. To provide the discipline of X- and gamma-ray astronomy:
   (a) Continued wide investigator participation;
   (b) Facilities for exploratory studies with small instruments; and
   (c) Facilities to rapidly exploit new technical developments and new scientific discoveries.

it is recommended that an SAS-type spacecraft be operated as an experiment "bus" or that space continue to be provided in the OSO wheel section. If the OSO wheel is to be made useful for these purposes, the telemetry capacity must be expanded and the wheel rotation rate must be reduced.

2. Rocket experiments will continue to play an important role in X-ray and ultraviolet astronomy both for testing and exploiting new techniques and for allowing broader participation. The present number of X-ray rocket flights, about 12 per year, is not sufficient to satisfy the growing needs of existing groups, and we recommend that over the next 5 years the number be increased to 18 per year. We recognize that this recommendation requires sufficient expansion of launch support personnel and facilities. In addition, we see an important need for improved Aerobee 150 performance, and anticipate a growing need for the Aerobee 350.

To provide improved sensitivity and performance and to demonstrate the very great promise of two of the major recommendations (High Energy A[1] and the X-ray telescope) of the High-Energy Panel of the AMB, we recommend that NASA make available to the X-ray astronomy community a few guided Aerobee 350 or equivalent rockets during fiscal year 1970.

3. SAS-type spacecraft, particularly the proposed improved

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[1] Also called the "heavy Explorer" in other parts of AMB position paper.

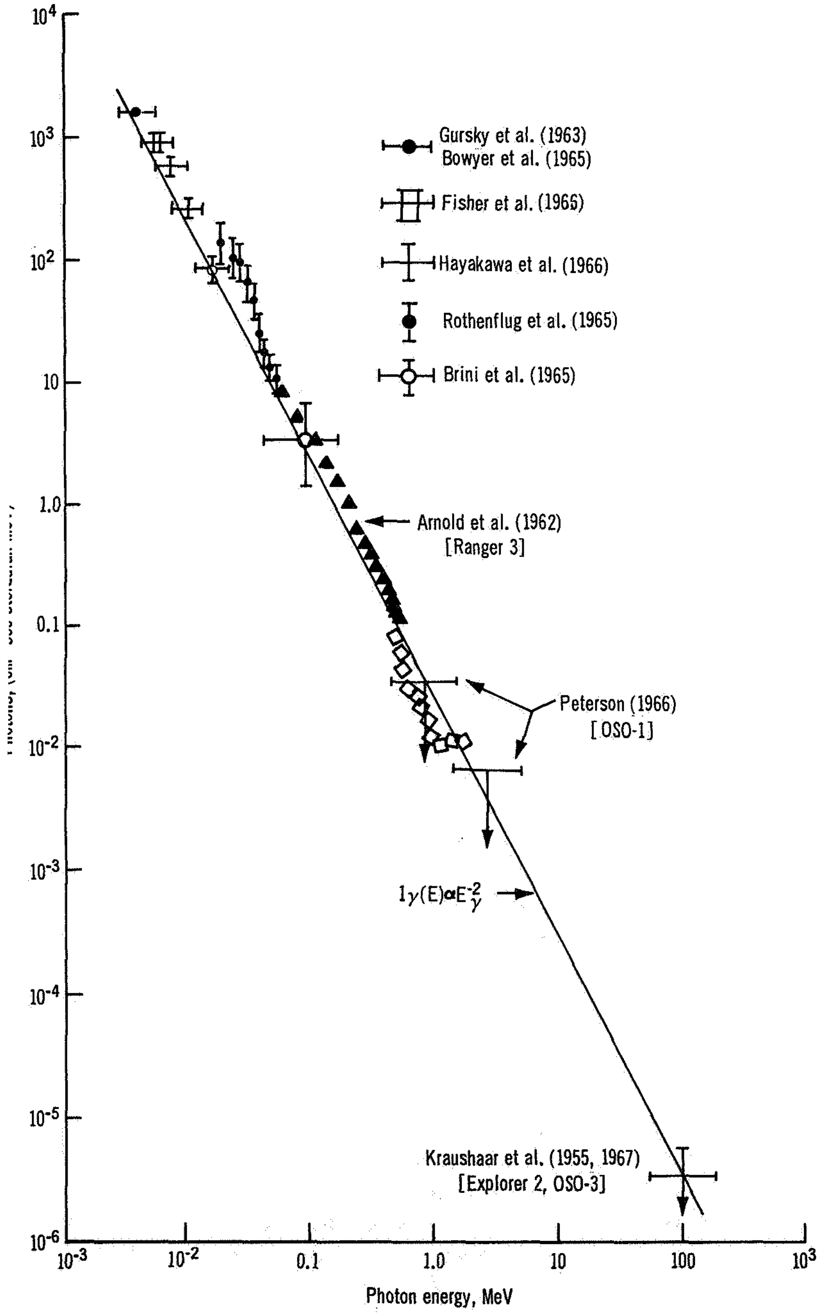

FIGURE 1.—Diffuse and presumably intergalactic intensity measurements.

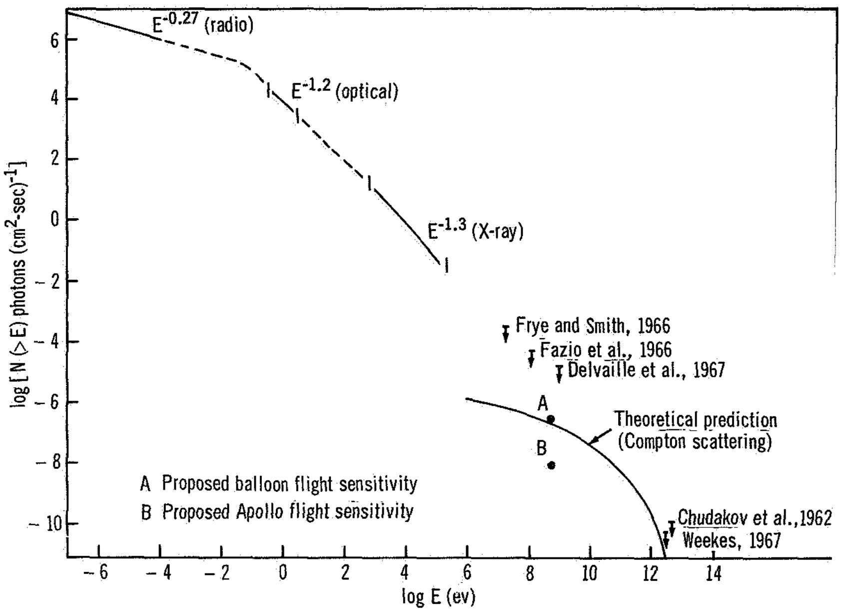

FIGURE 2.—Taurus A (Crab Nebula) integral electromagnetic energy spectrum.

version with 1 arc minute pointing and essentially zero spin rate will provide an important facility for broad energy resolutio studies of discrete X-ray and low-energy gamma-ray sources t a flux level $10^{-4}$ Sco X–1. Studies of the diffuse radiation in al three energy regions require spacecraft of the SAS type, but degree pointing is adequate. We recommend that NASA develo as soon as possible such a pointing system for the SAS.

4. The High-Energy Panel recommends that the OWSE carr an integrated group of instruments that require observatio of the same celestial objects. The high-sensitivity, high-energ gamma-ray spark chamber, the high-energy gamma-ray Cerenko telescope, and the high-energy resolution nuclear transition gam ma-ray detector are three such instruments.

5. The outstanding problems of X-ray astronomy relate to th nature of the physical processes occurring at the sources an the relation of X-rays to the emission at other wavelengths. T solve these problems, it is necessary to study in X-radiation th location, structure, presence of line emission and polarizatio Instruments to perform these tasks require long observatio periods (30 minutes) of a given source in a pointed mode. Absolu pointing capability accurate to arc minutes with similar stabilit

is required, in order to achieve an internal electronic fine guidance accurate to arc seconds. The OAO would admirably fulfill this role. We therefore recommend that an entire or a significant part of an OAO or a suitable alternative be made available for X-ray astronomy at the earliest opportunity. (See Woods Hole recommendations nos. 8 and 9.)

6. The High-Energy Panel of the AMB recognizes that many large experiments proposed for X-ray and gamma-ray astronomy can and should be flown as unmanned devices, and recommends that a payload be designed for an Atlas-Centaur-class launch. These experiments would extend in sensitivity the results of the SAS-type experiments. Experiments for this vehicle include large area detectors for nuclear transition gamma rays, high-sensitivity spark chambers, and a Cerenkov telescope. From the hardware point of view, this recommendation should be considered in combination with Recommendation—P&F—1.5 of the Particle and Fields Panel.

*7a.* The next logical step in imaging X-ray systems beyond the X-ray experiment discussed in Recommendation 5 will require an entire spacecraft of the Observatory class, designated "High Energy B." [2] In Alternative 1, this would be an unmanned mission. In Alternatives 2 and 3, the possibility of man attendance will mean that photographic film recovery may be part of the design. The plans include a single X-ray telescope of between 0.5- and 1-m diameter, focal length 5 to 10 meters, to be shared by several experimenters by substituting instrument packages at the focus.

The Panel feels that this is the next essential step to be taken before the 5- to 10-m aperture, 50- to 100-m focal length X-ray telescope planned as part of NASO.

*7b.* To achieve the full potential of X-ray imaging devices, it is necessary that supporting development of improved reflectors and detectors start in 1970.

8. The High-Energy Panel of the Astronomy Missions Board finds that vigorous and competent groups, working on problems where solutions are necessary to the development of high-energy astronomy, are being forced from the field because of lack of sufficient Supporting Research and Technology funds within NASA. The cost of the continued support of these groups is minute compared with the cost of the related flight missions. The work of these groups will almost certainly have broad and direct impact on the NASA missions. A larger portion of NASA's total budget should be allocated to the support of these and similar groups.

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[2] Also called the OXO—Orbiting X-ray Observatory—in other parts of the AMB position paper.

## FLIGHT SCHEDULES

### *High-Energy Astronomy*

### ALTERNATIVE 1

| | 69 | 70 | 71 | 72 | 73 | 74 | 75 | 76 | 77 | 78 | 79 | 80 | 81 | 82 | 83 |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Sounding rockets | 14 | 15 | 15 | 16 | 17 | 18 | 22 | 24 | 26 | 28 | 29 | 31 | 31 | 31 | 31 |
| Explorers: | | | | | | | | | | | | | | | |
| $E < 15$ keV | | SAS–A | | SAS–C | | (a) | (a) | | (a) | (a) | | (a) | (a) | | (a) |
| $E < 10$ MeV | | | | | | (a) | | (a) | (a) | | (a) | (a) | | (a) | (a) |
| $E > 10$ MeV | | | SAS–B | | (a) | | (a) | (a) | | (a) | (a) | | (a) | (a) | |
| OSO [b] | G | H | | I | J | | K | | | | | | | | |
| OWSE | | | (a) | (a) | (a) | (a) | (a) | (a) | (a) | (a) | (a) | (a) | (a) | (a) | (a) |
| OAO [c] | | | | | | E | | | | | | | | | |
| High energy | | | | | | A | | B | | | C | | | | |
| SR&T and data analysis | (a) | (a) | (a) | (a) | (a) | (a) | (a) | (a) | (a) | (a) | (a) | (a) | (a) | (a) | (a) |

[a] Indicates additional missions of the same type.

[b] Modest X- and gamma-ray instruments can be accommodated in wheel section.

[c] To accommodate first stellar X-ray imaging telescope.

ALTERNATIVE 2

| | 69 | 70 | 71 | 72 | 73 | 74 | 75 | 76 | 77 | 78 | 79 | 80 | 81 | 82 | 83 |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Sounding rockets | 14 | 15 | 15 | 16 | 17 | 18 | 22 | 24 | 26 | 28 | 29 | 31 | 31 | 31 | 31 |
| Explorers: | | | | | | | | | | | | | | | |
| *E* < 15 keV | | SAS–A | | SAS–C | | (a) | (a) | | (a) | (a) | | (a) | (a) | | (a) |
| *E* < 10 MeV | | | | | | (a) | | (a) | (a) | | (a) | (a) | | (a) | (a) |
| *E* > 10 MeV | | | SAS–B | | (a) | | (a) | (a) | | (a) | (a) | | (a) | (a) | |
| OSO [b] | G | H | | I | J | | K | | L | | M | | | | |
| OWSE | | | (a) | (a) | (a) | (a) | (a) | (a) | (a) | (a) | (a) | (a) | (a) | (a) | (a) |
| OAO [c] | | | | | | E | | | | | | | | | |
| ATM | | | | | | B (HEA) | | | | | | | | | |
| High energy | | | | | | | | B | | | C | | | D | |
| SR&T and data analysis | (a) | (a) | (a) | (a) | (a) | (a) | (a) | (a) | (a) | (a) | (a) | (a) | (a) | (a) | (a) |

Either OAO–E or ATM–B can accommodate first stellar X-ray imaging telescope.

[a] Indicates additional missions of the same type.

[b] Modest X- and gamma-ray instruments can be accommodated in wheel section.

[c] To accommodate first stellar X-ray imaging telescope.

*High-Energy Astronomy—Continued*

ALTERNATIVE 3

| | 69 | 70 | 71 | 72 | 73 | 74 | 75 | 76 | 77 | 78 | 79 | 80 | 81 | 82 | 83 |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Sounding rockets | 14 | 15 | 15 | 16 | 17 | 18 | 22 | 24 | 26 | 28 | 29 | 31 | 31 | 31 | 31 |
| Explorers: | | | | | | | | | | | | | | | |
| $E < 15$ keV | ----- | SAS–A | ----- | SAS–C | ----- | (a) | (a) | ----- | (a) | (a) | ----- | (a) | (a) | ----- | (a) |
| $E < 10$ MeV | ----- | ----- | ----- | ----- | ----- | (a) | ----- | (a) | (a) | ----- | (a) | (a) | ----- | (a) | (a) |
| $E > 10$ MeV | ----- | ----- | SAS–B | ----- | (a) | ----- | (a) | (a) | ----- | (a) | (a) | ----- | (a) | (a) | ----- |
| OSO | G | H | ----- | I | J | ----- | K | ----- | L | ----- | M | ----- | ----- | ----- | ----- |
| OWSE | ----- | ----- | (a) | (a) | (a) | (a) | (a) | (a) | (a) | (a) | (a) | (a) | (a) | (a) | (a) |
| ATM | ----- | ----- | ----- | ----- | ----- | B (HEA) | ----- | ----- | ----- | ----- | ----- | ----- | ----- | ----- | ----- |
| High energy | ----- | ----- | ----- | ----- | ----- | ----- | ----- | B | ----- | C | ----- | D | ----- | E | ----- |
| SR&T and data analysis | (a) | (a) | (a) | (a) | (a) | (a) | (a) | (a) | (a) | (a) | (a) | (a) | (a) | (a) | (a) |

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[a] Indicates additional missions of the same type.

## PROGRAM SUMMARY

### I. X-rays, $E < 15$ keV

1. *High-sensitivity surveys:*

A. SAS–A, S–027, and X-ray detectors on OSO–H will provide sky surveys in 1- to 8-keV region to $10^{-4}$ Sco X–1 level with 0.5° resolution. Broadband (proportional counter) energy resolution. Approved programs. 1970–71.

B. Instruments with sensitivity, angular and spectral resolution similar to those described in A above, but with extended energy response (200 eV to 1 MeV). SAS, the OSO wheel, and OWSE. 1972–74.

C. Very-high-sensitivity surveys ($10^{-6}$ Sco X–1) with state-of-the-art angular resolution, but at least 0.1°. ATM–B, possibly OWSE or Centaur-class High Energy Payload A. 1972–74.

2. *Detailed study of individual sources:*

A. Instruments with sensitivity and energy resolution similar to those required under 1A and 1B above, but flown as Explorer-type payloads that can be pointed. The greater time "on source" allows high sensitivity and more sophisticated experiments than possible during surveys. SAS (1972–).

B. First stellar X-ray imaging telescope. To provide positions to $\sim 1$ arc second, wavelength to $\Delta\lambda/\lambda \sim 0.01$ for sources to $\sim 10^{-4}$ Sco X–1. Instrumentation is state of the art, or requires modest development. OAO (1973–75).

C. Second stellar X-ray imaging telescope. Design goal $\sim$1-m aperture, interchangeable instruments at focus to provide image detectors, polarization measurements, spectral studies. Supporting development of the system, reflectors, and detectors should start now. Centaur-class, High Energy Payload B (1974–76).

3. *Rockets and Explorers.*—Broad participation with relatively quick turnaround.

### II. Gamma rays, $E < 10$ MeV

1. *Search for nuclear transition gamma ray emission:*

A. Development of detectors, collimation methods, and cryogenic techniques must continue, backed up by balloon flight program.

B. Angular and energy resolution sufficient to detect line fluxes in the $10^{-4}$ to $10^{-7}$ cm$^{-2}$ sec$^{-1}$ region. Possibly Explorers, but more likely ATM–B, OWSE, or Centaur-class High Energy Payload A. 1972–74.

C. If no line emission from Crab, must examine other supernova remnants or galactic nuclei at even high sensitivity.

2. *Continuum studies:*

A. Continued pointed balloon flight investigations. Broad energy resolution, state-of-the-art angular resolution to reach $\sim 10^{-3}$ cm$^{-2}$ sec$^{-1}$ level at $10^5$ eV for study of selected X-ray discrete sources.

B. Broad energy resolution, state-of-the-art angular resolution to reach $10^{-4}$ cm$^{-2}$ sec$^{-1}$ level at $10^5$ eV for study of selected X-ray discrete sources. SAS, OSO wheel, and OWSE. Payload should include compatible X-ray instruments for time variation studies. 1972–74.

C. Broad energy resolution, solid angle, and area matched to diffuse intensity. SAS, OSO wheel, and OWSE. 1972–74.

D. High-sensitivity, broad energy resolution, state-of-the-art angular resolution to study continuum gamma rays from known extragalactic X-ray sources. OWSE, ATM–B, or Centaur-class High Energy Payload. 1971–74.

### III. Gamma rays, $E > 10$ MeV

1. SAS–B will provide a sky survey with angular resolution and area for sensitivity in $10^{-6}$ cm$^{-2}$ sec$^{-1}$ flux region. Well matched to diffuse component. Some energy resolution. Approved program. 1971.

2. SAS–B and larger spark chamber devices will provide data on "lumpiness" of apparent diffuse component and some data on energy spectrum. Specific energy spectra-measuring devices for SAS-type launches may be necessary to complement directional data.

3. *High-sensitivity survey spark chamber.*—State-of-the-art angular resolution, some energy resolution, sensitive to $10^{-7}$ cm$^{-2}$ sec$^{-1}$ flux. ATM–B or Centaur High Energy Payload A. 1974–76. Further development with balloon backup necessary.

4. *Cerenkov telescope, primarily for study of discrete sources.*—Crude energy resolution, sensitivity $10^{-8}$ cm$^{-2}$ sec$^{-1}$ above 500 MeV. Balloon development necessary. OWSE or Centaur High Energy Payload A.

## X-RAYS, $E < 15$ keV

### Current State of Observations

The conditions exist for X-ray observations to become a major area within astronomy, with possibly an impact comparable to that of radio astronomy. X-ray production is not just a minor sidelight of cosmic phenomena. There are many X-ray sources. They display great diversity in their characteristics and the present observations must be presumed to represent only a small fraction of the potential observational material. Instrumental techniques are at hand to construct large arrays of detectors with high sensitivity, to obtain spatial resolution of the order of arc seconds with imaging optics, to perform high-resolution spectroscopy, and to measure polarization. These techniques have been already utilized in the study of solar X-ray emission and others have been utilized in the laboratory for a number of years. Not only will it be important to study the X-ray sources to obtain information on the X-ray emitters themselves, but unique information can be obtained concerning the properties of the interstellar material and the structure of galaxies.

About 30 discrete X-ray sources have been resolved. Almost all the data have been obtained from either sounding rocket flights which total less than 2 hours of observation time or about a dozen balloon flights. Figure 3 shows the distribution of sources in the sky, plotted in galactic coordinates. The outstanding feature of this distribution is the concentration of sources around the galactic equator, thus indicating that most of the sources are galactic objects. Thus far, only two sources at high galactic latitude have been unambiguously detected. These may well be

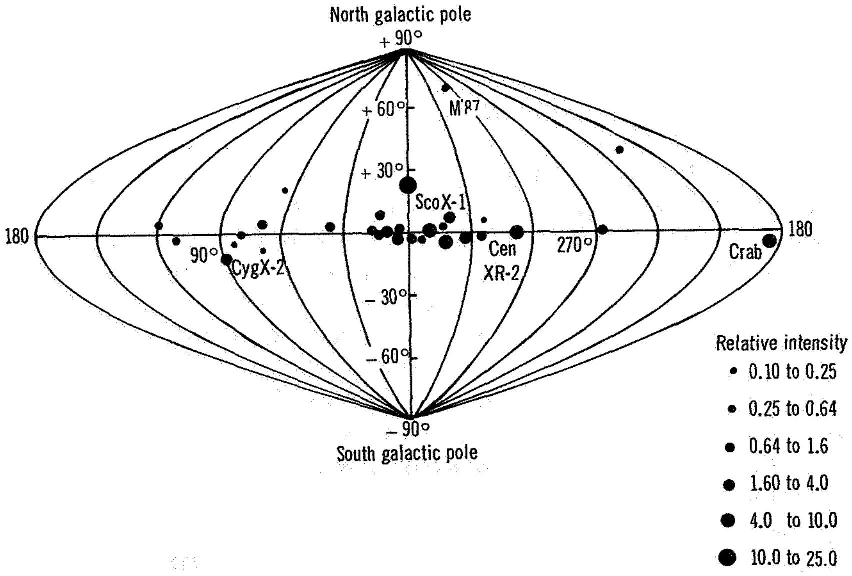

FIGURE 3.—X-ray sources energy interval, 1 to 10 keV.

extragalactic objects and, in fact, there is strong evidence for identifying one of them with the radio galaxy M87 (see below). The strength of the reported sources ranges from Sco X–1 ($3 \times 10^{-7}$ ergs/cm$^2$–sec, 1 to 10 keV) to others that are about three orders of magnitude weaker. The positions of several of the stronger sources have been defined to a precision of the order of arc minutes while the others are known to about a degree or two.

### *Galactic X-ray Sources*

Among the comparatively few galactic sources about which some measure of detailed information is available, there appear to be objects of at least two different kinds. The objects of the first kind are remnants of supernovae. The prototype of this class is the X-ray source in the Crab Nebula. The Crab has been positively identified as an X-ray source by two independent techniques: observation of a lunar occultation of the nebula in 1964 and by a high-resolution mechanical collimator in 1966. The X-ray source was found to have an angular diameter comparable to that of the visible nebula and the centers were found to be coincident within experimental error. At least two other galactic supernova remnants, Cas A and SN 1572, have been tentatively identified with X-ray sources. However, precise location and angular size information is still lacking, so that the association is not conclusive. Considerations of a statistical character, based on the density of supernovae, the density of X-ray sources, and the observational uncertainty in their positions, indicate that very likely additional X-ray sources are supernovae remnants.

Some of the other sources are of an entirely different nature. As a result of an observation with a high-resolution modulation collimator, Sco X–1 was located to a region of 4 square arcmin that included a bluish-flickering star of magnitude 12.5. On the basis of the thermal bremsstrahlung emission mechanism, the color and magnitude of the star were quantitatively in agreement with the hypothesis that it is the optical counterpart of the X-ray source. This identification was reinforced by a measured value of less than 20 arcsec for the angular size for the X-ray source. Hence, the energy emitted in X-rays exceeds that in the visible by a factor of $10^3$, making Sco X–1 in a very real sense an X-ray star. By analogy, an optical candidate with similar properties has been found within 6 arcmin of the less well-determined X-ray position of Cyg X–2. The two optical objects have been the recipients of a great deal of attention from astronomers who have noted peculiar features in their spectra and variability. The behavior of Cyg X–2, in particular, is suggestive of a binary system. A consequence of the optical identification has been estimates of the distance to the source. The conventional interpretation of the strength of the interstellar Ca II absorption line has placed a lower limit of 250 pc upon the distance to Sco X–1.

For these sources, and the others, correlations of position have been suggested with galactic distributions of novae, O–B associations, planetary nebulae, and Wolf-Rayet stars.

### *Extragalactic X-ray Sources*

Strong evidence has been presented for the emission of X-rays from the galaxy M 87 (Virgo A). A source has been localized to a region in Virgo that is well removed from the galactic equator. If this identification is correct, from the observed X-ray flux and from the approximately known distance of M 87 (about 10 megaparsec) one finds that the energy radiated by this object in the form of X-rays is of the order of $10^{43}$ erg $\text{sec}^{-1}$, which exceeds by a factor of 50 the emitted power in radio.

On the other hand, the strong radio source, Cygnus A, has been shown to have an X-ray emission of less than $2 \times 10^{-10}$ $\text{ergs/cm}^2$-sec at the Earth. Less-significant upper limits have been placed upon X-ray emission from the members of the local group.

### *Structure of X-ray Sources*

A start has been made in the direction of measuring the angular structure of X-ray sources. Previously, advantage has been taken of the rare lunar occultation occurrence to demonstrate that the emission region in the Crab Nebula was of finite size. Subsequently an instrument, the modulation collimator, was used to measure the size of the Crab in two dimensions and to set an upper limit of 20 arcsec upon Sco X–1.

### *Spectra*

The gross spectral features of X-ray sources have been observed with proportional counters. Substantial differences have been seen between various sources. Several sources are characterized by exponentially decreasing energy spectra that are consistent with simplified thermal bremsstrahlung radiation with temperatures in the range $10^7$ to $10^8$ °K. The higher energy data from balloons shows that others, such as the Crab Nebula and Cyg X–1, have much harder energy spectra that approximately fit a power law with spectral indices between 0.5 and 2. This is suggestive of synchrotron radiation or perhaps thermal bremsstrahlung in a situation of temperature nonuniformity.

Evidence has been presented for a low-energy cutoff in the spectrum of Cyg X–3. It appears that this effect may be taking place in some of the other sources. It has not yet been determined to what extent the absorption takes place in the source itself or is a consequence of the intervening interstellar medium.

### *Variability*

Variations in total emitted power and spectral hardness have been seen in some of the sources. The most dramatic instance has been Cen XR–2. This source was first detected in April 1967 at a level of intensity comparable to Sco X–1. Subsequently it was seen in three additional rocket flights, but with a decreasing intensity and effective temperature. Finally the region of the sky containing Cen XR–2 was explored without success by a rocket flight in September 1967. The failure to see Cen XR–2 means that its intensity (in the soft X-ray region) was at least 10 times smaller than in May 1967. The situation concerning the spectrum and the variability of Cen XR–2 is further complicated by a balloon observation carried out in October 1967, which revealed a hard component of the spectrum.

Variability has occurred in other sources. Cyg X–1 was observed to have undergone a large change in intensity during a year. Results from various rockets have indicated changes in the effective temperature of Sco X–1.

### *Diffuse X-ray Background*

Evidence for a diffuse X-ray flux has existed since the discovery of cosmic X-rays and there now appears to be a diffuse background throughout the X- and gamma-ray energy regions. Observations with thin-window proportional counters have shown that there is a substantial flux at 44 Å, a region where interstellar absorption effects are very large and therefore able to provide new data on the interstellar medium. There is evidence that the entire spectral range is not compatible with a simple function, which may indicate that more than one physical process is contributing to the flux. The intensity of the apparent background flux has already set meaningful bounds on the temperature and density of the intergalactic medium. Except for the interstellar absorption effects mentioned above, there is no evidence for other than an isotropic distribution of the radiation.

## Objectives of Observational X-ray Astronomy

Broadly speaking, at least three general categories of immediate experimental objectives can be defined for X-ray astronomy. These include surveys in selected regions of the sky, detailed studies of individual objects, and long-term monitoring of certain sources. A more detailed description can be given as follows. These objectives are quite realistic in that they are based on instrumentation ideas that are either straightforward extensions of existing devices or which have been demonstrated to be successful in the laboratory. Hence, it is reasonable to expect that all of these immediate objectives can be fulfilled within the next few years, providing sufficient support is forthcoming for an adequate number of missions and for the development of the associated technology.

### *High-Sensitivity Surveys*

Surveys are needed to establish catalogs of X-ray sources containing information on the position, intensity, and spectral distribution. Such catalogs are the necessary precursor for any detailed study of individual X-ray sources and yield valuable information of themselves. Positional correlations with known classes of objects can provide clues as to the nature of the X-ray sources, and the distribution of the X-ray sources in the galaxy can yield information on their evolution and their relation to other galactic objects. Specific objectives of surveys are

(1) *Detection of sources that are a factor of* $10^{-3}$ *as intense as those seen so far; i.e., to a limit of* $\sim 10^{-6}$ *Sco X–1.*—In the case of observations of galactic X-ray sources, the present objects are probably the order of 1-kpc distance; thus, a sensitivity of $10^{-3}$ Sco X–1 is sufficient for observing the most distant galactic objects, even allowing for interstellar absorption. The only candidate for an extragalactic X-ray source, M–87, is $10^{-3}$ Sco X–1; thus a sensitivity of $10^{-6}$ Sco X–1 is required to compile a thorough catalog of other extragalactic objects. Also at this sensitivity, common X-ray sources should be apparent within the nearest galaxies.

(2) *High angular resolution observations of regions of large source density, such as the galactic equator.*—The required angular resolution is the order of 1 arcmin, especially in regions at low galactic latitude where one is looking across the entire galaxy, or in a region, such as Cygnus, where one is viewing along a spiral arm. At high galactic latitudes, coarser resolution is probably adequate.

(3) *Determination of locations for a large number of sources with precisions between 1 arcsec and 1 arcmin.*—It is unlikely that X-ray observations alone will suffice to establish the physical nature of these sources. Since, generally, X-ray sources are faint in the optical and radio regions, positions accurate to 1 arcsec are necessary to identify the sources among the many optical and radio objects of similar brightness in the Milky Way. In less-congested regions, positions to 1 arcmin may suffice.

(4) *Observations over a broad wavelength range to exhibit the spectral differences between sources in terms of hardness and degree of attenuation at long wavelengths to yield direct information on the nature of the X-ray sources, for both individual objects and on a statistical basis.*—This is entirely analogous with the assignment of radio indexes.

(5) *Observation of the diffuse X-ray flux in a broad wavelength interval with instruments providing good angular resolution (1/100~1/1000 steradian or 3 to 30 square degrees).*—Measurement of the spectrum of the X-ray background can decide among different physical processes such as emission from a hot intergalactic gas, inverse compton interactions between the 3° K blackbody radiation and cosmic electrons, or the superposition of the X-ray emission from very distant galaxies. The celestial distribution of the X-ray background as a function of energy is needed to separate extragalactic, galactic, and local X-ray production. Also the intensity and spectrum of the background is directly related to the density and temperature of intergalactic gas.

### *Detailed Studies of Individual Sources*

(1) *Measurement of the X-ray flux from optical or radio classes of objects.*—Selected lists of single objects, suspected of being X-ray sources, can be studied with higher sensitivity than is attainable from all-sky surveys. Examples of such objects could be the known or suspected supernova remnants, the local group of external galaxies, nearby clusters of galaxies, certain radio galaxies, etc.

(2) *Location, to an accuracy of better than 1 arcmin.*—Surveys can be expected to yield high-precision positional data only for the more intense sources; however, it will be necessary to establish the position of selected numbers of even the faintest observed X-ray sources for the purpose of finding optical and radio counterparts.

(3) *Measurement of the angular size, shape, and density of the region of X-ray emission.*—This applies both to individual sources

of finite extent (M–87, Crab) and to aggregates of sources that may be found in neighboring galaxies. The structure of a source can yield immediate insight to its nature (a cluster of single sources or an extended source can be distinguished).

(4) *Spectral measurements with resolutions of between 0.1 and 0.01 percent.*—Spectral data may provide the most valuable information regarding the emission process that gives rise to the X-rays. A hot plasma will give rise to line-emission characteristic of the composition and temperature of the gas, while the synchrotron process will yield a purely continuous spectrum. Independently of the spectrum at the source, passage through the interstellar medium will produce absorption edges. The presence and values of these absorption features can furnish data on the composition of the interstellar medium almost entirely independent of the physical state of the medium.

(5) *Polarization to a precession of less than 1 percent.*—The presence of polarization is indicative of synchrotron emission, although collision bremsstrahlung can give rise to residual polarization under certain special conditions.

### *Monitoring the Radiation*

Time variation of the intensity emitted by several X-ray sources has been observed and is possibly a rather common feature.

(1) Correlated X-ray and optical observations on identified sources can help to determine the nature of the production process and the physical characteristics of the objects. Different conditions give rise to variations occurring over periods from seconds to months.

(2) Search for transient phenomena in galactic and extragalactic objects will allow us to establish the occurrence in X-rays of explosive outbursts such as are observed in the optical region in novae and supernovae.

## Supporting Technology

Implementation of the scientific objectives of a program of X-ray astronomy will require development and technological support. There are several aspects to the question of supporting technology. In the first place, there are the detectors themselves. Secondly, there are the auxiliary devices which are needed for the construction, calibration, or use of the primary instrument. Finally, for advanced missions there are problems involved in the integration and use of the various components of the experiment related to the nature of the vehicle and the mission.

In the area of the detectors, the recommendations given below reflect the needs of imaging and nonimaging systems.

### *Imaging Systems (Grazing Incidence Telescopes)*

Although grazing incidence telescopes have already been productive, certain advances in the state of the art are needed before the enormous capability of the X-ray telescope can be fully utilized. Effort is recommended in the following areas.

### *Telescope Fabrication*

1. Development of techniques and facilities for measuring the characteristics of the mirrors in the X-ray range.
2. *Establishing visible light criteria that can be used by mirror fabrication for determining the quality of a mirror surface.*—It should be noted that present technology for measuring the figure of a mirror is adequate; what is required are new criteria for assessing the quality of the surface polish.
3. *Study of combinations of mirror surfaces, other than the conventional paraboloid-hyperboloid, for use in astronomical applications.*—The present configuration is restricted to a narrow range of *f*-numbers. Other combinations can yield a variety of *f*-numbers and might be more easily fabricated. Mirror systems may also be important in certain nonimaging applications.
4. *Study of materials suitable for mirrors.*—The relevant criterion is the dimensional stability in the appropriate mechanical and thermal environment.
5. *Advances in polishing technique.*—Present technology aims at high-quality optics in the visible range. The requirements in the X-ray range are necessarily more severe.

### *Image Detectors*

A suitable image detector which maintains the inherent resolution capabilities of the telescope is needed. (A microchannel plate electron multiplier appears promising.) This necessitates the following work:

6. Studies of the spatial resolution, noise characteristics, and stability in an adverse environment.
7. Development of a stable, rugged cathode with a large quantum efficiency in the X-ray region.
8. Development of a suitable system for reading out and storing the information obtained by the device.

### *Spectrometers and Polarimeters*

Specific instruments or systems may wish to exploit the imaging characteristics of the telescope to restrict the observation to select sections of an extended region of X-ray emission. This implies the following requirements:

9. Development of spectrometer systems based on Bragg reflection and transmission gratings or other principles that can fully utilize the high-resolution capabilities of the telescope. The same applies to polarimeters.

10. Consideration should be given to the problem of integrating the various devices into the telescope system; i.e., making them compatible with the telescope itself and with each other.

### *Other Focusing Systems*

Most of the research efforts outlined under "Imaging Systems" are also needed for the development of nonimaging focusing systems.

### *Nonfocusing Systems*

11. A thorough study should be made of large-area proportional counter arrays with low background levels. It has recently been demonstrated that residual non X-ray cosmic background levels in proportional counters can be reduced by more than an order of magnitude at least in the energy range 2 to 10 keV by the method of pulse-shape discrimination. Counter parameters, particularly the gas mixtures, should be optimized to derive maximum benefit from the effect. The method should be extended to higher Z-gas mixtures.

12. Techniques for fabricating compact large-area mechanical collimators of very high angular resolution need to be developed in conjunction with the large-area proportional counter arrays. Uniformity, alinement, and resistance to degradation through the various mission phases are essential.

13. Particular attention should be given to the development of detectors sensitive to long wavelength. Two concepts: ultrathin organic film window gas flow proportional counters and windowless electron multipliers should be pursued. Stability and efficiency are problems in both cases.

### *Auxiliary Devices and Methods*

14. Support should be provided for the development of photometric standards, particularly in the form of X-ray sources for the purpose of calibrating detectors. Both monochromatic and

continuous energy spectra are needed. Several concepts should be considered, including the use of the synchrotron spectrum that is present at high-energy electron accelerators.

15. Similarly, methods of preparing large-area X-ray beams of extremely fine collimation are needed for the proper calibration of the high-resolution instruments.

## GAMMA RAYS, $E<10$ MeV

### Current State of Observations

At present, about a dozen strong X-ray sources have also been observed with energy spectra extending to higher energies. The spectra appear to be either the power-law type (possibly due to synchrotron emission) or the exponential type (possibly due to thermal bremsstrahlung). In most cases, the spectrum is not well enough measured to provide definitive evidence. Forty-keV X-rays from the extragalactic object M–87 have been reported. Most of the observations have been made from balloons, although the OSO–III will provide the spectrum of some 20 objects over the 7.7- to 200-keV range. The highest energy $\lambda$-rays (several hundred keV) yet detected from discrete objects are from the Crab Nebula and Cygnus XR-1.

No extraterrestrial gamma-ray lines have yet been observed from any source, discrete or diffuse. Upper limits of the predicted 0.5–MeV annihilation radiation lines of $5\times10^{-2}$ photons/cm$^2$ and the 2.2-MeV lines from deuteron formation of $10^{-3}$ photons/cm$^2$-sec apply to most point objects, including the Sun. These were obtained some years ago on the early Ranger flights and have not yet been superseded, although upper limits from current balloon experiments are also at this level. The search for nuclear $\gamma$-ray lines from $r$-process element formation has resulted in upper limits for the Crab Nebula. These limits are about $10^{-3}$ photons/cm$^2$-sec for the 60-keV Am$^{241}$ line and the 180-keV Cf$^{251}$ line, as determined from a Ge(Li)-cooled detector flown on a balloon. The predicted line intensities are $10^{-1}$ to $10^{-7}$ photons/cm$^2$-sec.

The diffuse or background X-radiation observed with rockets has been extended to higher energies by the Ranger observations, and these observations have been confirmed by balloon observations to form a continuous spectrum from several hundred electron volts to several MeV. At present this diffuse component is known to be isotropic to within about 10 percent; the low-energy gamma-ray instrument aboard OSO–III should provide further information on the anisotropy to about 200 keV. The prospect of detecting $\gamma$-rays in the 0.3- to 10 MeV range is particularly exciting, since the recent discovery of 100-MeV gamma rays from the galaxy by the high-energy gamma-ray detector on OSO–III. The lower energy photons from the galactic center should be easily seen with a rather modest collimated balloon-borne detector, unless the extrapolation to lower energies from 100 MeV is exceedingly flat.

Solar gamma rays have not been detected, although they have been predicted as a part of flare phenomena. X-rays to about 50 keV, presumably an extension of the soft solar flare X-ray spectrum, have been observed many times and one reported event (July 7, 1966) produced detectable X-rays to 300 keV.

Time variations of X-ray sources are established. These variations have now been observed in the 50- to 100-keV range, and may be relatively more severe at the higher energies. The most studied source is Sco XR–1, where detailed correlations of X-ray variations with the previously known optical fluctuations have not been observed.

## Objectives

The objectives in this energy region have been made much more definitive since the 1965 Woods Hole report, because of the extensive balloon measurements to ~300 keV which are now available, and the discovery of a definite galactic flux in the 100-MeV range.

### *Diffuse Background*

*a.* Extend the energy spectrum of the background radiation to energies above 1 MeV. There is no information between 1 and 100 MeV, and it is in this region that production mechanisms can be distinguished. In one class of cosmologies, the background should peak at ~20 MeV.

*b.* Study the energy spectra and departures from isotropy in order to separate galactic and extragalactic components, and to determine their production mechanisms.

*c.* Study the background radiation with good angular resolution (~1°) to separate possible apparent background from many weak sources and a true diffuse background.

### *Discrete Sources*

*a.* One of the central problems in nucleogenesis is the understanding of the origin of elements in the upper half of the periodic table. A vitally important step toward achieving this understanding would be the discovery of nuclear transition gamma rays (from the so-called *r*-process elements) coming from supernova remnants or other gaseous regions. The problem has been thoroughly investigated theoretically and predictions of line intensities from the most likely source, the Crab Nebula, are available. Preliminary work has been done and should continue to be done from balloons. Definitive measurements require large-area, well-collimated, cryogenically cooled solid-state detectors flown on satellites so as to view likely objects for extended periods of time.

*b.* Extend the energy spectrum of known strong X-ray sources to 300 keV, 1 MeV and higher, if possible, in order to study gamma and X-ray production mechanisms. To 300 keV, balloon work will continue to be profitable, but satellite work is needed. Above 300 keV, measurements must be made from satellites.

*c.* Study time variations of gamma-ray fluxes from point sources, determine the associated spectra changes, and correlate these with optical X-ray and radio variations. Such data will provide information on the various source mechanisms, and the dynamical conditions in the emitting object. Short-period, transient variations, and occasional additional points may be obtained from balloon work; long period and continuous observations require instruments with large spectral coverage on satellites.

### Supporting Technology

The following areas require continued technological development. In many cases these developments may be advanced simultaneously with balloon observations preparatory to placing instruments on small or large satellites.

(1) *Continued study of anticoincidence collimation techniques for use in the 300-keV to 10-MeV range.*—Here instruments capable of collimating a 100-cm$^2$ detector to about 10° have now been designed but have not yet been constructed and tested. Test of these techniques and extension to a 500-cm$^2$ area and 3° aperture is most desirable. Further investigation of the Compton telescope technique and pair coincidence spectrometers should be profitable.

(2) *Development of less-expensive, more reliable large area and large volume cooled solid-state detectors for use in space-qualified gamma-ray spectrometers.*—Further work on associated active and passive cryogenic cooling sytems for balloon and satellite systems is needed.

(3) *Development and study of direction-determining and background-rejecting techniques for use in the 15- to 300-keV region.*—At present there are several schemes being investigated and used in balloon, rocket, and small satellite programs. Progress depends on continued investigations.

(4) *Continued development of larger, more reliable balloon sysems for use with larger payloads in the 70's.*—Although this work has progressed quite well since the 1965 Woods Hole document, he demands have also increased, and this work must be continued.

(5) *Further development of balloon data-handling, control, and guidance systems.*—Once again considerable improvement has occurred since 1965; however, a "universal" balloon pointing system, good to at least 0.1°, preferably 1 minute, is not available. Each laboratory now constructs its own system as part of the experiment, with little input from other groups or exchange of ideas. This is cumbersome and inefficient.

## GAMMA RAYS, $E > 10$ MeV

### Current State of Observations

### *The Diffuse Flux*

While convincing evidence has existed for some time for the existence of a diffuse (and presumably intergalactic) flux of X and gamma rays below 1 MeV, only the recent OSO–III gamma-ray results have established a diffuse flux at higher energies. This diffuse flux consists apparently of two components, one being truly diffuse and isotropic and the other being concentrated from directions near the galactic plane and particularly from near the galactic center.

The intensity of the isotropic component falls near an $E^{-2}$ extrapolation of the low-energy measurements, as shown in figure 1. The intensity of the galactic component is appreciably greater than can be comfortably accounted for by collisions of energetic particles (cosmic rays) with the interstellar medium.

Measurements of the diffuse gamma ray flux in the $10^5$- to $10^{10}$-eV energy region must be performed from satellites. The diffuse atmospheric background flux prevents significant measurements at balloon altitudes. Rockets are not applicable due to the limited payload weight and short exposure times. The Goddard Space Flight Center is planning a digitized spark chamber for SAS–B which will be capable of verifying and significantly extending the diffuse flux measurements above 30 MeV. The University of Rochester has a high-energy gamma-ray detector on OSO–III, but gamma-ray detection is a secondary purpose and no results are as yet available. A very small acoustic spark chamber, provided by the University of Southampton, is aboard OGO–E. Preliminary results seem to indicate a serious background problem.

### *Flux from Discrete Sources*

No positive evidence exists for gamma radiation greater than 10 MeV from discrete cosmic sources. Several instances of a detectable flux have been reported in the literature, but these results have not been verified by other experiments. The spectra of several discrete cosmic X-ray sources (e.g., Crab Nebula, SCO X–1) have been measured to energies of the order of 500 keV. No gamma rays with energy greater than 500 keV have been detected from the quiet Sun or from solar flares.

To date, the most serious attempts at point source detection of gamma rays in the 30-MeV to 10-GeV energy region have been carried out with balloon-borne spark chambers. Several groups have succeeded in establishing upper limits near $10^{-5}$ photons $(\text{cm}^2\ \text{sec})^{-1}$ to the flux from likely sources.

The Minnesota and St. Louis groups use nuclear emulsions in conjunction with the spark chamber. The nuclear emulsion tracks are used to observe the converted electron-positron particles before they scatter, thus achieving much better angular and energy resolution than with spark chambers alone. Balloon-borne equipment may yield flux sensitivities of the order of $10^{-6}$ photons $(\text{cm}^2\ \text{sec})^{-1}$.

An alternative detector to the spark chamber is being pursued by a joint group from Cornell University, the Naval Research Laboratory, and the Smithsonian Astrophysical Observatory. The proposed device is a very large area (5 $\text{m}^2$) gas-Cerenkov detector with about 1° angular resolution. Unlike the spark chambers which require accurate knowledge of, but not precise, pointing, the Cerenkov detector will require prolonged exposure to a suspected source. Its advantages are simplicity and large area. The predicted minimum detectable flux (over 500 MeV) for a balloon flight is $4 \times 10^{-7}$ photons $(\text{cm}^2\ \text{sec})^{-1}$. The group is presently involved in an extensive balloon-flight program and an experiment has been proposed for satellite flight.

Table 1 sumarizes the predicted sensitivities of several gamma ray detectors now under construction.

### *Conclusions*

Even at present levels of sensitivity, the established upper limits of intensity have permitted significant cosmological and astrophysical conclusions. It has been noted that in the X-ray region, the diffuse flux is far below that predicted to originate in the intergalactic medium by the hot universe model and hence permits the discarding of this model. Similarly, the weak intensity of the

TABLE 1.—*Cosmic Gamma-Ray Sensitivities for Proposed Experiments*

| Experiment | Group | Technique | Schedule | Vehicle | Gamma-ray energy, MeV | Period of observation | Flux sensitivity photons, $cm^{-2}$ $sec^{-1}$ |
|---|---|---|---|---|---|---|---|
| (1) [a] | Goddard SFC | Spark chamber | 1971 | Satellite | 30 | 6 months, sky survey | $5\times10^{-7}$ |
| (2) | Goddard SFC | Spark chamber | 1971 | Satellite | 30 | Pointed, 1 week | $1\times10^{-7}$ |
| (3) [b] | Minnesota-St. Louis | Emulsion-spark chamber | 1968 | Balloon | 200 | 1 flight | $1\times10^{-6}$ |
| (4) | Minnesota-St. Louis | Emulsion-spark chamber | 1972 | Satellite | 100 | Pointed, 10 days | $5\times10^{-8}$ |
| (5) [c] | Cornell-SAO-NRL | Gas Čerenkov | (?) | Balloon | 500 | Pointed, 6 hours | $4\times10^{-7}$ |
| (6) | Cornell-SAO-NRL | Gas Čerenkov | (?) | Satellite | 500 | Pointed, 1 month | $8\times10^{-9}$ |
| (7) [d] | SAO | Atmospheric Čerenkov | 1969 | Ground | $2\times10^{5}$ | 100 drift-scans | $8\times10^{-10}$ |
| (8) [e] | Dublin-Harwell | Atmospheric Čerenkov | 1969 | Ground | $1\times10^{6}$ | 100 drift-scans | $1\times10^{-10}$ |

[a] Fichtel, 1968.

[b] Long, 1968.

[c] Fazio, 1968.

[d] Fazio, Helmken, Rieke, and Weekes, 1968.

[e] Porter, 1968.

0.5-MeV line and at 100 MeV rule out the hypothesis of a universe containing equal amounts of matter and antimatter.

When interpreted, the observations from OSO–III of a 100-MeV flux from the galactic plane should provide significant insight into the density and distribution of interstellar high-energy particles.

The results of the Explorer XI and OSO–III experiments have indicated that the high-energy intergalactic electron density is less, at least by a factor of 10, than the galactic density. This conclusion is based on the expected gamma-ray flux due to Compton scattering by high-energy electrons of visible light photons in intergalactic space.

The failure to detect $10^{13}$-eV photons from the Crab Nebula has shown that the high-energy electrons in the Crab Nebula are not the product of high-energy proton interactions and therefore must themselves have been accelerated.

## Objectives

Gamma radiation in the >10-MeV region of the electromagnetic spectrum is a particularly important probe in cosmology because of (*a*) high penetrability through galactic and intergalactic matter, (*b*) the direct and simple relationship that exists between the data received and the cosmic source mechanism, and (*c*) the importance of the information that can be obtained on matter and photon densities, high-energy particles, magnetic fields and antimatter.

The immediate objectives of high-energy gamma-ray astronomy can be divided according to the nature of the source; i.e., whether it is diffuse or discrete. The present discussion is concerned with radiation in the $10^7$-eV to $3 \times 10^{11}$-eV energy region, since above the latter energy the radiation can be detected with ground-based instruments.

### *Diffuse Gamma-Ray Flux*

(1) *Improve angular resolution measurements.*—While the OSO–III results establish the existence of gamma rays of cosmic origin, the poor angular resolution of the instrument cannot distinguish, except in the crudest sense, between a truly diffuse source and a conglomerate of discrete sources. Quite apart from the inherent value of the study of discrete sources, the flight of instruments with improved angular resolution is clearly necessary in order to further study even the diffuse gamma rays.

(2) *Increase flux sensitivity.*—Rather fundamental considera-

tions limit high gamma-ray detection resolution to about 1° in the foreseeable future. Given the intensity of the apparent diffuse component ($10^{-4}$ cm$^{-2}$ sec$^{-1}$ sr$^{-1}$), one sees that sensitivity to discrete sources in the $10^{-6}$ to $10^{-7}$ cm$^{-2}$ sec$^{-1}$ region is appropriate.

(3) *Improve energy resolution.*—It is tempting, in view of the simple power law relationship which apparently exists in the X- and gamma-ray energy spectrum, to attribute the radiation in this broad energy region to a single physical process. But note that there is a broad unexplored region of two decades, extending from 1 MeV to 100 MeV. The two likely high-energy gamma-ray production processes—neutral meson decay and electron interactions (bremsstrahlung and inverse-Compton scattering)—can fortunately be distinguished by even very crude energy spectrum measurements. This is because a simple relativistic argument shows that a $\pi^\circ$-decay gamma ray spectrum must peak at $mc^2/2=$ 70 MeV regardless of the energy spectrum of the $\pi^\circ$ mesons, and an electron-produced gamma-ray spectrum is bound to be similar to a simple power law such as $E^{-2}$. An additional factor which makes energy measurement important is the fact that certain cosmologies predict a peak in the background energy spectrum at about 20 MeV.

A very important ingredient, therefore, to the understanding of the diffuse gamma-ray component will be its energy spectrum in the region 10 to several hundred MeV. Crude ($\sim$30 percent FWHM) energy resolution is sufficient. It is doubtful that the OSO–III or SAS–B instruments will provide this information. Future devices which aim to study the diffuse component should include energy measurement.

### *Gamma Radiation From Discrete Sources*

(1) *Search for discrete sources with increased flux sensitivity.*—Theoretically, there is every reason to believe that measurable gamma fluxes emanate from at least the stronger radio sources. The predicted flux levels are in general much smaller than the present level of sensitivity, $10^{-5}$ cm$^{-2}$ sec$^{-1}$ and are near $10^{-7}$ or $10^{-8}$ cm$^{-2}$ sec$^{-1}$ for many of the more interesting objects.

(2) *Energy spectrum measurements.*—Electromagnetic radiation in the radio and optical region, if produced by synchrotron radiation, are indicators of the energetic electron and magnetic-field content of the radiating regions. Electromagnetic radiation in the X- and low-energy gamma-ray regions are indicators of the energetic particle and photon content, if the radiation is inverse-Compton produced. Electromagnetic radiation in the high-energy

gamma-ray region (10 MeV and over) is an indicator of the energetic particle and gas or photon content. As mentioned in connection with diffuse sources, crude energy spectrum measurements in the 10- to 100-MeV region can distinguish between electron- and nucleon-produced radiation.

(3) The minimum next major effort in high-energy gamma-ray discrete source detection should aim toward sensitivity in the $10^{-7}$ cm$^{-2}$ sec$^{-1}$ region. Crude energy measurement is important.

### Supporting Technology

High-energy gamma-ray detection is characterized by low quantum arrival rates, large potential background, and inherent difficulties in determining arrival directions and quantum energy. As a result, the following areas require development:

(1) *Further investigation of the background problem.*—The primary flux of charged cosmic-ray particles is at least 1000 times that of the primary high-energy gamma rays. Furthermore, in the atmosphere and in the walls of a vehicle or the detector itself, these charged particles are efficient generators of secondary gamma rays which (except for the directionality of radiation from point sources) are indistinguishable from the primary gamma rays one hopes to detect. Associated with this secondary production is the likelihood of long-range penetration and large-angle scattering. In order to detect sources as weak as $10^{-8}$ photons (cm$^2$ sec)$^{-1}$, the rejection factor must be of the order of $10^9$. The background problem can and should be further investigated. Laboratory tests should be carried out in accelerator-produced beams and new developments should be thoroughly tested with balloon-borne instruments.

(2) *Improved angular resolution.*—Energetic gamma rays can only be detected via the electron pairs they produce. Pairs are only produced by gamma-ray interactions in matter, and in matter electrons scatter. Hence it is vital to develop instruments wherein pair direction is determined before appreciable scattering has occurred. The use of nuclear emulsions represents one attempt to attack this problem. Very-thin-plate multilayer spark chambers is another. Yet another attempt is to accept only very energetic gamma rays whose pairs scatter less. High-energy gamma-ray detection has not been developed to the point that one method can be demonstrated to be clearly superior to others. The relatively modest efforts involved in developing and balloon-flight testing new detection methods is essential.

(3) *Energy measurement.*—Some energy resolution in high-energy gamma-ray detection is essential to distinguish between production mechanisms. Measurement of pair opening angle, pair

scattering, and total absorption are all possible and suggested methods. Here again, no single method is demonstratively best or indeed even of proven worth in cosmic gamma-ray detection. Only interested and more or less competing groups can solve this problem.

(4) *Rapid data analysis.*—To achieve the required large background rejection, particularly in spark chamber devices, a high information content per recorded event is necessary. The need for rapid analysis and storage of large numbers of events, mainly background on which the sought-for flux is superimposed, calls primarily for electronic ingenuity, built-in detector selectivity, and experience with special computation techniques. Experience in the solution of similar but sometimes even more serious problems in high-energy nuclear physics suggests that this problem is probably solvable. But few, if any, astronomy-oriented groups are actively pursuing the rather sophisticated methods the nuclear physicists have developed.

(5) The points raised in paragraphs 1 to 4 above emphasize the areas of uncertainty and need in gamma-ray-detection techniques. Despite these uncertainties, certain features characterize the requirements of high-energy gamma-ray detectors. They are:

(*a*) Pointing accuracy or aspect knowledge is necessary to only about 0.5°.

(*b*) Some devices (the gas-Cerenkov detector) require pointing to within 2° for prolonged periods measured in days or weeks. Other devices, in particular spark chambers, can accommodate drifting motions with aspect knowledge to within 0.5°. Data analysis is significantly more complicated with drifting motion.

(*c*) No presently conceived gamma-ray detector will have an integration time short enough for a scientist-astronaut to react to the incoming data in real time. Repair is perhaps desirable, but it seems clear that high-energy gamma-ray detectors are best suited to semiautomatic or fully automatic operation.

(*d*) High-energy gamma-ray detectors will be large and heavy. Useful observations can be made on the diffuse component with SAS-type payloads, and some exploratory work is possible in searching for discrete sources. A serious discrete source detector, however, will have to be one or more square meters in area and will likely weigh more than a ton.

(6) As mentioned in low-energy $\gamma$-ray astronomy (p. 36), continued development of large balloons and guidance systems ($\sim 1°$) is needed.

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# Optical Space Astronomy

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## INTRODUCTION AND SUMMARY

### Introduction

Man's understanding of the vast universe in which we form so minute a part has been largely based on knowledge obtained with optical telescopes. Pointed at stars, at gaseous nebulae between the stars, and at the remote stellar aggregations called galaxies, these telescopes have given man his present knowledge on the size of the universe, and on the origin, composition, evolution, and present age of the stars. Data obtained with these instruments require for their interpretation the physical laws deduced from terrestrial observations, but the astronomical measurements have in the past provided an important and in some cases a primary verification of these laws, as, for example, in theories of gravitation and in the properties of matter at enormous temperatures and pressures.

In space astronomy the effectiveness of optical telescopes is very greatly enhanced by the absence of a surrounding atmosphere. The air absorbs essentially all ultraviolet radiation short of 3000 Å, and much of the infrared spectrum between 1-$\mu$m and 1-mm wavelength. In addition, thermal inhomogeneities in the Earth's atmosphere deflect the light that does pass through, and limit the sharpness of most astronomical photographs to about 1 arcsec. An optical telescope in space can effectively focus photons with wavelengths between 1000 Å and 1 mm, with a sharpness limited in principle only by the ratio of its diameter to the wavelength of light; thus this entire spectrum can be measured with a single such instrument.

The ability to make measurements at wavelengths hitherto inaccessible and to obtain much sharper images at visible wavelengths would open up completely new fields of research. Such an extension of man's capabilities can be expected to produce results both along lines that are anticipated, based on extrapolation of present knowledge, and along entirely new and unexpected lines. Because of the relatively great complexity and high cost of optical space instruments, with the pointing and guidance accuracy they require, this area of space research has started slowly, and most new results are still in the future. However, even a few flights of small spectrographs on sounding rockets have revealed tremendous stellar winds streaming away from hot giant stars at velocities of about 1000 km/sec, and have indicated that the amount of interstellar gas between the stars in the form of atomic hydrogen may be substantially less than previously assumed.

On the basis of the extensive astronomical results obtained from the ground, we can predict with considerable confidence those areas in which observations with space telescopes should give important information. We know that there are major questions in astronomy which cannot readily be answered with data from ground-based instruments, but which we believe could readily be resolved with observations obtained by telescopes above the atmosphere. Parts II through V of this report on optical space astronomy summarize some of the more important of these questions, and indicate the great importance which precise answers could have for astronomy as a whole. As examples, we give here four areas of astronomy where optical space telescopes could yield particularly significant results.

(1) *Activity in stellar atmospheres.*—While much has been learned about storms, eruptions, and other violent events characterizing the weather in the solar atmosphere (flares, prominences, bursts, spicules, etc.), there is relatively little detailed information on such processes in other stars. Detailed studies of the ultraviolet radiation emitted by stellar chromospheres and coronas in a wide variety of stellar types should give fascinating new information and may greatly help to clarify the physical processes involved. In particular, stellar spectrophotometry in the ultraviolet should give detailed information on velocities, temperatures, and physical state of the gas in these outer stellar atmospheres and the changes of these quantities with time. Such information would be of particular interest for the very hot, very cold, and

very unstable objects, which hold clues of especial importance for understanding the history and fate of stars.

(2) *Nature of the interstellar medium.*—The gas between the stars plays a pivotal role in the formation and evolution of stars and galaxies. Astronomers believe that this enormously rarefied material, initially a mixture of pure hydrogen and helium, condenses into new stars, where heavy elements are formed, and is then ejected back into interstellar space, gradually changing the chemical composition of the gas. This picture, however, is still uncertain, since so little is known about the chemical composition, energy balance, and overall physical state of the interstellar gas. The absorption lines of all the abundant atoms in interstellar space are concentrated at wavelengths short of 3000 Å, which do not penetrate the Earth's atmosphere. Measures of such absorption lines with a space telescope would give detailed information on the chemical composition of the interstellar gas and would increase a thousandfold the sensitivity for detecting this gas.

(3) *Physical processes in planetary atmospheres, comets, and the interplanetary gas.*—While much information can be obtained from research at visible wavelengths, many types of atoms and molecules which are of crucial importance in the planetary system can best be detected in the ultraviolet or infrared. In addition, spectroscopic data with high spatial resolution obtainable from a space telescope can give detailed information on the phenomena in planetary atmospheres. As on the Earth, such atmospheres are continually changing, and more continuous measurements can be obtained with space telescopes around the Earth than with occasional planetary probes.

(4) *Galactic explosions.*—We have only recently begun to realize the importance of gas injection and other forms of violent activity in certain galaxies, especially in their relatively small but massive nuclei. In the quasi-stellar objects (quasars), such phenomena are associated with an enormous release of energy. As with hot stars, measurements in the ultraviolet and perhaps also in the infrared are likely to give information on gas ejection not obtainable in visible wavelengths; the structure of the emitting region and of the expanding gas can be examined with high-resolution images of the closer galaxies. A large space telescope, of 120-inch aperture, would be particularly useful for extending such research to more varied as well as more distant objects, and should contribute very significantly in this and other ways to our understanding of the Universe.

### Flight Requirements

The flight requirements for the scientific program envisaged by he Panel on Optical Astronomy are summarized immediately below and in tables 1 and 2.

#### *Airplanes and Balloons*

(*a*) Infrared telescope, with broadband photometry, and an nterferometer-type spectrophotometer. On the minimum program this should be a 36-inch telescope; on the maximum program a much larger instrument, up to 120 inches, should be provided. On both levels the programs should be balanced between large platform flights, such as available with the NASA Convair 990, and smaller, single experiment, flights, such as the Lear Jet.

(*b*) High-resolution visible telescope (Stratoscope).

#### *Sounding Rockets (20–50 lb)*

(*a*) Stellar UV spectrographs, 1-Å resolution, until OAO–C and later instruments are available: for studies of stellar atmospheres and for interstellar absorption lines. Possibly a standby for a bright comet.

(*b*) Far-IR scan of some regions of the sky, with broadband photometers: for measurement of emission by interstellar dust grains, and for detection of sources whose emission peaks at wavelengths greater than 20 $\mu$m.

(*c*) Miscellaneous small-scale instrumentation, to be tested for later flights.

#### *Small Satellites (100–200 lb)*

(*a*) Broadband UV photometer and polarimeter: for measurement of selective extinction and polarization by grains and for measurements of variable stars and galaxies; also solar-system objects.

(*b*) UV sky survey: for measurement of UV emission from interstellar gas, preferably at several wavelengths.

(*c*) IR telescope, refrigerated, with interferometer-type spectrophotometer: for spectral measurement of IR emission from planets, stars, and from gas and grains, primarily at wavelengths between 10 and 200 $\mu$. The feasibility and effectiveness of such an instrument depend critically on whether a suitable lightweight, low-power cryogenics system can be developed. (See report of Infrared Panel for a more detailed discussion.)

TABLE 1.—*Optical Space Astronomy—Maximum Program Above Which There Would Not Be Enough Good Groups of Scientists To Carry Out The Program*

| Project | 1971 | 1972 | 1973 | 1974 | 1975 | 1976 | 1977 | 1978 | 1979 | 1980 | 1981 | 1982 | 1983 | 1984 | 1985 |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Rockets | 15 | 17 | 20 | 22 | 24 | 24 | 24 | 24 | 24 | 24 | 24 | 24 | 24 | 24 | 24 |
| Explorers | ----- | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 1 |
| Observatory class | ----- | D | E | F | G | H | I | J | ----- | ----- | ----- | ----- | ----- | ----- | ----- |
| Next step beyond observatories—possibly man attended. | ----- | ----- | ----- | ----- | ----- | ASTRA A | ----- | ASTRA B | ----- | ASTRA C | ----- | ASTRA D | ----- | ----- | ----- |
| Ultimately desirable instrument(s). | LST start ——————→ LST launch | | | | | | | | | | | ----- | ----- | ----- | ----- |
| Other needs:<br>IR airplane | 1 continuously, with size and scope depending on funds available—maximum up to 120-inch telescope. | | | | | | | | | | | | | | |

TABLE 2.—*Optical Space Astronomy—Minimum Program Below Which Good Groups of Scientists Will Not Remain in the Field*

| Project | 1971 | 1972 | 1973 | 1974 | 1975 | 1976 | 1977 | 1978 | 1979 | 1980 | 1981 | 1982 | 1983 | 1984 | 1985 |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Rockets | 12 | 12 | 12 | 12 | 12 | 12 | 12 | 12 | 12 | 12 | 12 | 12 | 12 | 12 | 12 |
| Explorers | ----- | 1 | ----- | ----- | 1 | ----- | ----- | 1 | ----- | ----- | 1 | ----- | ----- | 1 | ----- |
| Observatory class | ----- | D | ----- | E | ----- | ----- | F | ----- | ----- | G | ----- | ----- | ----- | ----- | ----- |
| Next step beyond observatories—possibly man attended. | ----- | ----- | ----- | ----- | ----- | ----- | ----- | ASTRA<br>A | ----- | ----- | ASTRA<br>B | ----- | ----- | ----- | ----- |
| Ultimately desirable instrument(s). | ----- | ----- | ----- | ----- | ----- | LST start ——— launch in 1986→ | | | | | | | | | |
| Other needs:<br>IR airplane | 1 continuously; fly best available platform | | | | | | | | | | | | | | |

### *OAO-Size Telescopes*

OAO–B—Goddard: UV scanning spectrometer, instrumental width 2 Å or wider, for spectrophotometry of stars and emission nebulae.

OAO–C—Princeton: UV scanning spectrometer, instrumental width 0.1 Å for measurement of interstellar absorption lines in stars down to 6th mag. Stellar spectra with instrumental width of 0.1 Å or 0.4 Å.

OAO–D—National facility: UV scanning spectrometer with offset guidance, instrumental width 0.3 to 0.5 Å, or more, for measurement of stellar spectra, including interstellar absorption lines down to 8th mag; at 40-Å resolution, stars at mag 13 can be reached.

OAO—National facility: Wideband UV spectrophotometer and polarimeter, with offset guidance. E and H in maximum program omitted in minimum, for research on variable stars, interstellar grains, and galaxies.

OAO—National facility: UV echelle spectrometer, with integrating television tube, instrumental width 0.1 Å, for measurement of stellar spectra down to 9th mag; also low dispersion capability, 1 Å down to 12th magnitude and 100 Å to 18th magnitude (F and I in maximum program, E in minimum.)

OAO—National facility: IR interferometer-type spectrophotometer plus broadband IR photometer, used both for poin sources and for extended sources; development of lightweigh cryogenics refrigerator is required. (G and H in maximum program, F in minimum.)

OAO or ASTRA A—National facility: same UV echelle spectrometer as above, plus high-resolution imagery (limit stella mag 26 in visible with an image tube exposure time of severa hours) with broadband filters. (OAO—F or G in minimum program; ASTRA A, with primary mirror diameter between 40 and 60 inches, in maximum program.)

ASTRA B: same as A, with improved capability, and mor flexible instrumentation, including infrared capability. Manned maintenance possible.

### *Large Space Telescope*

Aperture 120 inches or more, with resolution corresponding t at least 120 inches.

Indefinite life with manned maintenance; alternatively launching of a completely unmanned such instrument every few year might provide comparable data.

Scientific instrumentation similar to ASTRA B, with perhaps higher spectral resolution also available, with resolving power of $3 \times 10^5$.

For additional details of the far-reaching and fundamental scientific program with this instrument, see LST Report (Space Science Board, 1969).

## STARS

Past experience has shown that each new advance in observing technique or in spectral region made available has yielded completely unexpected astrophysical effects or interesting objects: very recent examples of these are the pulsars, mass-ejecting supergiants, infrared stars, and X-ray sources. However, in newly available spectral regions, experiments are initially planned to answer questions raised by previous observations or theories. Given below are six examples of such unanswered questions that can perhaps be settled during the coming decade with the space vehicles currently envisioned. Other projects, such as measures of double stars and proper motions with high spatial resolution, may be added later.

(1) Detailed studies of individual stars should be preceded by a continuing survey, with moderate spectral dispersion, of many bright stars to (1) determine the general characteristics of the ultraviolet spectra of normal stars of various temperatures, radii, ages, compositions, or other obvious physical parameters; (2) discover stars whose ultraviolet spectra seem to be unusual; (3) survey stars whose gross ultraviolet photometric characteristics seem unusual as found from the OAO–2 measurements; and (4) survey stars known from their visual spectra to be abnormal, such as the peculiar A-type (magnetic) stars, T Tauri stars, pulsating stars, and symbiotic stars. Such a long-range survey is similar to those of the Henry Draper Catalog or the two-dimensional MK classifications; it might in time involve thousands of stars and could be conducted best with spectrum scanning equipment like that planned for OAO–B and OAO–D.

(2) Recent observations from Aerobee rockets gave the unexpected result that some massive stars have doppler-shifted absorption lines in their ultraviolet spectra, indicating that they are ejecting material with speeds far in excess of their velocities of escape. The rate at which they lose material is enough to affect significantly their later evolution. What kinds of stars exhibit this behavior and what is the physical mechanism that drives away their outer layers? To answer these questions will require (1)

from space, line profiles obtained from high-resolution (0.2-Å) ultraviolet spectra of at least several dozen representative stars, and (2) from ground-based observatories for the same stars, high-resolution spectra of their visual regions that sample lower levels in their atmospheres. Both sets of concurrent observations should be repeated frequently during roughly 1 to 2 years to search for time-dependent fluctuations in the loss of material. The initial ultraviolet survey of bright stars can be accomplished with a moderate-sized telescope and spectrograph similar to the OAO–C. Later studies of similar stars in more distant galaxies, such as the Magellanic clouds and Messier 31, to determine whether this behavior is a universal characteristic of massive stars, will require many hours of observing time with a space telescope of much larger light-collecting power, such as one of 120-inch aperture.

(3) Current theories on the formation of the chemical elements in the initial gas from which galaxies were formed ("big bang" theory) and in stellar interiors themselves start with proton-proton reactions in hydrogen to form helium and then interactions between helium nuclei (alpha particles) to form heavier elements up to iron and the other metals, passing first through carbon, oxygen, neon, magnesium, silicon, etc. An evaluation of the abundance of the latter elements is crucial to an understanding of how stars evolve and what occurred during the first few minutes of the universe.

Various spectral lines of these elements are observed in the visual regions of hot stars. However, all these lines originate from highly excited states of those elements, and an extrapolation to the much higher numbers of atoms existing in the lower states depends very sensitively on the temperatures of these stars. To improve drastically our determinations of stellar abundances will require both of the following: (*a*) Observing the lines originating from the low-lying levels; all these are in the ultraviolet region, mostly between 500 and 3000 Å. (*b*) Obtaining better stellar temperatures; since hot stars have the bulk of their energy output in the ultraviolet, it is necessary to observe that spectral region to obtain good temperatures. The abundance analyses will initially require high-resolution (0.1 to 0.2 Å) spectra of at least a few dozen representative stars. Over a period of time, special problems in the field would require similar data for many more stars. Most of these spectra can be obtained with a moderate-sized (e.g., 36-inch) telescope, such as the OAO–C. Certain crucial stars, such as those of population II and other cool stars, are fainter in the ultraviolet and will have to await the completion of telescopes of larger light-gathering power, such as a 120 inch. The temperature determinations will require low-resolution photometric measures, such as are being carried out with OAO–A2, but again have to be supplemented by spectra of appreciable resolution (about 0.5 Å) to determine what fraction of the radiation in the broadband filter passbands is absorbed in spectral lines.

(4) Above the photosphere of the Sun are the complex chromosphere and corona: low-density high-temperature regions that are heated by sound or hydromagnetic waves originating from below the atmosphere. The corona is important because it gives indirect information about the solar convection that produces the waves responsible for the heating and because it is the origin or transmitter of the solar wind that, in turn, has such subtle but far-reaching effects on the planets. We can examine certain aspects of the solar chromosphere from the ground in two ways: (*a*) by spectroscopic observations at the solar limb, and (*b*) by observations in the centers of the calcium H and K lines because their opacities prevent seeing deeper into the star than the chromosphere.

There is ground-based evidence, again from the calcium lines, for the existence of chromospheres around most or all cool stars, and theoretical conclusion as to why these chromospheres should exist. The chromospheres apparently contain magnetic fields that decay slowly with time, and the chromospheres are also turbulent (or in some more organized form of motion) with speeds that increase with the size or intrinsic brightness of the stars. But to learn more about stellar chromospheric physics, motions, and evolution, we must observe more than just the H and K spectral lines. The ultraviolet spectrum of the Sun observed initially with rockets shows a rich spectrum of chromospheric and coronal lines. Preliminary surveys of various types of stars of differing ages, magnetic field strengths, and composition should be made with moderate-dispersion spectrographs, such as those planned for OAO–B and OAO–D; for a better understanding of the physical processes involved, we require high-resolution spectra of such stars to obtain line profiles and line strengths. Also, we wish to continue such observations over short time intervals, to search for cyclical variations (capable of yielding rotation periods) in chromospheric line strengths produced by extensive plage regions being carried across the disk by rotation, and over intervals of a decade or more to see whether other stars exhibit sunspot cycles (which in the case of the Sun is correlated with such important phenomena as changes in the solar wind, radio transmission, auroral intensities, and perhaps biological effects on the Earth). For observations of emission lines, intermediate dispersions should be adequate (between 0.5 and 2 Å); hence such programs can be started with telescopes like the OAO–B or D; however, a 10-year sequence of such observations would require either a closely spaced sequence of OAO's or man-maintained capabilities for moderate-sized telescopes.

It would be interesting to look at the chromospheric lines as a function of phase in the spectra of certain pulsating stars, such as the Cepheids, because those lines may show the effects (line doubling) of shock waves moving outward from the atmospheres; similar pulsating stars (population II Cepheids) of lower surface gravity are already known to exhibit these effects in their atmospheres. Finally, it is hoped that a study of the chromospheres around other stars will help explain the cause and nature of the solar chromosphere.

(5) Supernovae are energetic explosions that may seriously affect their surrounding regions and objects, particularly if their maximum energy output is in the ultraviolet. Supernovae are so infrequent per galaxy (about 1 per 100 years) that most of our knowledge about them is, and probably will be for some time, obtained from the apparently faint examples in distant galaxies. Such supernovae are discovered at the rate of several per year if there is suitable ground-based surveillance. To obtain spectra of 10- to 15th-magnitude supernovae will require telescopes of about 36-inch aperture or more, such as OAO–D; since the spectra have very broad lines, low spectral resolution is sufficient.

(6) Recent ground-based infrared observations through atmospheric windows have shown that some stars emit unexpectedly large amounts of infrared radiation, perhaps due to cool circumstellar envelopes of partially ejected material. The determination of the frequency of such stars and the physical conditions in their envelopes will require observations over a wide range of infrared wavelengths. Initial spectrophotometric measures of bright stars can be made with the NASA 990 airplane and 36-inch infrared telescope; fainter objects must be studied with OAO– or LST–type instruments.

## INTERSTELLAR MATTER

The study of interstellar matter covers a wide range of topics and problems, including circumstellar envelopes around stars, planetary nebulae, clouds of gas and dust in spiral arms of galaxies, and the still hypothetical intergalactic gas. This diffuse matter plays an important role in the evolution of galaxies and the universe. Interstellar gas condenses to form stars, becomes enriched in heavy elements deep inside the stars, and is then ejected back into space. The physical processes which occur in the enormously rarefied medium between the stars are not well understood, and are in many cases very difficult to study under laboratory conditions. For example, dynamical effects in ionized gases in the presence of a strong magnetic field, and with energetic particles also present, are of central importance both in controlled fusion research and in the understanding of interstellar gas problems. Thus increased understanding of this complex field could be of vast practical importance to man's welfare.

Some aspects of interstellar matter research can be enormously advanced by observations from above the atmosphere. Spectrophotometry in the ultraviolet, and in the infrared, and high spatial resolution is visible and other wavelengths all promise to yield information that could not be obtained in any other way. Some specific research programs that could be carried out in these various areas are described below.

### Ultraviolet Absorption Lines

High-resolution observations of interstellar absorption lines in visible light (Münch, 1968) have indicated that Ca$^+$ and Na atoms are distributed in clouds with differing space velocities, ranging in a few cases up to 100 km/sec; preliminary information has also been obtained on the chemical composition of the interstellar gas.

All those programs suffer from the very small number of observable lines. While some 15 atomic lines and almost as many molecular lines have been measured, only the Na D lines and the K and H lines of Ca$^+$ are sufficiently strong for systematic measurements in many stars. Interstellar atoms and molecules are almost always in the ground state, and the energy of the first excited state to which a radiative transition is permitted is generally more than 4 eV above the ground state. Hence the interstellar absorption lines of most elements, including all the abundant ones, lie shortward of 3000 Å. The ability to detect and measure such ultraviolet lines would essentially open up a new field of research. With present techniques, about 10$^{-5}$ gm/cm$^2$ of gas with normal composition are required to produce appreciable absorption or emission in 21-cm radiation (about 10$^{19}$ H atoms/cm$^2$) or in the K, H, or D lines. However, strong absorption in the resonance O I line at 1302 Å will be produced by 10$^{-8}$ gm/$^2$ of gas with normal cosmic composition (about $10^{13}$ atom/cm$^2$ of O); the high abundance of oxygen atoms and their concentration in the neutral state account for this great increase in absorbing power. Thus for detecting interstellar matter, an ultraviolet space spectrograph is more sensitive by 3 orders of magnitude than a similar instrument on Earth's surface.

This huge increase in sensitivity can be used to advantage in a variety of ways. Absorption by the gas in regions of very low density can be detected, provided either that the doppler shift is sufficient to separate the resulting absorption lines from the stronger features produced by regions of higher density in the line of sight, or that the composition or state of ionization in the low-density region is sufficiently different so that the lines produced are not absorbed in high-density regions. Clearly, the number of measurable lines in the ultraviolet should much exceed that in the visible.

Some progress in this area can be made with only moderate resolution. Ultraviolet spectra obtained with sounding rockets, with a resolution of about 1 Å (resolving power of about 1000), have already indicated that the local density of neutral H is almost an order of magnitude less than normally assumed (Jenkins, 1969). Preliminary measures of absorption lines of O I, C II, Si II, and Al II would, if confirmed, also require substantial modifications in our accepted picture of the interstellar gas (Stone and Morton, 1967). To realize the full potentialities of this subject, however, a substantial increase in spectral resolving power seems required. To resolve interstellar from stellar lines, a resolving power of at least 3000 is required, if the star in question has a rotational velocity in the line of sight of several hundred km/sec; for more slowly rotating stars, a resolving power of some 30 000 may be needed, almost as great as was used by Adams in his classical study of the interstellar K and H lines. For special purposes, such as detecting the very faintest lines or resolving closely adjacent components, a resolving power as great as 300 000 would be useful.

Scientific questions that could be answered with measurements on ultraviolet absorption lines have been discussed by Spitzer and Zabriskie (1959) and more recently in the LST Report (Space Science Board, 1969). Some of the most interesting of these are outlined below.

(1) *Abundance of $H_2$.*—Molecular hydrogen may be an important constituent of the interstellar gas, particularly in dense clouds. Even if the relative abundance is low, such molecules are

important cooling agents, and would affect any shock wave in the gas. The strong Lyman band extending shortward of 1108 Å contains many lines that could be measured in distant hot stars, if the ratio of $H_2$ to H is some $10^{-6}$ or greater. The relative strengths of different bands would give information on the kinetic temperature and density in the regions containing $H_2$, and, perhaps, on the abundance of deuterium relative to hydrogen—an important clue to the nucleogenetic history of the gas. (Continuous absorption by $H_2^+$, peaking at about 1000 Å, might be observable by accurate broadband photometry.)

(2) *Electron density.*—At the moment, there is no very reliable determination of $n_e$ in individual H I regions, although data from pulsars are beginning to yield important information on the mean $n_e$. Measurements of the Mg I line at 2852 and of the adjacent Mg II doublet could give the ratio of Mg I to Mg II atoms. From the known flux of ultraviolet radiation and the atomic cross sections involved, the electron density $n_e$ could be computed. Similar measures could be made with neutral, singly ionized and multiply ionized lines of C, S, and Si, giving important cross-checks.

(3) *Abundance of interstellar molecules.*—The vibrational transitions of polyatomic molecules such as $NH_3$, $CH_4$, $H_2O$ in the infrared all have very small transition probabilities. The strong electronic transitions all lie in the ultraviolet ($\sim$1500 Å), and provide much more sensitive tests for the presence of these molecules. The only strong lines of diatomic molecules such as CO, $N_2$, $O_2$ also fall in the ultraviolet.

(4) *Relative abundances of the elements.*—With a wide variety of lines accessible from many elements, the chemical composition of the absorbing clouds could be determined. Possible variations of this composition with position may be anticipated, depending, perhaps, on the rate of mass ejection in the neighborhood from supernovae and giant stars. Abundance anomalies in the gas, due to most atoms being "locked up" in the solid particle or grains, could also be explored; measures on the titanium and calcium lines suggest the possible importance of this effect.

(5) *Physical nature of the gas outside spiral arms.*—With the greatly enhanced sensitivity of ultraviolet spectroscopic measures, one could investigate the physical properties of gas between the spiral arms and in the galactic halo or corona. Differential galactic rotation and possibly also increased turbulence would provide doppler shifts needed for distinguishing the absorption in these regions. In addition, such highly ionized atoms as C IV, N V, and

O VI might be present in these low-density regions and could be detected.

### Emission Lines

Our relatively extensive knowledge of planetary nebulae is almost entirely derived from a study of the emission lines in their spectra. Similar information (Aller and Liller, 1968) is obtainable from diffuse nebulae, though these have been less thoroughly explored because of their lower density and diminished surface brightness. In particular, the radial velocities of the gas have been extensively measured, the kinetic temperature and electron density evaluated, and the chemical composition determined. Extending these measures to the ultraviolet might offer some new surprises, though lines excited by electron impact at an electron temperature of some 10 000° will tend to be relatively much weaker in the ultraviolet than in the visible. The very steep dependence of ultraviolet emission line intensities on electron temperature might make such lines well suited for temperature determinations.

Measurement of the L$\alpha$ emission line from H II regions and planetary nebulae might be of particular interest. Photons of this radiation escape primarily in the doppler wings, and may reach the Earth without absorption by the intervening hydrogen gas. The amount of energy escaping in this way is relatively large; measurement of the total flux and of the line profile should give important information on physical conditions in and immediately surrounding these hot ionized regions.

Emission lines in the far-infrared region of the spectrum would give entirely new information. It is believed that most of the energy input into H I regions is radiated in forbidden lines of C II, Si II, and other elements which have an excited state within less than 0.1 eV of the ground state. Spectrophotometry in the region between 5 $\mu$m and 500 $\mu$m is technically feasible and could yield important new results on the temperature, density, composition, and physical state of H I regions. Considerable research in this subject can be done from the ground, at wavelengths where the water vapor in the Earth's atmosphere is not too absorbing, and more can be done from balloons and high-flying aircraft. It seems likely that useful results from above the atmosphere could be obtained with relatively modest spatial and spectral resolution. For extended sources a large-aperture telescope is not needed, and unless the atomic lines are blended with many strong molecular emission lines, high spectral resolution is also unnecessary for many purposes.

The specific emission lines that may be anticipated and the scientific questions that could be answered with such observations have been discussed by Osterbrock (1968) and Pottasch (1968). A brief listing of some of the most significant problems follows:

(1) *Kinetic temperature and density in H I regions.*—This information, which would supplement that obtainable from ultraviolet absorption lines, would give additional knowledge on the possible presence of dense, hotter regions which might not be seen in the absorption-line observations. Collisions between clouds could produce such regions.

(2) *Energy input.*—A measurement of the total radiated energy (outside transient high-density regions produced by shocks) would give a value for the energy input, and thus yield an upper limit for the amount of heating by suprathermal particles, which have been proposed as the chief heating agent in H I regions.

(3) *Chemical composition.*—While measurement of scarce elements is probably more difficult in the far infrared than in the ultraviolet, measures of infrared emission lines could detect $H_2$, if this is present with an abundance of $10^{-3}$ times that of H, and could measure the amount of many other species as well.

### Optical Effects of Grains

The small solid particles, or grains, in interstellar space make their presence known by absorbing, scattering and reemitting radiant energy from the stars (Greenberg, 1968). These particles affect the temperature, the dynamics, and perhaps also the composition of the gas, and are believed to play a significant part in star formation. The polarization which they produce provided one of the first observational indications for the presence of a magnetic field, and yields detailed information on the topography of this field.

There are two primary ways in which observations from space would be particularly crucial for the study of grains. Firstly, an extension of the selective extinction data farther into the infrared and into the ultraviolet would delineate the distribution of grain sizes that must be assumed. In addition, such measures might give some information on composition, especially if the detailed dependence of extinction on wavelength shows features attributable to resonances in the optical properties of the grain material. Preliminary measures in the infrared, obtained from the ground, indicate that in some regions the selective extinction in this region is higher than expected, indicating more of the larger particles than had previously been assumed. Similarly, measures in the ultraviolet, obtained with sounding rockets, indicate a gradual increase of selective extinction with increasing frequency, suggesting a greater number of relatively small particles than had previously been thought likely. Knowledge of the complete distribution of particle sizes is required if the physical factors involved in particle growth and destruction are to be understood. In addition, differences in the shape of the extinction curve between different regions, suggested by measures in visible light, might well be substantially enhanced in the ultraviolet and infrared, giving information on the differences of grain-size distribution between different regions. Measurement of the polarization at ultraviolet and infrared wavelengths would also give significant information on grain shapes and sizes.

Second, a measurement of the radiant energy emitted by the grains, at wavelengths in the far infrared, would give directly information on the coalbedo of the grains, since the amount of energy incident on the grains is known, at least approximately. Since the albedo is probably almost unity and quite uncertain in any case, the coalbedo is at present conjectural. Knowledge of the coalbedo should help to distinguish between different possibilities for the composition of the grains. The distribution of the emitted energy with wavelength should give information both on the temperature of the grains and on their composition.

These programs can be advanced very significantly with telescopes of relatively small apertures, just as on the Earth's surface most observational research on interstellar extinction has been carried out with the smaller instruments. For some programs, such as the distribution of grains in remote regions of our galaxy or in other galactic systems, large apertures might be necessary. It is not clear that infrared extinction measures particularly require a space telescope, though a large aperture is needed for collection of stellar photons. The emission measures in the far infrared could doubtless be carried out with high-flying aircraft, balloons, or, at some wavelengths, with sounding rockets; again a rather small telescope would suffice for the low spatial resolution appropriate to such exploratory investigations.

### High Spatial Resolution

Our knowledge of the spatial structure of nebulae is based on photographs taken under the usual seeing conditions on the Earth's surface, with resolutions of about 0.3 to 1.0 arcsec. Thus a 40-inch diffraction-limited telescope, with a resolution of about 0.1 arcsec in visible light, would greatly increase our knowledge of nebular structural details; the Large Space Telescope, with a resolution in visible light of about 0.03 arcsec, would permit further improvement in the data.

While it is difficult to indicate in advance what results would be obtained from high-resolution images, a number of interesting problems can be suggested. Expanding shells of gas, ranging from planetary nebulae up to supernova shells, tend to show fascinating detail, including complex filamentary structure. Some of this detail may be related to the presence of a magnetic field; some may result from a thermal instability. Pictures with higher resolution might help to decide between different theoretical possibilities. Pictures in dense clouds, where star formation is believed to occur, might give valuable clues as to how star formation takes place and how planetary systems such as our own have formed. For example, in the Lagoon and Orion nebulae, objects about 100 A.U. across could be resolved by a 40-inch diffraction-limited 40-inch telescope, permitting the resolution of individual protostars. In addition, the nature of such dark objects as globules and elephant trunks might also be clarified by high-resolution pictures.

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## EXTRAGALACTIC ASTRONOMY AND COSMOLOGY

Even in ground-based astronomy, where large telescopes have been available for many decades, useful extragalactic research continues to be done with intermediate-class instruments in the 36- to 48-inch range. These applications include in particular broadband photometry, spectrophotometry, measurement of radial velocities, and discovery of unusual emission-line spectra. For space telescopes of such intermediate size, the extended wavelength range, improved resolution, and dark sky promise important advances over ground-based instruments.

The extragalactic problems accessible to intermediate-class space telescopes fall into two categories: those tolerating pointing accuracy sufficient only for acquisition and rough guidance (say 10 to 100 arcsec), and those requiring fine guidance (1 to 0.1 arcsec).

In the first category falls the extremely important question of UV and IR spectral energy distribution of galaxies and quasars. Broadband and intermediate-band photometry (1000- to 100-Å resolution) can be done on the brighter nearby galaxies ($m \sim 5$–10) of large angular diameter with very crude resolution and guidance, and on the larger galaxies out to about $10^7$ parsecs with guidance still as poor as about 0.1 arcmin. Such photometry will be definitive of the total optical spectral emission of nearby galaxies, and should give more information about the stellar populations and help to determine any contributions from nonstellar sources of light.

Direct contribution to cosmological problems of the highest interest begins with the ability to do accurate spectrophotometry on selected galaxies of intermediate distance ($10^7$ to $10^8$ parsecs), work which also requires guidance at least in the 10-arcsec range. Interpretation of the directly observed Hubble parameter ($H_0$) and deceleration parameter ($g_0$) requires application of photometric corrections for red shift ($K$-correction) and for evolutionary effects. Sandage and other astronomers using large ground-based telescopes have established, by careful stepping techniques, $K$-corrections and evolutionary corrections which should be adequate to permit determination of $H_0$ to at least 10 percent accuracy and of $g_0$ to somewhat better than sign. But observations over the widest possible wavelength range to the greatest possible distance should give additional confidence in the accuracy of the corrections and help to reveal any unsuspected effects such as large-scale departures from chemical homogeneity.

Work by Low and others shows that some active galaxies and at least a few quasars emit most of their light in the infrared. Complete IR spectra, from 2 to 1000 $\mu$m, even of the very lowest resolution, are likely to be of greatest interest in finding the origin of this phenomenon and understanding its role in galaxy evolution. Airplane observations should be pushed very hard over the next several years to establish whether substantial satellite IR systems are needed to carry out this program.

But apart from the studies mentioned above, most interesting extragalactic work requires somewhat higher spectral resolution, and a guidance precision better than 1 arcsec. Ultraviolet spectra, in the 1- to 100-Å range of resolution, offer exciting possibilities. To mention only three examples: quasar red shifts often depend on only one spectral line—indeed, a number of apparent quasars have no identifiable lines in the wavelength regions so far studied. While ground-based observations in the red and infrared can resolve many of the ambiguities, the 1000- to 3000-Å region may well be crucial in important cases, and should help in removing suspicion of observational bias in such important relations as number versus red shift. At low spectral resolution, 18th-magnitude objects should still be readily accessible to a 40-inch space telescope, if an integrating image tube is used.

Detection of intergalactic absorption lines may be possible in the far UV; in particular, even a relatively modest telescope-spectrograph combination should be able to test for intergalactic hydrogen, carbon, oxygen, and nitrogen, using nearby as well as more distant galaxies; a much farther-out possibility would be the direct detection of He absorption or emission in objects of great red shift ($z > 3$).

As a third possibility, best (or perhaps only) open to space telescopes, we have come to expect large cosmic velocities always to be of the same sign, that of recession. Burbidge has pointed out that substantial blue shifts would be hard to detect from the ground, since most if not all of the expected strong lines would be shifted well into the ultraviolet below 3000 Å. So far there is no evidence that the universe contains any spectral-line objects having large blue shifts with respect to our galaxy, but it would be a useful precaution to check otherwise featureless spectra for this possibility.

The other general area of interest for high-performance systems lies in diffraction-limited imagery. Here again, most extragalactic studies of objects bright enough to be recorded in reasonable times would profit from routine availability of 0.1-arcsec resolution. Such programs would include morphology of galaxies, photometry of clusters, of H II regions, and of very bright individual stars in galaxies somewhat beyond the local cluster, the filamentary structure of peculiar objects such as the exploding galaxy M82 and the jet of M87, the partial resolution of nuclear regions of active galaxies, and perhaps of the fuzzy appendages of at least the brighter quasars.

In particular, with a telescope of 40-inch-aperture galactic nuclei of about the 21st magnitude, with a starlike core smaller than 0.1 arcsec in diameter, could be recorded with an integrating image tube in an exposure of about an hour. For such an exposure the diameter of the first diffraction ring should appreciably exceed the size of the resolution element on the image tube photocathode, if the full resolution permitted by the optical system is to be utilized. The structure of nuclei having angular diameters of 0.5 arcsec and brighter than the 18th magnitude would be resolved in 1-hour exposures. Thus the structure of dozens of galactic nuclei could be examined, and at least upper limits for their diameters. In view of the apparent importance of compact nuclei in galactic evolution, a study of the star densities existing in these objects would be very important.

For detection of faint stars, the size of image could be reduced so that the diameter of the first diffraction ring was about equal to the resolution element of the image tube. With the greater concentration of light in star images, and the threefold darker night sky, detection of stars down to the 26th apparent magnitude should be possible with a 40-inch diffraction-limited orbiting telescope; such performance would make it profitable to check "empty" regions of accurately known coordinates of strong radio sources, pulsars, and perhaps some X-ray sources.

However, extragalactic research, because of the extreme distance and apparent faintness of most of the objects which are important to study, requires for major progress the development of the largest feasible space telescopes, together with ground-based instruments also of the largest feasible size. Thus with the resolving power of a 120-inch aperture, stars of the 28th apparent magnitude can be studied, with detection possible in principle even at magnitude 29. Research on galaxies that could be carried out with such a powerful instrument is discussed in the report of the Large Space Telescope Committee (Space Science Board, 1969).

## PLANETS, SATELLITES, ASTEROIDS, AND COMETS

### Introduction

The overall objective to understand the origin, evolution, and future of the solar system by optical study of the planets, satellites, asteroids, and comets includes the following programs:

(1) *The physical structure and chemical composition of planetary and satellite atmospheres, and their variations.*—Data desired are: temperature, pressure, chemical composition, ionic

composition, aerosols, circulation, turbulence and energy transfer of the atmospheres all as functions of altitude, latitude, phase angle, time of day, season, solar activity, cloudy areas, local surface activities and interaction with the solar wind.

(2) *Planetary, satellite, and asteroidal surfaces, and near-surface structure.*—Chemical and physical structure and time variations in surface radiation over large phase angles and over all frequency bands including inherent and induced radiations of general or local origin. Evidence of local tectonic disturbances on Mars or Mercury would be of extreme interest. Photometric and polarimetric data over all possible spectral regions are critical to knowledge about inaccessible surfaces.

(3) *Comets.*—Physical and chemical nature of the nucleus, physical processes in the coma and tails (ion and dust), and interaction with solar radiation and the solar wind.

The invaluable results from planetary probes are so specialized that no attempt is made here to incorporate their optical potentials into this discussion.

Photometry of the asteroids, satellites, the rings of Saturn, and comet dust tails for albedo, polarization, phase function, and light variations in the ultraviolet, combined with similar and simultaneous photometry in the visual and the infrared will add vital information concerning the nature of the surfaces of these bodies. Such observations can be conducted with instruments of aperture in the range 0.5 to 1.0 meter on Explorers and OAO's, and with increased efficiency at greater apertures.

The rings of Saturn are still poorly understood with regard to origin, composition, structure variations, and ultimate stability.

### Infrared Observations

Until lower weight, more efficient, cryogenic techniques become available, infrared studies can be made in space only by rather massive, probably manned orbiting telescopes. The need for infrared observations from satellites will depend upon the results obtained from the extremely important observations that can be made from balloons, high-flying aircraft, and rockets. Possibly the need will emerge for a small astronomical satellite carrying a relatively short-lived cryogenic infrared telescope.

(1) *The energy balance of the major planets.*—One of the important current problems in planetary astronomy is to determine the proportion of energy emitted by the major planets which is derived from solar radiation, as opposed to that which derives from internal sources. The measures by Low appear to establish

the existence of important internal energy sources, but a definitive treatment of the central problem requires the measurement of the radiant energy distribution over large ranges of wavelength and phase angle, including both polar and equatorial limb variations. A definitive solution to this problem may require a flyby mission.

(2) *The helium-hydrogen abundance ratio for the major planets.*—Our understanding of the processes which led to the formation of the solar system and of the chemical elements in general would be greatly advanced if the abundance ratio of helium to hydrogen were known for Jupiter. The most direct method for determining this ratio by remote sensing is from the shape of the emission spectrum, which is also needed for establishing the thermal balance, as described in the preceding paragraph. Model-atmosphere calculations show that the color index defined in terms of intensities near 14 and 50 $\mu$m differs by 1 magnitude between a depleted atmosphere composed of pure hydrogen and one containing equal amounts of hydrogen and helium.

(3) *Temperature profiles in planetary atmospheres.*—The evaluation of local temperature profiles in planetary atmospheres can be made from high-resolution spectra in the strong bands of a major atmospheric constituent, such as the 15-$\mu$m band of $CO_2$. Airborne infrared techniques for these and similar high-resolution studies of planetary spectra should be pushed to their limit.

(4) *Infrared absorption by $CO_2$ and $H_2O$ in planetary atmospheres.*—The $CO_2$ and $H_2O$ absorption can be measured with great sensitivity at low dispersion in satellites. Other absorbers in the same range can be investigated.

(5) *High-resolution near-infrared spectroscopy.*—Such spectroscopy can be used not only for temperature and pressure profiles of a fixed planetary locality but also to give the topographical distribution of such profiles, thus providing information on elevation differences, meteorological conditions, sites of geothermal activity, etc., particularly for Mars. Such research is probably limited to large ground-based telescopes and possibly airborne telescopes until flybys or large space telescopes provide improved capabilities.

(6) *Low-resolution infrared spectroscopy.*—Such spectroscopy is needed to clarify the composition of the rings of Saturn. Efforts should be made to obtain more such information from airborne infrared spectrographs.

(7) *Radiometry in the thermal infrared.*—Infrared radiometry can provide good characterization of surface thermal inertias and

their lateral distribution in addition to some information on the vertical structure of the planetary subsurface and the variation of emissivity with position on the disks of Mars and Mercury. Higher resolutions probably obtainable from aircraft or balloons would be desirable for these purposes, but preliminary studies with such equipment should give important results and provide guidance for observations to be made from orbital telescopes. Similar statements as those above apply to studies of surface constituents which are nonuniformly distributed such as molecules outgassed from volcanos and fumaroles, molecules cycled through any biosphere, exposed deposits of surface minerals such as carbonates which have distinctive near-infrared absorptions, local frost deposits, etc.

A low-resolution experiment of interest would be a high-spatial-resolution scan of Mars in a particular $CO_2$ band to get variation in column density and hence in surface altitude.

(8) *Infrared polarimetry.*—Coupled with optical and ultraviolet measurements, infrared polarimetry can provide vital information on the real and imaginary parts of the surface refractive index, on particle sizes and on packing modes for the surfaces of Mercury, the satellites and asteroids, and possibly Mars and for the rings of Saturn and the dust clouds of comets. Such observations from aircraft are urgently needed and will determine further requirements for orbital infrared equipment.

### Spectral Analysis of the Planets

Here the problems concern the constituents of planetary atmospheres, their distribution over the surface of the disk of a planet, their variations with time, their distribution with altitude, and the concomitant temperatures measured in the atmosphere by means of band spectra. The extremely limited spectral studies of planets so far obtained with rocket spectrometers to a wavelength of 1800 Å provide exciting and mostly unresolved problems concerning the chemical constituents and the general emission characteristics of Jupiter's atmosphere. The planned OAO's should carry on this research to provide the basis for generally sound models of planetary atmospheres. The remaining problems that can be studied with high dispersion using a diffraction-limited telescope in the 1.0- to 1.5-meter aperture range will be determined on the basis of the results from earlier satellites. From past experience, one can predict with a high level of confidence that the problems uncovered by the lower dispersion instruments to be solved by instruments of higher resolving power will be more exciting and more significant in understanding planetary atmospheres, particularly those of the giant planets, than the problems that can be specifically delineated in our present state of ignorance. Continued work from balloons in the region of wavelengths greater than 2200 Å is highly desirable, but the need for spectra with the highest possible dispersion providing analysis of the finest details over planetary disks will remain a vital problem of the highest importance, possibly even for planets such as Mars and Venus where flybys will have made fairly intensive studies. As an example, the Connes' high-dispersion spectra of Venus showing traces of HF and HCl and even isotopes of Cl in the HCl give both the strong expectation of striking discoveries to be made by such techniques in the far ultraviolet with high dispersion equipment and also the basis for theoretical interpretations of profound significance in understanding the structure and processes in the atmospheres. Strong ultraviolet absorption by liquid droplets or solid particles may limit this spectroscopic technique to analysis of the outer atmosphere, through which the ultraviolet light can penetrate. For Jupiter, the isotopic composition constitutes a record in time of early stages in the formation of the solar system. The determination of the abundances of various isotopes in the major planets relative to H and $He^4$ would provide a valuable history of solar activity and its relevance to the formation of the planets. Again the spectra obtained with OAO's should clarify the expectations to be derived from larger instruments and higher spectral resolution. Clearly the continuing steps will be of immense value in unraveling the present and past circumstances in the solar system.

Ultraviolet spectral observations of planets from space are particularly needed for the analysis of those atmospheric constituents that are also present in the Earth's atmosphere, especially for $N_2$, $O_3$, and CO.

Ultraviolet spectra should be particularly valuable in the study of the satellites, specifically Titan and Rhea for Saturn and the four Galilean satellites of Jupiter. Their ultraviolet spectra as they come out of shadow into sunlight should be of particular interest. Preliminary work with the OAO's should establish the potentialities for diffraction-limited instruments of larger aperture.

In the region of the spectrum just on the long-wavelength side of the ionization continua of light atoms and molecules (1000–2000 Å), most of the light scattered from planets and satellites with atmospheres will be resonance radiation of the atoms and molecules of the upper atmosphere, typically from column depths of the order of $10^{18}$ atoms/cm$^2$ or less. Radiation from different species can be distinguished with moderate resolving power (range 10–100 Å) and relative abundances obtained. Special interest lies in measuring $N_2$, $O_2$, $H_2$, $NH_3$, $CH_4$, and their component atoms, particularly hydrogen by $L\alpha$.

## Comets

We can only speculate today about the reasons that the comets, in sharp contrast with the planets, are icy conglomerates of matter populating a region around the Sun with linear dimensions more than 2 orders of magnitude larger than that occupied by the planets. It has long been realized that this dichotomy of the state of aggregation of matter in the solar system holds important clues regarding the nature of the processes through which the solar system was formed. A comet approaching the inner regions of the solar system in a nearly parabolic orbit is presumably a sample left from the primordial matter which formed the solar system and, thus, the study of its composition and structure adds vital information concerning the physical and chemical environment when the solar system was very young.

It should be realized that a study in situ of a comet by means of a probe raises a variety of nearly insurmountable difficulties. Hence, for many years, we must rely on cometary data obtained from great distances and utilizing as much of the electromagnetic spectrum as possible from the far ultraviolet through the infrared. No radio or radar measures of comets are yet verified.

(1) *The chemical composition of the ices.*—The composition of cometary ices remains enigmatic. It is well known that the optical features of cometary spectra represent only a small fraction of all the molecular species that must be present. The spectrographic analysis of daughter and yet unknown parent molecules of observable compounds is critical to the understanding of cometary structure. Such an analysis would establish the balance between the gaseous and solid phases. The cometary molecules OH, CH, NH, and $NH_2$ should be traced to the parent solids, possibly $H_2O$, $CH_4$, $NH_3$, hydrates, and others, frozen in the nucleus, through molecules which have spectra inaccessible from ground. The knowledge of the parent solids will specify the temperature-pressure conditions of comet formation. The origin of nonhydrated compounds such as CN, $C_2$, $C_3$, and ions like $CO^+$, $CO_2^+$, should also be clarified. Do they have stable parent molecules in the solid phase or are they formed photochemically from the

hydrated ices? Of special importance will be the detection and measurement of the $H_2$ molecule emission. Detailed spectroscopic analysis of the ions and their motions near the nucleus will provide almost laboratory information on magnetohydrodynamics in specifying the interaction of the solar wind with cometary material. Ion production and molecular dissociation are poorly understood in comets.

Specific molecules such as OH, $H_2O$, $C_2H_2$, $NH_3$, $CH_4$, and other diatomic molecules and radicals may be expected to show emission in the infrared. Airborne spectroscopic studies are clearly indicated.

In the ultraviolet below a wavelength of 3000 Å, moderate dispersion spectra could show the resonance spectra of $H_2$, $N_2$, $O_2$, $NH^+$, $CN^+$, N, O, C, and other possible molecular and atomic species. Furthermore, the broader range of spectrophotometric observations will permit us to determine the abundances and life cycles of the many molecular species, most of them probably unobserved in cometary spectra today. All spectrographic equipment can, on occasion, give valuable results on comets.

(2) *The constituent solids and gases.*—Radiometer and spectrophotometer measurements in the infrared will provide brightness temperatures and energy-level populations, essential for understanding the photochemical processes described in the preceding paragraph. Improved determinations of the absolute size distributions among the solid particles and some information about their chemical composition can be obtained from photometric measures in "windows" between the molecular bands, both in the infrared and ultraviolet.

All opportunities to observe brighter comets in the infrared from balloons and aircraft, and in the ultraviolet from rockets, from OAO's, and from other satellites should be seized vigorously. Major problems may well be solved and new insights gained from the presently planned programs. An opportunistic attitude should be taken with regard to observations of comets from larger and more powerful optical instrumentation of the future.

## SUPPORTING RESEARCH AND TECHNOLOGY

It would seem that in a period when priorities must be carefully assessed, the SRT program should emphasize the solution of problems that are likely to be important within the next decade (i.e., between 5 and 15 years). The technological problems of diffraction-limited space telescopes with apertures between 40 and 120 inches are substantial enough; we have considered, in the following, primarily a collection of topics that deserve NASA attention because of their direct importance for such instruments. However, we recognize that still longer range research and technology tasks do and must exist, but point out that here we have deliberately avoided consideration of such issues. In addition, continued support of astrophysical theory, which is not discussed specifically here, is of fundamental importance for any program involving astronomical research.

### Optical Materials, Auxiliary Optical Elements

High-resolution photographs at very short wavelengths with a large reflector probably will require an auxiliary correcting lens or mirror systems of high performance and transmittance. The low transmittance of lithium fluoride below about 1100 Å makes the search for window material for far-ultraviolet work desirable, especially if it should appear that some observations below 911 Å are feasible. The development of mirror coatings having both improved reflectance in the ultraviolet and long lifetimes under spacecraft conditions should also be pursued, with, if necessary, the possibility being left open that they might be deposited or recoated after launch. The study of contamination or modification of optical systems in space should be continued: it is essential that the response of the system not change progressively or erratically with time as the result of film deposition on surfaces, or transmittance changes induced by the radiation environment in space, or temporary pollution of the vicinity by propellants or other material escaping from spacecraft. Attention should also be given to the smoothness of optical surfaces, especially those metallically coated, insofar as they contribute to the level of scattered—particularly ultraviolet—light in spectrographs.

Since spectral dispersion in most satellite ultraviolet spectrographs will continue to be provided by diffraction gratings, including echelles, it is essential that NASA insure the availability of plane gratings of first-class quality (with respect to freedom from ghosts and scattered light, high resolving power, high reflectance, and adequate blazing) and adequate size. Since replica gratings will often be used in space, it is necessary also to be certain of the long-term stability of the replicating layer with respect to a vacuum environment and continued thermal cycling. For the infrared wavelengths, interferometric spectroscopy is becoming increasingly important. Both high-resolution and low-resolution ruggedized interferometers should be developed for spacecraft use.

## Large Optical Elements

It is important to establish the aperture region above which it will be necessary actively to monitor and to control the figure of a large satellite telescope mirror in order to maintain diffraction-limited performance. Only when this limiting dimension has been determined can it be ascertained whether the complexities of active optics have to be faced at this time (in the philosophy of the first paragraph of this report). This decision may depend crucially upon the material proven to be most suitable for the primary mirror. It is thus of great importance to establish the performance and stability of new materials such as Cer-Vit and ULE, in both unitary and composite structures. Particularly important are the questions of long-term figure maintenance under repeated thermal cycling, and the ability to accept a perfect policy. It is obvious that an essential condition is the ability to manufacture under terresterial conditions a large mirror that will remain diffraction limited in a zero-g environment.

## Detectors

For the ultraviolet, continuing study and development of a varied array of integrating image-detecting devices is essential. Special attention should be given to high-efficiency image tubes and image converters having electrical readout. Particularly important in these devices is resolving power and photometric homogeneity across the field. The optimum tradeoff in photographic emulsions between high optical sensitivity, on the one hand, and low response to Van Allen belt radiation, on the other, should be investigated, and on this basis the usefulness of photographic detection in manned/unmanned spacecraft clearly defined. Once this position is established, the necessity or otherwise of special techniques to be superimposed upon the conventional photographic process—such as delayed sensitization, postexposure desensitization, or development, scan, digitization, and transmission to Earth—can be clearly spelled out.

For the infrared, continuing support should be given to the development of improved detectors, both bolometric and photoconductive. A critical adjunct to detector development is the development of the cryogenic systems capable of cooling detecting systems to liquid helium temperatures which can be operated for extended lifetimes on spacecraft with low power and weight requirements.

### Laboratory Astrophysics

The impending availability of great quantities of spectroscopic data from satellite spectrographs underlines the need for the laboratory information required to interpret it. The most obvious and immediate need is for adequate analyses of the optical spectra of the more abundant atoms and ions that lie in the space ultraviolet, and for the transition probabilities (and damping constants for the stronger lines) of as many lines as possible. Similar information will be required for a number of molecular species that occur in both stellar atmospheres and the interstellar medium. In the latter case, more information on the spectra of molecular ions is required, and there is a specially urgent need for more information on photoionization cross sections of the more abundant atoms and molecules; full allowance must be made for the complications of autoionization upon both ionization from the ground state, and recombination to all the excited states.

### Interrelationships With Ground-Based Astronomy

Ground-based astronomy will certainly profit directly from many of the instrumental techniques and technologies that are developed for space observations. It is to the advantage of both that, since the problems are so similar, the closeness of this relationship constantly be exploited. For example, opportunities thoroughly to test and debug space hardware on terrestrial telescopes should not be neglected.

The growing availability of satellite spectrophotometry on great numbers of faint stars for which the conventional optical parameters (multicolor data, radial velocity, accurate spectral classification) are unknown requires that the output of such supporting information by terrestrial observatories be increased in proportion. But such large routine programs will certainly not be undertaken unless NASA actively pressures and supports them, probably through new ground-based facilities.

The ability of ground-based astronomy to supplement and support the space-astronomy programs depends not only on the number and size of telescopes but also critically on the detectors available. In particular, work should be pushed toward the goal of developing imaging detectors of high quantum efficiency ($>$ 10 percent), large area ($>$ 10-cm diameter), wide spectral range (3000–12 000 Å), high resolution ($<$ 5 $\mu$m at 50 percent modulation transfer), uniform photometric and geometric field ($<$ 1 percent inhomogeneities), wide dynamic range (essentially linear over intensity ratios $> 10^2$), ready utilization with fast optical systems (faster than *f*/2), and capable of commercial production and wide distribution. It may prove that no single detector will satisfy all of these conditions, but even a good step in that direction would be equivalent, for many of the most important astronomical problems, to a severalfold increase in the number of large telescopes available.

*Report of the Infrared Astronomy Panel*
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# Infrared Space Astronomy

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## SCOPE AND DIRECTION OF INFRARED SPACE ASTRONOMY

Infrared studies promise new ways of studying the nature and structure of our galaxy and other galaxies. For the first time, using stellar radiation at 2 $\mu$m, we have observed the galactic nucleus. This shows that infrared radiation can adequately pierce the interstellar dust to enable studies of the distribution of stars through our galaxy. The extension of such work involves wide-field infrared observations which cannot be made from the ground because of fluctuating atmospheric emission. At 100 $\mu$m, the emission of the interstellar dust has been observed; it is an appreciable fraction of the luminosity of the galaxy. Further, observations of the galactic center at 10 $\mu$m show a component of emission that appears to peak toward longer wavelengths. Observations of the nuclei of some other galaxies show very large emission peaking toward longer wavelengths. These observations yield totally new kinds of information about our own and other galaxies.

The enormous infrared emission of some galactic nuclei and the possibly related large far-infrared emission of objects like planetary nebulae and of $\eta$ Carinae lead to a strong suspicion that some unknown physical process is taking place. Since a large fraction of the observed radiation of these objects is in the infrared, it is vital to understand the astrophysics of these processes both as a fundamental process and for its evolutionary significance.

These classes of observation are the major demonstrations that infrared astronomy above the atmosphere has unusually exciting contributions to make. However, other observations of perhaps lesser significance show that studies above the atmosphere may have wide scope. First, stars with infrared-emitting dust shells around them have been found and we can expect that far-infrared submillimeter wave versions of these objects will exist, without being able to predict their frequency, character, or full role in stellar evolution. Infrared line emission has been detected from a hot gas cloud; this promises that far-infrared studies may detect line emission from cool gas clouds, with all the wealth of interpretive detail that this could bring. Observations show excess infrared radiation from major planets with important implications about the structure and evolution of planetary interiors. Observations of total infrared background radiation are now yielding their first positive results. Theoretical studies ascribe cosmological significance to such observations.

These measurements have been made within the current limitations of infrared observation. However, the theoretical potential for detector improvement is enormous. The true potential beyond the current achievements will only appear as equipment is developed and put to use.

## LEVEL OF TECHNICAL EFFORTS

Infrared space astronomy involves a pyramid of scientific effort, with the foundation being ground-based astronomy. In observing from higher levels above the Earth, the range of interesting scientific problems will increase. However, despite the atmospheric problems at the lower levels, there is no sudden jump in capability because a given expenditure can buy more observing time and more powerful equipment.

Very little at all is known in infrared astronomy. Most of what has been learned has been gleaned from pitifully few ground observations. These observations were made in the main with telescopes so cheap that an entire installation cost less than one sounding rocket flight. To advance from this equipment and these observations to a satellite program without taking a substantial step in ground-based infrared would be scientifically and fiscally irresponsible.

One needs better techniques to take advantage of the higher altitudes. These require the research and the detector development that are being pressed for the lower altitudes. Thus, there has to be the pyramid structure of effort referred to.

A minimal program for the logical development of the subject over the next 5 years involves the following:

| | *5-year totals* |
|---|---|
| 1. Ground-based observation with current telescopes | 4000 observing nights. |
| 2. Ground observation with a new large telescope and several small telescopes. | 4000 observing nights. |
| 3. Detector development | |
| 4. Small airplane flights | 2500 hr. |
| Small balloon flights (~12 hr/flight) | 20 flights. |
| 5. Minimal sounding rocket program | 22 flights. |
| 6. 36-inch airborne telescope (500 hr/year starting 1972). | 1000 hr. |
| 7. Cryogenic development program | |
| 8. Further sounding rockets | Additional 30 flights. |
| 9. Construction and launch of SAS satellite | |

The highest priority items are placed at the beginning of the table.

## GOALS AND ACHIEVEMENTS

Experience shows that the opening of any new spectral region leads to surprises and discoveries. Thus, we expect that studies in the inaccessible 5 octaves of 25–700 μm will substantially modify our ideas of the nature of significant astronomical processes.

The goals of infrared astronomy are to provide data that demonstrate facts where infrared alone can provide these facts or where infrared studies of a phenomenon are easier than at some other wavelengths. In these goals, the role and future of infrared astronomy remain in doubt to a greater extent than for similar goals of other spectral regions. This is because infrared suffers from a technology gap. There is a far greater range between the fundamentally attainable performance of infrared telescopes and the actual performance than there is for any other spectral region. This is mainly because current infrared detectors do not approach appropriate theoretical limits to their performance. It is also because terrestrial telescopes are bathed in atmospheric radiation. In predicting the future of infrared astronomy, one is asking the equivalent of predicting the future of optical astronomy from those observations that could be made in daylight.

The new achievements of infrared astronomy that currently define its scope arise from advances in detector technology initiated about 1960. Thus the current assessment of achievements and goals is subject to unusually rapid revision.

### The Solar System

Infrared emission from zodiacal dust has apparently been detected with burnout zones near the Sun, indicating that the matter has separate volatile constituents. Emission from solid particles has also been detected from comet Ikeya-Seki. Further studies of this emission promise a crude chemical analysis of the material, and an opportunity to compare solar-system debris with the varieties of cosmic dust in our galaxy.

Infrared emission from planets determines their heat balance. Currently there are indications that the major planets may have appreciable internal sources of energy, although the situation is confused because the atmospheres of these plants appear to have time variations in their transmission. Internal sources of heat would be explained in terms of the structure of the planetary interior and its time development. Other studies that relate to development of planets are the observation of minor constituents of their atmospheres. Infrared spectra reveal such materials as halides in the atmosphere of Venus.

Infrared astronomy is well adapted to studying objects at planetary temperatures. Thus one major goal is to relate studies of our solar system to studies of other objects where the various processes of planetary formation may be taking place. This goal is to treat the entire solar system as a phase of stellar evolution in which observations are easier, just as the Sun by itself is studied in a context of stellar evolution today.

### Stellar Evolution

In our galaxy, current estimates of star birth and death rates would suggest that matter re-forms new stars as rapidly as possible after being ejected from dying stars. Stellar evolution can thus be considered a cyclic process with a hot condensed phase that we call stars, and a diffuse cool stage that is the interstellar medium. The objects linking these phases appear to be characterized by infrared emission.

It is now well established that many of the objects associated with the term "infrared star" are in very early or very late stages of stellar evolution. The phenomenon can be described as a shrouding of the inner high-temperature core by an extended cold envelope that absorbs part or all of the high-temperature radiation and reradiates it into the infrared. The phenomenon appears to be widespread, and may account for discrepancies between predictions of the rate of return of matter to space from star deaths, and observation of far less matter actually being returned from visible objects. The extended low-temperature envelopes of the dying stars may be this missing mass.

In the interstellar matter created by star deaths, condensation processes depend on means of cooling, and infrared radiation from dust provides very effective cooling and shielding of matter for condensation. The first observations of the emission of this dust are now being made. In the direction of the galactic center, the general dust is observed to emit strongly near 100 $\mu$m. In the Orion Nebula, a region of star formation, the dust appears hot enough to emit at 10 and 20 $\mu$m.

Regions of star formation show not only emitting dust patches but also a whole family of infrared stars that must cover a wide variety of phases of evolution of protostars and protoplanetary systems of varying masses. The variety of objects pose a challenge in piecing together the story of the early phases of stellar evolution.

The recent discovery of intense radio line emission, evidently generated by natural maser action in space, has been observationally associated with objects that emit strongly in the infrared. The understanding of these processes almost certainly requires the combination of infrared and radio measurements.

### Galaxies

The study of the distribution of stars in our galaxy has long been hindered by the opacity of interstellar dust. This opacity decreases toward the infrared. Thus the first infrared sky surveys at 2 $\mu$m are beginning to give a first clear picture of the distribution of stars in our galaxy. At longer wavelengths, surveys could reveal the distribution of dust emission, and in its spectral behavior find the distribution of the stellar radiation field in the Galaxy. These studies would be useful for fitting our knowledge of the galaxy into a context of the nature of galaxies in general.

Recent observations of intense emission from the nucleus of the galaxy and initial observations of the Andromeda galaxy promise to relate the processes of ordinary galaxies to the strange energetic processes in Seyfert galaxies and some quasars.

### Unknown Physical Processes in Nebulae and Galaxies

Large unexpected infrared output from various nebulae, such as the planetary nebulae and $\eta$ Carinae, and from certain extragalactic objects, including Seyfert galaxies and some quasars, has been found. Even with the limited number of available observations, we can make the following general statements. A mechanism for generating intense far-infrared radiation exists but is not satisfactorily understood. Furthermore, there appears to be a fundamental physical problem in accounting for the large energies that are produced in most of these objects. For example, in the case of the most energetic of the "infrared galaxies," a rest mass comparable to a galactic nucleus is apparently being transformed in $\sim 10^8$ years into infrared radiation. Of no less intrinsic importance is the phenomenon of eruptions in certain of these objects which has been observed mainly in the millimeter and centimeter spectrum. These eruptions are the most energetic single events in nature and can be studied by ground-based observations only poorly since the peak output occurs at shorter wavelengths.

By studying these phenomena we hope to learn the physics of at least one new radiative mechanism and its associated energy source. In the process the structure and evolution of galaxies should become much clearer, and we may gain a powerful new tool for studying the cosmological problem.

## GOALS TO PURSUE

### Solar System

In future studies we would like to place major emphasis on those aspects that are cosmogonic or in other ways relate to our role in the universe.

Thus studies of zodiacal dust should be aimed not only at determining its spatial distribution and temperature distribution but also at its chemical composition. All of these would be useful for comparing with dust distributions around other stars. None are available from current or planned observation in other spectral regions.

Analyses of planetary atmospheres are only a small part of the determination of planetary chemical compositions. Planetary compositions, however, are important because the large gravitational fields present in the early history of the solar system will have tended to retain the more volatile constituents. The most volatile of all may perhaps be the minor constituents of planetary atmospheres. In other parts of planetary studies, infrared observations can play a part. Those studies are primarily meteorological, geological (in the broadest sense of the word), or biological. They have great value and should be jointly pursued with groups from other disciplines.

The study of the outer layers of the Sun is a ground for checking our understanding of stellar atmospheres. One poorly understood region is the transition from an atmosphere that is cooler as one goes farther out, to a corona that becomes hotter as one goes farther out. This study requires infrared observations because the continuum opacity is near unity in the far infrared for the critical layers.

## Stellar Evolution

There are several key processes in star formation that we would like to observe in the infrared. The various stages in which a cloud of dust and gas shrinks should produce far-infrared radiation peaking at wavelengths that diminish as the size of the object decreases. Important line-emission processes that are expected in these phases may produce strong emission by neutral carbon atoms at 156 $\mu$m and by molecular hydrogen at 28 $\mu$m. Neither line can be observed from the ground. At later protostar phases, one hopes to fit in the variety of objects found already. However, T Tau, R Mon, VY CMa, $\eta$ Carinae, and the variety of objects found in H II regions are presumably only the brightest members of a whole family of objects. One needs to discover many more such objects to find connecting links. Some of these discoveries could well come from far-infrared observations.

Unexpectedly large amounts of infrared emission have been found from aging stars of various chemical compositions, and of temperatures as dissimilar as those of the planetary nebulae and some long-period variables not much warmer than 1000° K. Systematizing observations are needed. Again one expects studies in the far infrared to play a vital role in uncovering the most extreme examples of this phenomenon.

## The Galaxy

Study of some problems of galactic structure have been initiated from the Caltech sky survey at 2 $\mu$m. A series of mapping programs for the galaxy need to be initiated at space infrared wavelengths. These could afford to be of low angular resolution. One knows so little about our galaxy as a whole, and even less about its expected appearance in the infrared. There is an enormous gap between the 2-$\mu$m survey and the shortest wavelength survey at radio frequencies. Whole new classes of object and types of phenomena may be being ignored for lack of these survey-mapping programs. One expects to observe emission from interstellar dust clouds. More sophisticated studies may be able to observe line emission with all the wealth of interpretable detail this can bring.

### Other Galaxies

The energetic processes that result in substantial infrared emission from Seyfert galaxies and a proportion of quasars give rise to emission that probably peaks near 100 μm. Observations that establish the amount of this emission, the character of its time variation, and the sources in which it occurs could have profound consequences already described. One would also like to establish whether mild forms of this behavior exist in all or most galaxies.

### Cosmic Background Radiation

A variety of interesting theoretical suggestions have been made to predict weak cosmic infrared continua at various frequencies. This is because many emission processes are known at visible wavelengths, and the red-shifted radiation of very distant objects should appear in the infrared. Certain infrared emission processes will each add their own continuum or lines. Also, at radio wavelengths a microwave background radiation increases steadily toward the infrared, and one would like to know how the spectrum continues.

The studies require detection of faint background fluxes and the determination of their origin from their anisotropies. Potentially there are observations that can determine the motion of our galaxy with respect to a fundamental reference frame that they define. These studies can provide data for cosmology, a study which has suffered in the past from a severe deficiency of data.

## SPECIFIC GOALS

The specific goals of infrared measurements lie in two directions: surveys, both for and of statistical samples of objects; and the detailed studies of a few either very typical or atypical objects.

### Surveys

Unbiased surveys of essentially all the sky are essential in order to understand what kinds of infrared objects exist. One sky survey at 2 μm has been completed for the Northern Hemisphere; the kind of objects found have generally had color temperatures of ~ 1000°. It is clear that a survey at wavelengths longer than or equal to 20 μm will probably be more effective in finding entirely new types of infrared objects, such as the extragalactic objects. A second essential study is a survey to determine the luminosities of a large number of sources. Since there are many objects which emit a major fraction of their energy in the infrared, broadband measurements which lead to accurate total luminosities are required for all types of galactic and extragalactic objects. Specifically, the energy emitted in the wavelengths between 20 and 500 $\mu$m which is unassessable from beneath the Earth's atmosphere must be obtained.

### Detailed Studies of Selected Objects

The detailed studies of the infrared of selected objects will probably parallel the studies made at shorter wavelengths, since many of the same techniques are available. Limitations imposed by detectors, however, mean that for many details only the brightest objects will initially be able to be studied. Photometry in the classical atmospheric windows will undoubtedly continue to do much of the basic exploratory work; photometry with resolutions on the scale $\lambda/\Delta\lambda \approx 2$–10 will be required to bridge the energy gap between the visible and the radio regions. Measurements of this type are needed not only to obtain the gross energy spectrum but also to obtain deviations from the smooth continua which are indicative of exceptional astrophysical conditions.

Spectral resolution permits detailed analysis of composition and physical conditions. For most objects, $\lambda/\Delta\lambda \approx 100$ yields some meaningful data. Few objects are bright enough for $\lambda/\Delta\lambda = 1000$ or more. Emission-line objects, however, may well need higher resolution to yield velocity distributions in objects. In this case the velocity dispersion itself sets limits to the attainable resolution. $\lambda/\Delta\lambda = 3 \times 10^5$ is needed to observe a dispersion of 1 km/sec, typical of matter at 100° k.

### Studies of Diffuse Emission

The diffuse emission of the sky in the infrared gives the first look at the potentially cosmological studies. From these studies one would hope to determine whether cosmological observations are possible. There may be interfering factors, such as airglow emission, high-altitude clouds, vehicle contamination, or the emission of our own galaxy that interfere with these observations and force them into a regime where large telescopes will be needed to study individual faint objects. This is not necessarily an adequate substitute. With luck, the initial studies themselves may yield scientific results for these problems.

## MEANS OF MAKING OBSERVATIONS

Infrared observations are made from a great variety of platforms, each with their peculiar advantages and disadvantages. We attempt to summarize these in brief below.

We include ground-based astronomy because it fulfills a different role in infrared astronomy than it does in other fields. It is remarkable that almost without exception the senior infrared observers at institutions where ground-based observations are made also make observations from one or more of the other platforms. This is in striking contrast to optical or radio astronomy. It is a clear demonstration of the way that the various types of observations are linked in infrared studies.

### Ground

Observations from the ground are restricted to certain spectral bands with one large gap from 25 to 700 $\mu$m. Atmospheric emission is large and fluctuating. In part, these disadvantages are compensated by the relatively low cost of a major installation and by the flexibility of any program. As one tries to improve the atmospheric conditions by going to higher, drier mountaintops, the advantages in cost and flexibility become steadily less. They remain substantial, however, up to a height at which an observer needs constant oxygen, or a pressurized compartment for most of his work.

### Airplane

Stratospheric airplanes can make observations in all spectral bands. However, the most accessible altitudes of 35 000–40 000 ft are marginal for the 25–700-$\mu$m region. As the altitude increases, the danger to the observer and crew of sudden decompression increases. In current practice, 43 000 ft is often assumed to be the altitude at which oxygen masks must be worn, and at very high altitudes spacesuits are essential. The structure of an airplane restricts the range of viewing angles of equipment, and for this reason the aircraft is best suited to long observations of few objects rather than for survey work. This fits in naturally with its manned capability in requiring a moderately simple flight path for an observation, and permitting gyroscopic and photoelectric guides to point telescopes.

The current limitations of aircraft telescopes at high altitudes are largely imposed by residual atmospheric emission, though for some problems a cooled telescope might be advantageous. There are frosting problems for cold equipment exposed to the atmosphere; thus cold telescopes could be difficult to achieve. In general, the airplane offers a major advance for many infrared problems. It is not clear whether it is worth pursuing this advantage to telescopes substantially larger than 36-inch aperture. Small telescopes in small jets that can reach 50 000 ft should be made available as a standard item for development of new programs. The provision of a 36-inch aircraft telescope now planned is a major step. Some reservations remain whether the Convair 990, the currently planned carrier, can fly high enough for certain classes of observation. A program of small jet aircraft observations should be planned to obtain statistical data on the quality of various kinds of observations made from altitudes between 35 000 and 50 000 ft at different latitudes.

### Balloons

The balloon environment offers few advantages that do not accrue to the aircraft environment, and with certain disadvantages. However, if equipment is automatic or semiautomatic, if equipment is simple and rugged and does not weigh more than a few hundred pounds, a balloon program can be as effective as an aircraft program. Its particular advantages are that certain kinds of scanning modes are easy to achieve, and for problems in which residual absorption by molecules other than water are important, the extra height above aircraft altitudes may be valuable.

The cost of such programs appears to be competitive with the cost of small jet aircraft programs.

The exact division of effort between airplanes and balloons is delicate. Much will depend on the exact nature of ground facilities and organizational problems for the two types of observations. As a rule, complicated delicate equipment needing more than one flight should be restricted to aircraft where possible. Simple rugged lightweight equipment may be more appropriate for balloons. In particular, the very lightweight simple equipment may be cheaper to fly in balloons.

### Rockets

Rockets are the cheapest means of making exploratory observations from satellite altitudes. Because observing time is so short, the main goal of such programs must be in exploring the background flux in different spectral bands. In many cases rocket studies are necessary to learn about the high-altitude airglow emission and how it might interfere with astronomical observations from satellites. Incidentally during this work one makes observations that fall in the borderline between meteorology, and particles and fields of the solar system.

### Satellites

To take advantage of satellites, certain major technical advances are necessary. We believe that these advances are well worth the probable effort required to achieve them. We shall assume these advances in what follows.

#### *Small Astronomy Satellite*

The small astronomy satellite program initially offers to place 150-lb payload in orbit with an initial guidance capability of $\sim 1°$, and a potential of higher accuracy. Within this guidance accuracy, there is a capacity for studies of extended objects that could not be made from the ground, aircraft, balloons, or rocket survey studies. For few-years-ahead technology, such a satellite would probably only compete on a level footing with high-altitude aircraft for point source observations. For many-years-ahead technology, the small satellite could do better than airplane telescopes of ~36-inch diameter for equal observing times.

Because it is appropriate that the first infrared satellite should be an SAS satellite, we explore the nature of likely equipment in some detail.

Telescope cooling to low temperatures will be vital. It is expected that enough solid $H_2$ or $D_2$ could be carried to make a passive cooling system that lasted some months. The insulation levels required are not much higher than levels already in common use. Detector cooling would probably require $T \sim 4°\text{–}1°$ K; this would be attainable with $He^4$. The solid $H_2$ shield for a $He^4$ container could keep the hold time of this dewar to acceptable levels.

Detectors would be large, and current levels of performance would be adequate for most projected studies of extended objects. The detectors and their wavelength responses should be chosen to optimize the equipment sensitivity. Chopping systems should be available for both absolute level determinations and for differential studies of adjacent sky areas. We assume that the telescope size achievable in this configuration is 10 to 20 inches in aperture. One single large telescope is to be preferred to a few smaller telescopes.

#### *Large Satellite Telescopes*

The large satellite telescopes become most valuable when they can be cooled, and when really major strides have been made in detector technology. The cooling problem is no harder than for SAS systems, provided that one is prepared to accept a limited lifetime. For a really large telescope, an active cooling system could allow unlimited lifetime. The large telescope would be mainly used for point sources, though faint objects of small extent could also be observed. The infrared needs seem to conflict with the other possible uses of a large telescope, and if such conflicts arose, one would expect that an optimized 36-inch telescope would be better for most problems than an uncooled, unoptimized 120-inch telescope. Nonetheless, if detector technology failed to make advances, and if only active cooling systems were to provide adequate observing times, a large satellite telescope without mirror cooling could provide observations of point sources that could not be made with any other equipment.

## RESEARCH AND DEVELOPMENT

A viable space program in infrared astronomy needs both scientific and technological research.

Perhaps the most pressing need is for the development of cryogenics for both detector and instrumentation cooling. In fact, the uncertainty in the true potential of such a development program must be resolved in order to assess the practicability of spacecraft observations. All detectors in use with the infrared are cooled; the limiting sensitivity is a direct function of how cool the cell and its background can be maintained. Thus the need for a lightweight low-power cryogenic system which can keep the detectors at their operating temperatures for extended periods, e.g., months, is obvious. Furthermore, the background radiation provided by the telescope must be eliminated; again the technology must be developed in order to understand realistically about the feasibility of attempting spacecraft observations.

### Detectors and Preamplifiers

It has been demonstrated that the potential of telescopes in space (even more than telescopes on the ground or in planes and balloons) is expected to be limited by the inherent noise of detector-preamplifier combinations, rather than by the radiation observed. The current detectors are all capable of being improved to some extent. With bolometers, the limits can usually be described in terms of bulk properties of materials. Development would involve little cookery but considerable ingenuity. With photoconductors, the theoretical limits of performance other than by radiation tend to reside in obscure properties of the material. Development offers many more avenues, with possibilities of extreme success, but also of much time spent unprofitably.

A balanced program of detector development should include a major effort to make improved varieties of existing detectors, with the goal being immediate hardware available for all work in infrared astronomy. A second effort of comparable size should go into development of exotic detectors.

All current forms of detectors reach noise levels that strain the limits of performance of existing preamplifiers. Each detector will have its own optimum frequency of operation, and its own optimum impedance. Further, many cool detectors will develop high impedances; and to avoid capacitative losses, preamplifiers may need to operate in the same environment as the detector. Thus each detector development program requires an associated preamplifier development program. We would specifically recommend a general development of a parametric amplifier with a cooled input stage. This could well be the best preamplifier for several kinds of detectors.

### Cryogenic Problems

Telescopes in space will require systems for maintaining temperatures near absolute zero for long periods of time. This will be needed to maintain a low radiation field in the total area around a detector. Telescopes also must be cooled, but since a detector only receives radiation from a telescope over a limited solid angle and through cold filters, the telescope cooling need not be as extreme as that required for the detector.

Passive cooling systems in which large quantities of cryogenic fluids and/or solids are carried aloft should be developed first. These may need to be conceived as multifluid systems.

Active cooling systems are useful both as entire cooling systems and for use in hybrid systems to maintain the presence of that component which would be used up first. Because refrigerators use large quantities of electrical energy, a fully active system should only be considered for use with a large orbiting telescope. Hybrid systems should be examined for potential use with OAO-sized telescopes.

A related cryogenic problem is that of achieving low radiation temperatures inside telescopes. Scattered radiation from the spacecraft, Sun, Earth, or Moon would provide an infrared background far greater than other radiation predicted. A development program should be aimed at finding appropriate geometrical configurations and blackening materials to avoid this.

### Ground-Based Astronomy

An essential facet of supporting research is that of expanding the program of ground-based infrared astronomy. In particular, large-scale surveys are best done from the ground (despite the wavelength limitations that this implies), as are searches for long-term (i.e., many years) variations in infrared objects. The versatility of ground-based observations will always exceed that of space observations. Currently the largest telescope committed to infrared observations at a site selected for low humidity is a 60-inch telescope costing $100 000. Thus, at least one large infrared telescope (~120 inches) should be provided as a national facility in order to fully understand to what problems the space telescopes should be directed. The cost of such a facility would be about $1 million. The large aperture is required in order to be able to observe many of the extremely faint extragalactic objects and also to provide the scale needed to make spatial maps of complicated regions such as the Orion nebula. This must be available for long-, as well as short-, term programs.

Finally, in order to staff a program, sufficient funding should be made available at universities to provide both training facilities and student support on a graduate level of research.

### Contamination

Unpleasant topics tend to be disregarded. One such topic is the problem of contamination. Two kinds of contamination can be considered. (1) All astronomical observations contemplated here are carried out in the vicinity of the Earth which emits some 10 orders of magnitude more radiation than the signals one would like to observe in the sky. The problem of stray radiation and the minimization of telescope side-lobe response, therefore, are really serious problems which will require detailed optical analysis. Some work along these lines has already been done, but no systematic studies have been made public to our knowledge. (2) The second problem concerns contamination by gases and dust carried aloft by the vehicle. Contamination has been observed in all vehicles, but has not been studied with a view toward the minimization of false infrared signals. Inasmuch as the contamination problem still appears serious despite many years of work, particular attention will have to be spent on this nuisance item.

### Theoretical Studies

In the past 2 years, a small start has been made by theorists like Gould, Pottasch, Bahcall, and some others, to determine far-infrared spectral lines that might be expected under a variety of astronomical conditions. These studies are still far from complete. Some of the astronomical problems currently being encountered in the interpretation of excessive radiation from a variety of objects also require study. These efforts should be encouraged.

### Infrared Laboratory Studies

At the present time, there are only one or two laboratories in the world where substances of astronomical or atmospheric interest are being studied in the infrared. The infrared astronomer therefore is dependent on theorists to a much larger extent than, say, his ultraviolet counterpart. This deficiency should be corrected by encouraging wider laboratory participation in infrared astrophysical problems.

In addition to the fields mentioned above, there are several forms of instrumentation which have unique advantages to the infrared, but which are not in the mainstream of required technology and do not have the urgency which must be attached to the detector and cryogenic development.

### Spectrometers and Filters

At long wavelengths, beyond ~40 μm, commercial infrared transmitting filters are not available except as special items. Filters to isolate wide or narrow spectral ranges would be very useful for a whole variety of different astronomical purposes. This is well known from the frequent use in the visual and near-infrared parts of the spectrum. In the far infrared, two kinds of filters are currently used: transmission filters consisting of a variety of pure chemicals embedded in polyethylene and very fine wire mesh and metal spot filters which act as long and short wavelength cutoff devices, and in combinations as interference filters. A great deal of work needs to be done to allow experimenters access to a wide variety of cut-on and cutoff filters and to make narrow band $\Delta\lambda/\lambda \sim 1/10$ filters available across the entire region from 20 to 500 μm. Particular emphasis is required for low-pass filters because unwanted radiation is usually concentrated at shorter wavelengths.

Further development is also needed on high-throughput multiplexed spectrometers. The advantages of the Michelson interferometer in infrared work has been convincingly demonstrated in the past few years, and, as long as infrared detectors continue to be limited by sources other than the photon noise in the incident beam from the astronomical source, one will continue to benefit from high-luminosity spectrometers. These instruments allow the observer to make much more efficient use of the energy incident on his instrument and therefore allow him to gather information at a far higher rate than is achieved with single detector, single-passband spectrometers where only one narrow wavelength band is observed at one time. The goal of this study should be the development of a lightweight interferometer capable of operating at cryogenic temperatures.

## APPENDIX: NUMERICAL RELATIONSHIPS ON THE RELATIVE VALUE OF DIFFERENT TECHNIQUES

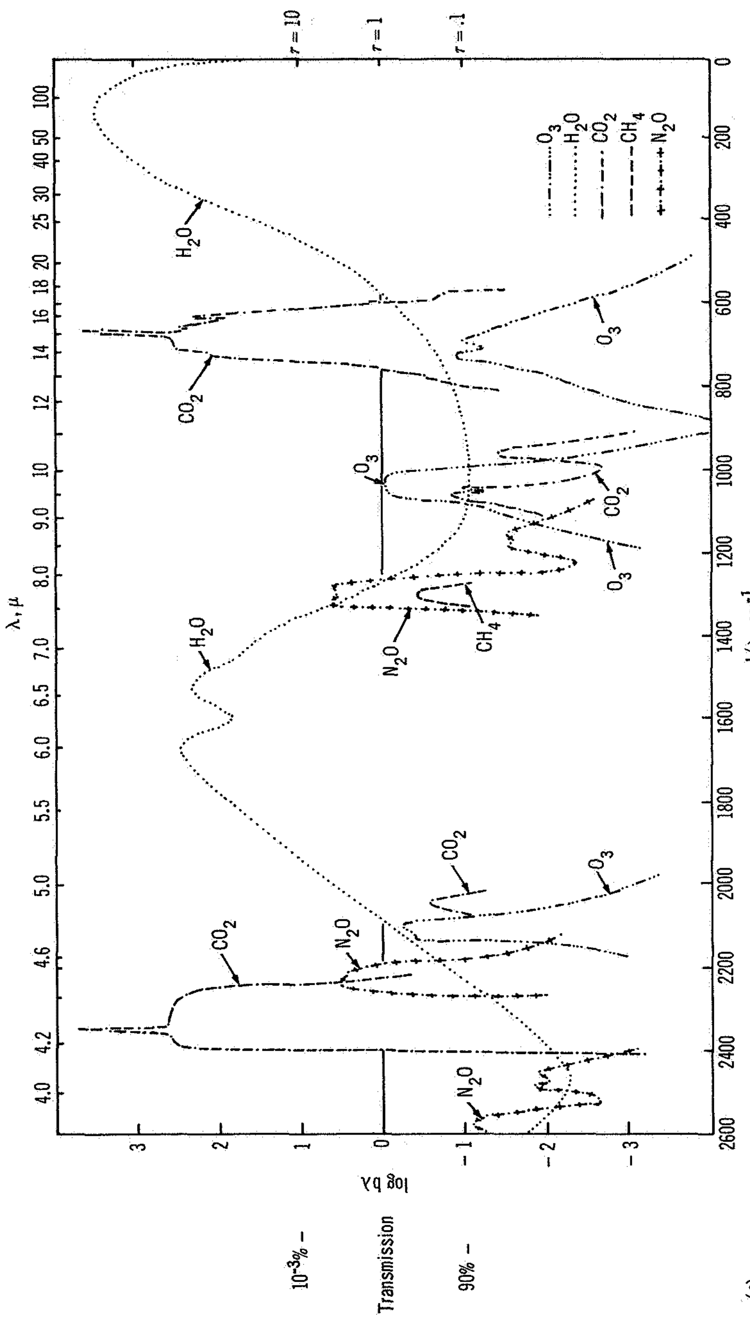
### Atmosphere Limitations

Infrared observations are possible from the ground, from airplanes, from balloons, and from space by either rocket or satellite. These different vehicles tend to increase in cost for observations with comparable equipment as one goes higher. Therefore, it is necessary to know how the atmosphere interferes with observations at various altitudes.

The opacity of the atmosphere is mainly determined by water vapor, although under dry conditions, opacity of $CO_2$, $O_3$, and possibly even $O_2$ may be important. Figure 1(a) and (b) show the opacity at sea level for 1 cm of precipitable water in a vertical column. The opacity in the clearer spectral regions is dominated by the pressure-induced wings of lines. Under these conditions, the opacity is proportional to $WP^{1/2}$, where $W$ is the amount of absorber and $P$ is the pressure. The exponent of the pressure will be lower if weak lines contribute appreciably to the total opacity.

From mountain sites, the $H_2O$ opacity is reduced by a factor of ~10 over figures 1(a) and (b) for extremely dry conditions (~1 mm of precipitable water). Other spectral features $CO_2$, $O_3$ retain almost the same strength as at sea level.

From airplane and balloon altitudes—35 000–100 000 ft—the pressure is reduced by factors of 4–100. $CO_2$ and $O_2$ are reduced by similar amounts. Ozone is concentrated high in the atmosphere and is not reduced as much. Water is mainly frozen into the lower atmosphere; however, it is convected up approximately as high as the tropopause, though with boundaries that are higher or lower with meteorological conditions. The height of the tropopause is itself time and latitude dependent. To obtain an optical depth of less than $\tau = 1$ in the middle of the $H_2O$ band at $100 \mu m$ would require less than 6 $\mu m$ of precipitable water at 35 000 ft, or less than 30 $\mu m$ of precipitable water at 100 000 ft. Observations at 45 000 to 50 000 ft from airplanes indicate an opacity of ~5 percent there, but the transition to far poorer conditions seems to occur between there and 35 000 ft. At balloon altitudes there have


λ, μ
4.0
4.2
4.6
5.0
5.5
6.0
6.5
7.0
8.0
9.0
10
12
14
16
18
20
25
30
40
50
100
τ = 10
τ = 1
τ = .1
log bλ
3
2
1
0
- 1
- 2
- 3
1/λ, cm$^{-1}$
2600
2400
2200
2000
1800
1600
1400
1200
1000
800
600
400
200
0
$O_3$
$H_2O$
$CO_2$
$CH_4$
$N_2O$
10$^{-3}$% -
Transmission
90% -

(a)

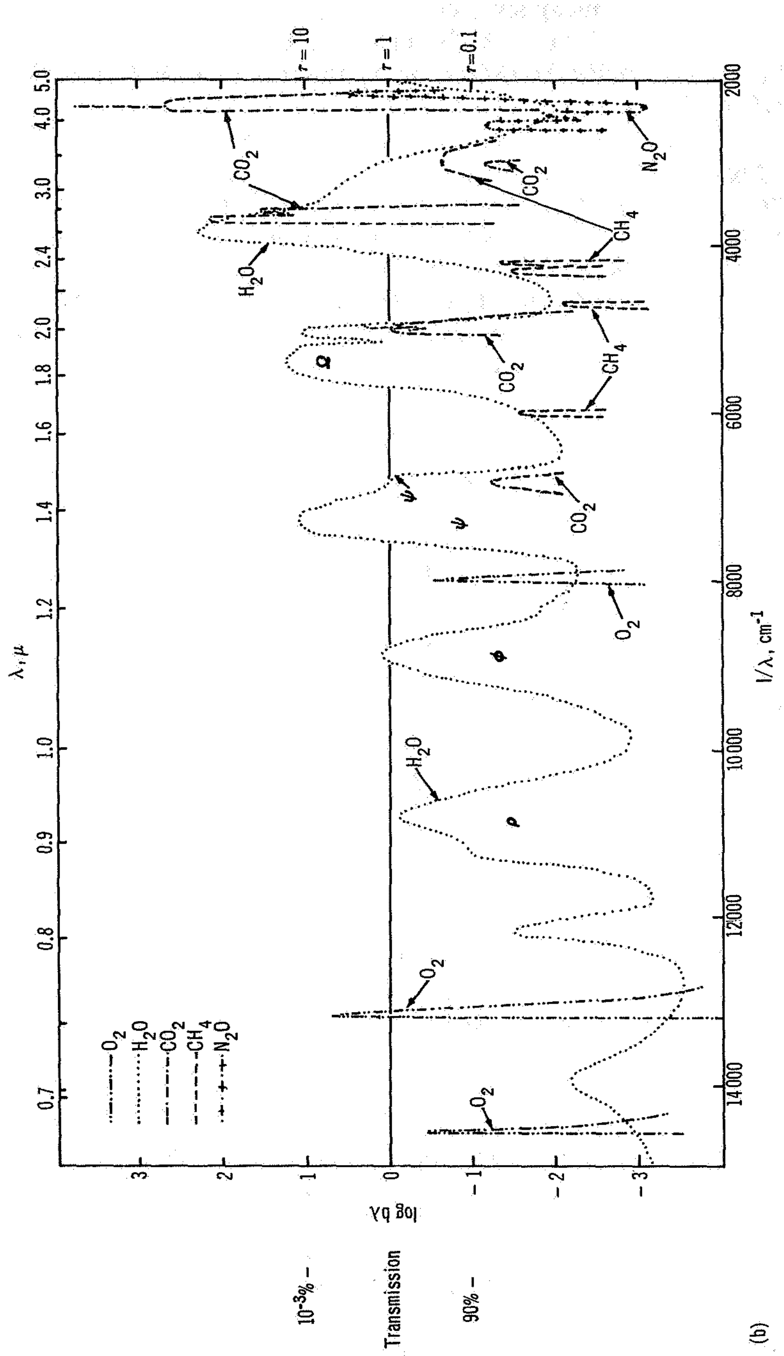


FIGURE 1.—Infrared band absorption of atmospheric gases at sea level. (*a*) 0–2600 cm⁻¹. (*b*) 2000 to 14 000 cm⁻¹.

been reports of atmospheric contamination by water carried up by the balloon and current observations have not yet indicated whether the opacity is better or worse than from the higher aircraft altitudes.

At altitudes of ~100 miles or more, atmospheric opacity should be negligible. It will be ignored in all further considerations.

Atmospheric opacity can affect observations in a variety of ways. It may lower the flux to the point where one can no longer determine the position of the object. It can cause large uncertain corrections that need to be applied to observations. On the ground, one finds that the transition between useful and useless observations tend to occur near $\tau=1$ at zenith.

### Radiation Fluctuations

Atmospheric emission is proportional to opacity, and fluctuates with time. Some atmospheric radiation will be received along with the radiation from an astronomical source. In part, the emission may fluctuate as a result of air currents, variable amounts of small ice crystals in the atmosphere, etc. There is also, however, the intrinsic statistical fluctuations of this radiation due to its wave and photon characteristics. The fluctuations observed at the ground have clearly been atmospheric fluctuations under poor conditions. Under the best conditions, one has been limited by the performance of available detectors. Telescope emission and its time variation have also caused major problems.

The minimum atmospheric emission noise, the wave-particle component, can be compounded with the minimum telescope emission noise to find how they would limit observations. Two distinct conditions arise. In observing point sources, the telescope need not observe a patch of sky substantially greater than the diffraction pattern. In observing extended objects, however, this aperture must be opened, and the most sensitive conditions arise when the aperture just equals the image size of the object.

For observing point sources, the radiation fluctuations have been calculated on the assumption that one wishes to observe the core and the first bright ring of the diffraction pattern of an evenly illuminated circular aperture. Then the throughput of the detector must be $\approx 10\lambda^2$. The bandwidth is assumed to give $\lambda/\Delta\lambda=2$. A number of different cases are considered.

CASE 1: The temperature of the telescope and atmosphere is assumed to be 300° K. A full blackbody radiation field at this temperature is assumed. This closely corresponds to typical observations from the ground.

CASE 2: The temperature of the telescope and atmosphere is taken to be 200° K, appropriate for ambient temperature telescopes in airplanes or balloons. The total opacity plus emissivity is assumed to be 0.01. This is optimistic for observations in the very middle of the $H_2O$ rotation band, but certainly achievable elsewhere.

CASE 3: The telescope is in space at an assumed temperature of 19° K with emissivity 0.01.

CASE 4: The detector is assumed to be limited by scattered radiation from the Earth or from the spacecraft with $T=300°$ K, $E=10^{-6}$. This is to indicate the level to which stray radiation must be suppressed. Rockets achieve far better suppression as a rule.

Figure 2 shows the detectivity $(=1/\text{noise equivalent power})$ as a function of wavelength for each of these four cases.

Figure 3 considers the case in which large throughput is needed, either for observing extended objects or, as will be shown later, for making sky surveys. In this case the detectivity limit is plotted for a throughput of 1-cm$^2$ steradian. For other values of throughput, the detectivity limit is proportional to (throughput)$^{1/2}$.

The detectivity can never be better than that inherent in the detectors' operation under optimum conditions. Current infrared

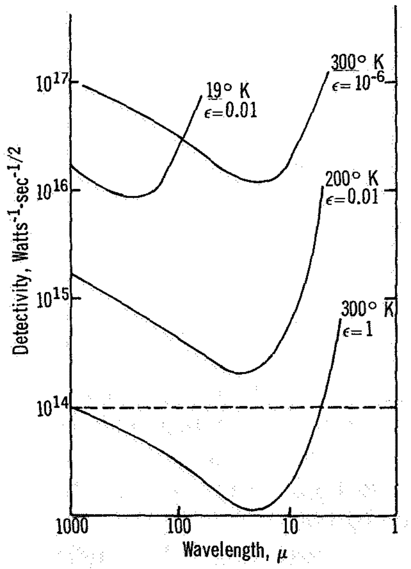


FIGURE 2.—Pointed systems.

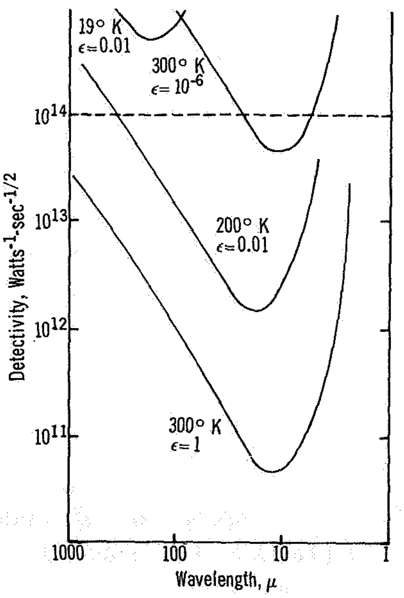


FIGURE 3.—Large throughput systems ($A\Omega=1$ cm$^2$-sr).

detectors used by astronomers in the 4-$\mu$m to 1-mm region have a detectivity of at best $10^{14}$ watts$^{-1}$ sec$^{-1/2}$. Detectors have been described in the literature with peak detectivities as high as $10^{15}$. The only fundamental limitations of detectors are the radiation noise already described. In principle, in the absence of radiation noise, the limiting detectivity of a detector need not depend on its size, and therefore on its maximum possible throughput. In practice, the most sensitive detectors made have always been among the smallest.

Because detector technology is capable of such drastic improvements, it is hard to estimate the future performance of equipment. Pessimistic estimates assume that for the next few years, detectivities of detectors in use will be at best $10^{14}$. Optimistic estimates hope for $10^{17}$. The panel has decided to make its present estimates on a basis of all detectors attaining a detectivity of $10^{15}$. Total optical losses including chopping, reduction to a sinusoidal waveform, spectral filtering, and losses due to surface reflection and absorptions will reduce this to $10^{14}$. It is assumed that this detectivity will be reached in all detectors of up to 1 cm in size, permitting throughput to reach 1-cm$^2$ steradian. It is assumed that the noise comes from the total energy incident on the telescope, with no allowance in this case for optical losses. In practice, the best complete systems in use achieve detectivities lower than $10^{13}$.

On figures 2 and 3 a line has been drawn at $10^{14}$. All positions below that line correspond to environmental limits to detectivity. All positions above this line correspond to a detector limit restricting the detectivity.

It is seen that within these limits one expects all equipment on the ground to be limited by environmental radiation, even when the telescope is used to observe point sources. All airplane, balloon, and space observations for point sources should hopefully attain a detector-limited detectivity.

For observations with large throughput, one expects ground observations to reach poorer limits of detectivity than airplane or balloon equipment. In turn, airplane or balloon equipment will reach poorer detectivities than rocket or satellite equipment, except perhaps at the longest wavelengths. From 5 to 40 $\mu$m, one hopes to attain satellite detectivities $10^3$ times higher than can be attained for ground observations of extended objects.

In practice, it will not be so easy to get this full benefit for pointing at faint objects, because with small telescopes few known astronomical objects are large enough to fill the throughput. For sky surveys, however, large throughput extends the observing time per object, and the full benefit of the detectivity can be maintained.

### Telescope Performance

The telescope aperture required for making discovery observations in the far infrared is quite uncertain. To observe the quasar 3C273 at 1 mm and detect fluctuations in its output required the 200-inch telescope. To discover the 100-$\mu$m radiation of our galaxy required a 1-inch balloon-borne telescope. However, some attempt will be made to formulate this problem. For simplicity all considerations will be made to refer to $\lambda = 100\ \mu$m, with $\lambda/\Delta\lambda = 2$.

*Pointed Systems*

The brightest stars at 10 $\mu$m have a magnitude $-6$, probably set by circumstellar dust emission. Objects in which one first sees the stellar continuum emission have a magnitude of about $-4$. At 100 $\mu$m

$$-6 \equiv 10^{-16}\ \mathrm{W\ cm^{-2}} \qquad \text{in } \lambda/\Delta\lambda = 2$$
$$-4 \equiv 1.5 \times 10^{-17}\ \mathrm{W\ cm^{-2}} \qquad \text{in } \lambda/\Delta\lambda = 2$$

Starting at the other end of the spectrum, some radio objects, e.g., 3C273, are known to emit 100 flux units with $F(\nu) \approx$ constant. H II regions may emit as much as 100 flux units per square minute of arc. Such objects at 100 $\mu$m yield $\approx 10^{-16}$ W cm$^{-2}$ in $\lambda/\Delta\lambda = 2$.

Newer classes of objects may be brighter. Thus, e.g., if the Kleinmann-Low infrared nebula is optically thick at 100 $\mu$m, it could yield $4 \times 10^{-13}$ W cm$^{-2}$. The central regions of the galaxy yield $3 \times 10^{-11}$ W cm$^{-2}$ though spread over 0.004 steradian. The $\eta$ Carinae emission, $\sim 10^{-13}$ W cm$^{-2}$, though not observed at radio wavelengths, steadily increases with increasing infrared wavelength until at 20 $\mu$m it is the brightest known object in the sky outside the solar system. Nonetheless we shall restrict ourselves to the classes of object first considered.

Assume that one requires to observe the 100 brightest objects of a class. Then one must observe $(100)^{2/3} = 20$ times fainter than the brightest objects. Then one wishes to observe

| | |
|---|---|
| Stars with shells | at $5 \times 10^{-18}$ W/cm$^2$ |
| Stars without shells | at $10^{-18}$ W/cm$^2$ |
| H II regions and quasars | at $5 \times 10^{-18}$ W/cm$^2$ |

all at 100 $\mu$m.

Let $F$ be the minimum flux detectable, $=10$ times system noise.

Let $N$ be the effective extratelescope N.E.P. for a detector, optics, filter, chopper, telescope combination.

For the assumed system $N=10^{-14}$ W sec$^{1/2}$.

Let $T$ be the telescope area.

Let $t$ be the observing time in seconds.

Then

$$F=\frac{10N}{T\sqrt{t}}$$

$$T=\frac{10N}{F\sqrt{t}} \qquad \text{pointed telescope formula} \qquad (1)$$

If as an extreme we assume $t=10^4$ seconds$=2\frac{1}{2}$ hours, and $F=10^{-18}$ W cm$^{-2}$, $N=10^{-14}$ W sec$^{1/2}$, then one needs $T=1000$ cm$^2$, a 14-inch telescope. If $N\approx10^{-13}$, as in best current practice, the telescope becomes a 40 inch. If $N=10^{-15}$, then with a 14-inch telescope one might even begin to observe the spectra of the brightest objects. Such are the uncertainties to be resolved by a development program.

### *Surveys*

We move on to sky surveys. Let us examine a fraction of the sky $1/R$.

| | |
|---|---|
| In a total time of | $t$ seconds |
| With a telescope of aperture | $T$ cm$^2$ |
| And a detector, optics, filter, chopper, telescope system of N.E.P. | $N$ W sec $^{1/2}$ |
| The detector system has throughput ($A\Omega$) | $D$ cm$^2$ ster |
| The minimum detectable flux is 10 times detector noise | $F$ W cm$^{-2}$ |

The time spent in one area of the sky is

$$\frac{RDt}{4\pi T} \qquad \text{seconds}$$

and the minimum detectable flux is thus given by

$$FT=10\,N\sqrt{\frac{4\pi T}{RDt}}$$

$$F=\sqrt{400\pi}\,\frac{N}{\sqrt{D}}\cdot\frac{1}{\sqrt{RTt}} \qquad \text{sky survey formula}$$

One can read the value of $N/\sqrt{D}$ as the reciprocal of the detectivity in figure 3. We shall consider three cases:

(1) A survey by satellite is made at any wavelength. $R=1$, $T=10^7$ seconds$=4$ months; $T=10^6$ cm$^2$; $F=3.5\times10^{-18}$ W.

(2) A balloon surveys a fraction of the sky in one night at $250\mu$, $N/D=2\times10^{-14}$, $R=100$, $T=4\times10^5$ seconds; $T=10^3$ cm$^2$; $F=3.5\times10^{-18}$ W. At shorter wavelengths the limit would be poorer.

(3) A 60-inch searchlight mirror is used for a survey at 1 mm from the ground. $R=2$, $T=10^7$ seconds, $T=2\times10^4$ cm, $N/\sqrt{D}=4\times10^{-14}$.

Then $F=10^{-19}$ W if atmospheric fluctuations are unimportant. However, the limit at, e.g., 20 $\mu$m is $10^3$ times poorer.

One sees that a small satellite as a survey instrument faces competition beyond 100 $\mu$m unless there is truly remarkable improvement in detectors. For shorter wavelengths, however, it is not easily challenged.

### *Surface Brightness*

The third case to be considered is the detection of a weak background flux or a low surface brightness. The limits of background flux detectable are set by (1) the ability to detect a weak signal and (2) the inability to subtract large interfering signals.

Of currently available vehicles, rockets seem best able to cope with the second limit. We now consider the minimum detectable surface brightness. The minimum detectable surface brightness is $B$ W cm$^{-2}$ ster$^{-1}$. Using other quantities as before

$$B=\frac{10N}{D}\,\frac{1}{\sqrt{t}}$$

However, $N/\sqrt{D}$ is a constant for those devices that are background radiation limited.

$$B=\frac{10N}{\sqrt{D}}\cdot\frac{1}{\sqrt{Dt}} \qquad \text{surface brightness formula} \qquad (3)$$

It is clear that the lowest value of $B$ is reached by having the largest value of $Dt$.

Low values of $N/\sqrt{D}$ are limited except for the longest wavelengths to rockets and satellites. The rockets are limited in observing time. Their observation will either discover the background signals in any spectral region or indicate if they are so low as to require a satellite for discovery.

When fluxes are discovered, one would like to determine spatial structure of such a flux. Since satellite observing times are, one hopes, $3\times10^4$ times longer than for rockets, one could hope to map in considerable detail any fluxes observed by rockets. The mapping of low surface brightness extended objects seems almost exclusively a problem for space vehicles. However, "extended" is taken here to imply a need for angular resolutions from 2° to $\sim10'$, because this is what is implied by $A\Omega=1$ cm$^2$ ster and telescopes of 14 to 140 inches.

*Report of the Radio Astronomy Panel*
*October 1968*
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# Low-Frequency Radio Astronomy in Space

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## INTRODUCTION

This report summarizes the first task of the Radio Astronomy Panel of the Astronomy Missions Board, which has been the construction of a program of low-frequency radio astronomy in space. The major proposal is for a large instrument, of the order of 10 km extent, that will allow observations of the order of 100 radio sources in the frequency region 1–10 MHz. A set of smaller instruments, mostly of Explorer class, for studying the solar system and the galactic background is also proposed.

Further reports on millimeter and submillimeter radio astronomy, and on the uses of space observation at conventional ground-based frequencies, will be the subject of later studies by the panel.

Support of major ground-based radio-astronomy equipment related to other phases of space astronomy is covered in part V of the AMB position paper.

## SUMMARY

### Scientific Problems

The history of radio astronomy has amply demonstrated that it can be characterized as the "science of new phenomena." Cosmology, galactic structure, supernova phenomena, and relativistic astrophysics are a few of the many branches of astrophysics that have been revolutionized by radio discoveries. Radio investigations within the solar system have shown that the physical environments of the Sun, Mercury, Venus, and Jupiter are completely different from expectations—so different that the physical design of spacecraft is profoundly affected. The extent of the Van Allen belts of Jupiter and the magnitude of the Jovian magnetic field have turned out to be far greater than expected. The structure of the solar wind has been studied from a few solar radii out to and beyond the Earth's orbit. Entirely unexpected phenomena such as the low-frequency noise bursts from Jupiter and the traveling solar noise bursts have been discovered and they display highly energetic processes that still are not understood.

Outside the solar system, entirely new classes of celestial objects have been discovered. The most dramatic are quasars and pulsars, neither of which were expected from optical observations. Whether quasars are cosmological or relatively local, they are uniquely energetic objects in which the effects of either strong gravitational fields or the largest scale structure of the universe are displayed. Pulsars have been studied for so short a time that, although their promise is far greater, one can only be certain that a uniquely periodic, energetic process has been discovered that will, at the very least, allow us to map galactic magnetic fields. Radio galaxies, whose enormous radio output is so great that they can be detected at distances far greater than the limits of optical techniques, offer the promise of probing the universe to enormous red shifts. Supernova remnants such as the Crab Nebula are still not understood, but once again it is clear that we are dealing with a highly energetic phenomenon. Our own galaxy is pervaded by high-energy particles, the cosmic rays, and radio observation of the galactic background are directly related to the electron component of this relativistic gas. In fact, the close relationship between radio emission and high-energy processes is abundantly clear.

The contribution of radio astronomy to our understanding of the large-scale structure of the universe must rely upon our understanding of the physical conditions and processes in radio galaxies and quasars. We still cannot specify the absolute luminosity of a radio galaxy from radio measurements alone, although it is clear that thousands of the known radio galaxies are at or beyond the limits of optical spectroscopic observations. The radio galaxies most commonly show a power law spectrum, the flux continuing to increase through the lowest frequencies at which reliable ground observations can be made (approximately 8 to 10 MHz). At lower frequencies, a cutoff imposed by plasma phenomena must limit the radio flux, but ground-based observations of low-frequency spectra are compromised by the highly variable absorption and scintillation effects of the ionosphere. Reliable observations from the ground could perhaps be made at 5 MHz, every 11 years near sunspot minimum, but reliable observations at lower frequencies must be made above the ionosphere.

One aim of the radio astronomers would be the establishment of a classification scheme, through space observations, of the low-frequency spectral characteristics of radio sources combined with ground-based observations of the high-frequency behavior of the radio spectra. There is the expectation, of course, that new spectral phenomena, and new classes of sources, will be discovered, as they have been when each new frequency band has been opened in the past. An impressive proportion of the important advances in astronomy in the past 20 years have come through radio observations, and the rate of discovery does not appear to be slackening.

The unexpected quality of radio-astronomy discoveries is a logical consequence of our ignorance of plasma phenomena. Through the study of astrophysical plasmas, the production of energetic particles, the existence of collective motions, the presence of magnetic fields, and the existence of natural maser processes has amply demonstrated the state of theoretical ignorance and raised a rich assortment of new possibilities. The yet-unsolved problem of plasma containment on Earth, with all the hopes it raises for the future of mankind, may well find a solution through the insight given by radio astronomical observations.

### Observing Requirements

Despite the undeniable fact that the opening of each new frequency band has seen discoveries of entirely new phenomena, a sine qua non has been adequate antenna gain. If radio astronomers could have used only single dipoles, few of the discoveries would have been made, and one can rhetorically ask what optical astronomy could have been done with photographic plates and Schott filters, unaided by optical devices of any kind. The sad truth of space radio astronomy is that exactly this state of affairs has obtained. The only satellites and rockets flown so far have carried single dipoles, and the two Radio Astronomy Explorers, while providing a logical new step, will be equivalent to only a few dipoles.

The size for the next generation of radio-astronomy instruments has been carefully considered by the Radio Astronomy Panel. The history of radio astronomy provides useful guidance. With a few exceptions, the discoveries of new classes of radio-emitting objects have been accomplished by instruments having an angular resolution of a few degrees. The discovery of low-frequency noise bursts from Jupiter; most of the studies of discrete radio sources; the discovery of quasars, pulsars, anomalous OH emission, and interplanetary scintillation, all required antennas with a resolving power of a few degrees or higher. In the band 0.5–10 MHz, however, new limitations arise from the effects of the interplanetary medium on seeing, and until these effects have been measured, 1° at 1 MHz appears to be the highest reasonable resolution to seek as a goal. One example of such an antenna is given in the appendix, which describes a combination of a rhombic antenna with a dipole array that approaches this specification. The antenna is 10 km in extent, but has a total payload weight of less than 2000 lb.

There are many kinds of measurements, however, that can profitably use smaller antennas, either single dipoles or two-dipole interferometers. These include calibration of absolute flux values, measurements of the electron density of the solar plasma, and observation of dynamic phenomena, such as solar bursts and noise bursts from Jupiter. Measurements of solar bursts should be particularly valuable in understanding the solar corona in the region 1 to 50 solar radii, a region particularly difficult to reach by optical observations or space probes. The detection of flares on the far side of the Sun may be possible, and the study of traveling disturbances in the corona through low-frequency measurements should add to our understanding of the dynamical processes in the solar wind. Observations of pulses from the planet Jupiter and from the ionosphere of the Earth can also be made with the same simple satellite-borne equipment.

## LOW-FREQUENCY RADIO SCIENCE IN SPACE

### Noncoherent Processes

Observations of electromagnetic waves in space near the Earth show complex phenomena: radiations involving magnetospheric plasma dispersion, whistlers generated near the Earth's surface, and hydromagnetic emissions of many kinds. These have in common two attributes: a close relation to the local gyro and plasma frequencies, and extremely dynamic, time-variable characteristics. Their frequency appears to correspond to ambient magnetic fields of less than $10^{-1}$ gauss and plasma densities of less than $10^2$ $\text{cm}^{-3}$.

Individual electron acceleration represents the fundamental source of radio emission. Suppose that each electron oscillates incoherently with respect to its neighbors at distances no more than one radio wavelength. An average electron, with energy $E_{av}$, interacts with EM waves both as an absorber and emitter. The more electrons there are, the greater the emission up to the point that the radio brightness becomes essentially $(E_{av}/\lambda^2)$ W steradian$^{-1}$ hertz$^{-1}$. Because of the extremely long wavelengths involved, radio emissions from incoherent thermal sources have exceedingly low brightnesses relative to emission at optical wavelengths.

In the cosmos there are many examples at radio wavelengths, in the metric and shorter wavelength region, where such incoherent sources are nevertheless intense enough to be detected merely because $E_{av}$ is stupendous. Typically, these are sources of synchrotron radio emission such as quasars or the radiation belts of Jupiter. At optical wavelengths, these objects are relatively faint, as a rule, and there are high-brightness objects such as stars and gaseous nebulae that are far more numerous. The radio observations perform the essential function of distinguishing new and unexpected high-energy phenomena from the more common, better understood objects that are intrinsically faint at radio wavelengths.

### Coherent Processes

Toward longer wavelengths, radio astronomy tends to indicate a new kind of phenomenon that cannot be studied at optical or at metric or shorter wavelengths. This is a consequence of the smaller distance between individual radiating electrons in terms of the EM wavelength, allowing collective phenomena to take place. $E_{av}$ can be very large, because one is concerned not with the energies of the individual electrons but with the total energy in the particular collective mode that is excited. For example, in Jupiter's radiation belts, there is a flux of $10^5$–$10^6$ cm$^{-2}$ sec$^{-1}$ in relativistic electrons. This corresponds to a mean electron separation of 1 m. Similar densities of relativistic electrons are found in radio galaxies, and even greater relativistic plasma densities probably exist in quasars and pulsars. Coherence phenomena involving the relativistic electrons occur only at longer wavelengths than the mean electron separation.

The standard wavelength for coherence phenomena in the Sun appears to be the plasma wavelength at frequencies near the plasma frequency. Nominally this equals the free space wavelength of an EM wave at the plasma frequency. Many details of importance, such as the way that escaping EM waves get coupled to trapped plasma waves, are not well understood. Nevertheless, we can assume generally that the electron plasma density establishes the wave frequency. Emission on a significant scale results from low-density plasma subject to dynamical phenomena like hydromagnetic shocks, rapid streaming, or convection. For the corona, the phenomena are outstandingly important at wavelengths longer than 1 m, corresponding to densities of $10^8$ electrons $cm^{-3}$. We may expect that astrophysical plasmas should show coherent emission like the Sun or Jupiter when they are excited by time-dependent phenomena.

In the Sun, Jupiter, and the Earth, we identify magnetic fields and plasma dynamics through associated low-frequency radio phenomena. The Sun and Jupiter have fields in excess of a few gauss and/or plasma densities of $10^6$–$10^8$ electrons $cm^{-3}$ and are therefore detectable from the ground by radio methods (gyro and plasma frequencies exceed a few MHz).

### Discrete Sources

In galactic and extragalactic objects, where plasma frequencies and magnetic fields are lower, the characteristic frequencies probably fall in the region between $10^5$ and $10^7$ Hz, frequencies that require space observations.

The observations of discrete sources such as radio galaxies, supernova remnants, quasars, and pulsars at long wavelengths must, however, include a large-enough number of sources to encompass a representative sample of the variety of sources known to exist. At present, there are about 500 sources whose flux densities have been measured over the range from 10-m to 3-cm wavelength. The principal difficulty in extending the spectrum of most radio sources to frequencies above 1420 MHz is the faintness of the sources. At the long-wavelength end of the radio spectrum, the increasingly serious effects of the ionosphere limit the accuracy of ground-based flux density measurements. At 20 MHz, scintillation is severe for more than half the time (at 26 MHz, repeatability of flux measurements is 13 percent rms), although measurements are still possible. At 10 MHz (30-m wavelength), scintillations are still more severe. Ionospheric absorption is increasingly serious, while interference from manmade signals propagating around the world by oblique reflection force the observer to make his measurements at sunspot minimum only. Even at sunspot minimum, on days of favorable ionospheric "seeing" as one goes from 10 to 5 MHz, the amount of interference-free observing time approaches zero. Further measurements suffer from large uncertainties because of the "patchiness" of ionospheric absorption. Measurements at still lower frequencies have been made, but comparison of these data with rocket measurements have revealed errors of a factor of 10 in ground-based observations at 2 MHz. These have now been shown to be due to uncertainty in the ionospheric absorption.

### Self-Absorption

Because of the high brightness temperature of quasi-stellar radio sources, self-absorption of the synchrotron radiation is expected to occur in the spectra of most of these sources between 1 and 10 Mhz. Very simple models of source structure would predict that self-absorbed nonthermal radio spectrum should drop off steeply with decreasing frequency with a spectral index of 2.5. Unlike the unabsorbed spectrum at higher frequencies, this rate of spectral decrease does not depend on the energy spectrum of the radiating electrons, but the radio spectrum is dependent on the physical model (e.g., not true for a self-absorbed core surrounded by a nonabsorbed halo). If the measurement yields that spectral index, it would provide a nearly positive identification of the presence of the synchrotron self-absorption mechanism. The determination of the self-absorption spectrum can provide a direct estimate of the strength of the magnetic fields in a source if its angular diameter can be determined.

Knowledge of the magnetic-field strength can be used to determine the lifetime of the radiating electrons and the ratio of the total particle energy to the total magnetic energy in the source. Quasi-stellar radio sources are also likely to have high internal electron densities. As a consequence, free-free absorption by electrons will produce a rapid decrease in the observed low-frequency radio spectrum. This decrease may be nearly exponential and can therefore be distinguished from the self-absorption effect in a simple model. If this effect is identified in the low-frequency spectrum, the electron density in the radio source can be deduced.

### Source Evolution

The majority of identified extragalactic sources are not quasi-stellar sources but usually consist of two large regions of emission not coincident with their parent galaxy. Ionizing collisions in these sources are not expected to be important. Free-free absorption may be present in some of these sources, while synchrotron self-absorption effects are expected to exist in the spectrum near 1 MHz. The principal value in determining the low-frequency spectrum of these sources is that a question concerning the evolutionary sequence of these sources may be answered. Present theoretical ideas suggest that synchrotron radiation losses of electrons produce a downward curvature in their radiation spectrum with increasing frequency. The frequency of onset of this curvature $\nu_0$ decreases with time. Most extragalactic double sources have spectra that are either straight or curve very slightly. If these sources are very young, $\nu_0$ will be greater than 10 GHz. If they are evolutionarily old, $\nu_0$ will be below 30 MHz. Observations below 30 MHz, in addition to settling this question, will provide an estimate of the age of those sources and their velocities of expansion.

### Interstellar Medium

The brightness of the background radiation from our galaxy continues to increase at the lowest frequencies observed to date from rockets. Much of the radiation is generated by synchrotron radiation from the electron component of the cosmic-ray gas, gyrating in the galactic magnetic field. The interrelationship between the electron and baryon components of cosmic radiation, the cool, neutral hydrogen gas observed by radio techniques at 21 cm and the ionized but nonrelativistic component, and the galactic magnetic field are revealed by a variety of different classes of observation. Ground-based observations of pulsars of known distance give the average electron density $\langle N_e \rangle$, and the average longitudinal magnetic field $\langle N_e \; B_L \rangle$ weighted by the nonrelativistic electron density. Low-frequency radio measurements at high galactic latitude measure the number of relativistic electrons per square centimeter in the line of sight. The space observations of galactic gamma radiation are generated by collisions between the cosmic radiation and the neutral hydrogen gas, but observed values of the gamma-ray flux appear to be too high by an order of magnitude if current ideas of gas density and cosmic-ray intensity in interstellar space are correct. The 21-cm observations of the neutral hydrogen gas yield its harmonic mean temperature $\langle 1/T_H \rangle$, as well as the total number of atoms per unit area in the line of sight. All the observations rely upon all other observations for correct interpretation, and the low-frequency observations help by giving several classes of information. First, the lower the observing frequency, the lower is the energy of the relativistic electrons responsible for the radio radiation. The production of gamma rays and the heating of the interstellar medium depend critically upon the number of particles in the energy range $10^7$–$10^9$ eV. The importance of the low-energy level of the particle energy spectrum arises from its steepness—$N(E)\,dE \sim E^{-2.7}dE$ which diverges at the low-energy end of the energy spectrum. The energy content of the relativistic gas depends critically, therefore, upon the low-energy part of the spectrum, which is measured most directly by low-frequency radio measurements.

At sufficiently low frequencies, the radio measurements are affected by the nonrelativistic ionized component of the interstellar gas. Absorption effects can be seen near the galactic plane in existing ground-based surveys at 18 MHz, but free-free absorption effects become increasingly important at low frequencies. At a sufficiently low frequency, the sky brightness should diminish when free-free absorption limits the photon mean free path sufficiently, and at that point the surface brightness is determined by the temperature of the free electrons. There is, eventually, a final cutoff when the initial frequency of the solar atmosphere is reached, somewhere between 10 and 30 kHz in the neighborhood of the Earth beyond the magnetosphere.

The phenomenon of the solar-plasma cutoff suggests an interesting possibility for a simple experiment on a planetary probe such as the grand tour. If the cutoff in frequency is sufficiently sharp, one could monitor, with very simple equipment, the average ion density in the vicinity of the spacecraft, and measure directly that point at which the probe leaves the solar cavity and reaches the interstellar medium. Along the way, direct measurements of the interplanetary electron density would be obtained.

### Intergalactic Medium

The determination of the low-frequency spectra of the very distant extragalactic sources may provide a means of measuring the electron density of the intergalactic medium. The value of this density has important cosmological implications, since there is a large discrepancy between the observed mass in the universe and the generally predicted mass from cosmological models. This "missing" matter may be in the form of intergalactic ionized gas. The problem of distinguishing between that component of the radio background that is generated within the galaxy, and the fraction that is generated in the intergalactic medium is formidable, but the combination of absorption studies of discrete sources, with the observed background brightness at low frequencies, offers some possibilities.

### Pulsars

The Crab Nebula source has recently been found at low frequencies to contain a bright central small-diameter source with a very steep radio spectrum. The brightness of this source required a coherent radiation process, quite different from the usually nonthermal cosmic radio sources. Very recently it was shown that this source is, in fact, a pulsar, sending pulses only a few milliseconds in duration with a repetition rate of 30 pulses per second. The steep spectrum implies that the source will be observable at much lower frequencies, including those frequencies that can be received with the proposed large filled aperture.

The pulsars are, typically, a low-frequency phenomenon, easily observable at wavelengths of a few meters, but observable only with difficulty at decimetric wavelengths. Their nature is not yet understood, but they are probably the remnants of supernova explosions. Whether they are white dwarf stars, neutron stars, or matter in an entirely new state of aggregation is not known. They may be rotating or vibrating at relativistic energies, and it is certain that radiation occurs through a coherent process. The expected flux in the 1–10-MHz range is hard to predict, but they would certainly be included in the program of objects to be studied. The pulsars are, apparently, the first clear examples in nature of matter in a state where gravitational fields play a dominant role in determining the physical condition of matter, and thus may be an ideal situation in which gravitational theories may be tested.

### Solar System

Ground-based measurements of radio emission from solar-system objects has produced an order of magnitude increase in our knowledge of their physics. Identification of the various classes of solar bursts, determination of the emission mechanisms, and the relationships between their occurrences and the occurrence of other forms of solar activity have yielded invaluable solar data. The observations of dynamic emission from Jupiter has proven the existence of new classes of planetary phenomena.

Space observations with low-gain dipole elements have extended this work to lower frequencies. For example, Type III (fast drift) radio bursts show extremely rapidly moving sources of excitation in the upper corona as high as 10 solar radii, corresponding to the lowest radiofrequency to which observations have extended. Motions this far away from the Sun are at speeds comparable to the velocity of light (0.1–0.5 *c*). Burst amplitude as a function of time may indicate local physical conditions such as kinetic temperature.

Type IV bursts follow solar flares at the surface of the Sun and extend down in frequency as low as 10 MHz, below which ground-based data are lacking. These emissions probably result from synchrotron effects in the upper corona, and represent dynamical phenomena on a scale not suspected from optical data. The low-frequency cutoff of Type IV emission may indicate plasma conditions at the source. Sensitive observations are required to sort out the rather weak Type IV emission from the strong galactic background at low frequencies. The low-frequency cutoff of Type II (slow drift) emissions has not been yet observed, and such observations would suggest the maximum height in the corona to which MHD shock waves propagate.

There are, in addition, classes of solar radio emission that do not fall into the presently known classification. We cannot, of course, predict the significance of their physical interpretation.

There is the interesting possibility of combined radio measurements (from a satellite in Earth orbit) with an interplanetary plasma probe that is physically within the emitting region. With the knowledge of the plasma conditions, together with the observed characteristics of the radio noise bursts, it should be possible to interpret other solar noise bursts more completely.

Jupiter shows extraordinarily complicated emission at as low frequencies as have been accessible to ground-based observations. Major surprises in the basic phenomena still undoubtedly lie in wait from lower frequency data. For example, there is the very recent discovery of Io's strong control of decametric emissions. Io orbits Jupiter at a distance of 6 planetary radii, traveling through a magnetic field of the order of magnitude of a few hundredths of a gauss. The corresponding gyrofrequency is perhaps 100 kHz, and one might expect interesting radio emissions at this approximate frequency.

It is interesting to note that, without the knowledge gained from radio observations of Jupiter, the spacecraft to explore that planet would be seriously misdesigned. The existence of energetic particles and strong magnetic fields were first suggested by the radio observations, which in themselves demonstrated the new discoveries that could be made when a new radiofrequency band was investigated. The radio observations can be expected to prove similarly interesting in the future, especially when they are made from a Jupiter probe, where they can complement the in situ plasma and magnetic-field measurements by measuring phenomena in progress in other parts of that planet's magnetosphere.

### Dilute Plasmas

The plasma densities in the Crab Nebula, Seyfert galaxies, and in quasars are typically $10^4$–$10^5$ cm$^{-3}$, with magnetic fields less than 1 gauss. Thus, the expected frequencies of plasma phenomena are in the vicinity of 1 to 3 MHz and require space observations.

Interstellar magnetic fields and plasma densities are very small, no more than $10^{-5}$ gauss or $10^{-1}$ $\text{cm}^{-3}$, respectively, and relate to emissions (excepting synchrotron effect) which are probably below values detectable from vehicles anywhere in the solar system.

Observations outside the blanket of the Earth's atmosphere may detect dynamical astronomical phenomena of a plasma origin in sources connected with stars, planetary systems, and planetary nebulae. There seems to be no other way to accomplish this objective than from space vehicles. We need to postulate the existence of fields and plasmas comparable to those of the Earth, the Sun's corona, and the planet Jupiter, but their existence is highly probable.

There are certainly dynamical phenomena in galactic nuclei and in gaseous nebulae where electron densities may be as great as $10^4$ $\text{cm}^{-3}$. This density is $10^4$ times smaller than the corona, and implies a plasma frequency 3 MHz or less.

This value is suggestive of the potential importance of extraterrestrial radio astronomy for investigations of dynamic galactic phenomena. New kinds of phenomena, which will escape discovery at earthbound observatories, remain almost certain to be discovered at satellite frequencies.

## LUNAR-BASED OBSERVATIONS

The future availability of the lunar surface for radio observation will have important implications for future planning. The far side of the Moon will be especially important for those frequencies that are heavily used by transmitters on Earth, and which pass through the ionosphere. This includes several decades of the radio spectrum above 5 MHz, and the panel will be considering the possible scientific uses of the far side of the Moon on a continuing basis.

For example, it may be possible to deploy on the Moon a very long linear antenna from storage reels directly onto the lunar surface and without elaborate space structures to support the wires above ground. A T-antenna, or a ring antenna, over distances of 50 or 100 km, might be automatically deployed from manned lunar exploratory vehicles as they go about their other business.

It appears that dynamic radio phenomena may also profitably be studied from the Moon's surface, where the permanence of the baseline makes radio direction finding easier. With a two-element swept-frequency interferometer, we could locate strong sources on the sky, including variable sources. There are reasons to expect variable galactic radio sources at low frequencies as well as at high frequencies. The proposed experiment should provide pioneering data on their characteristics.

## GROUND-BASED RESEARCH AND TECHNOLOGY DEVELOPMENT

### Ground-Based Technology Development To Aid Space Radio-Astronomy Programs

It seems that the main emphasis on development for space radio astronomy should lie in data-processing capabilities. In the VLF, LF, and HF radio bands, the problems of receiver noise, so critical at microwave frequencies, are relatively easy to cope with. Also, the difficulties of "plumbing," i.e., piping the radio signals from one part of the apparatus to another, are essentially eliminated in these low-frequency bands.

On the other hand, in this frequency range the phenomena are dynamic and complex in polarization and spectrum. This fact implies that more features of the emission need to be measured than is usually the case at higher frequency radio astronomy.

Dynamic spectroscopy and polarimetry are clearly appropriate space radio experiments. They require multichannel equipment, and at least a certain amount of onboard signal processing.

The thrust of equipment development should be toward digitization of data in early stages of experiments, and onboard computers to prepare the data for transmission to the ground. Based on this philosophy, we foresee continued usefulness of large-scale integration devices such as HF shift registers, counters, and storage. Sixty-four-place shift registers at 30-nanosecond rates, counters up to (2) 32, and fast-time storage at 4128-× 8- or 16-bit words are at or beyond the present state of the art for power and size levels compatible with space experiments.

None of these directions require, in our opinion, major new developments in technology. However, space radio astronomy provides still another area where microminiaturization is critically important.

### Ground-Based Decimetric Studies of the Sun

Ground-based observations of the Sun at fixed radio frequencies show that large bursts occur in time synchronism with extreme X-ray events observed from space. In many instances, the details of solar events on time scales of only a few seconds are shown simultaneously on 10-cm or 10-Å unit wavelengths. The importance of these phenomena for understanding the production of fast particles near solar flares is self-evident.

It is also clear from optical observations at *H*-alpha that flaring regions are there as characteristic in their shape and its variation as they are in radio or *x*-regions. Much detail appears on spatial scales of a few seconds of arc—say 5 or 6—which corresponds to the usual flare patrol pictures. There is every reason to be sure that X-ray detail during solar flares, if these data could be obtained routinely in fast sequences, would reveal similar features. These would be important, direct evidence for the localized pattern of fast particle generation in active areas of the solar chromosphere.

As a substitute for, and as independent evidence toward the physical interpretation of solar X-ray emission, we believe that a strong case can be made for high time and space resolution observations of decimetric solar emissions, on scales of 5 sec of arc or, better, in time frames of 1 sec.

An equipment to accomplish this objective is technically feasible today. It consists of an interferometer array whose basic resolution of 5 arcsec at 10-cm wavelength, is derived from a typical dimension of 4 km. For example, it might be a ring of this diameter, somewhat larger than the Culgoora ring operated by Wild and his associates in New South Wales, Australia. Instead of Wild's 100 elements, the present array requires about 1100 elements. Each element is a dish 10 ft in diameter. These dishes are spaced at 40-ft intervals around the perimeter of the ring. This interval corresponds to a linear "filling factor" of about one-fourth, which is the ratio of the size of the Sun to the beamwidth of these dishes operating at 10 cm.

With mounting, guiding equipment, and land costs included, each dish should cost about $20 000, representing in total about $22 million for dishes and land. Electronics, including a central computer to process the data for real-time display, should add perhaps $14 million, combining to give a total cost of about $36 million. This is admittedly a very crude estimate.

The cost of such an array should be compared with the cost of similar observations of the Sun in the region of extreme X-rays.

## SCIENTIFIC PROGRAM

The scope of the principal scientific objectives for a significant radio-astronomy program in space start with a large radio telescope, with high resolving power, operating over a broad range of low frequencies. The scientific objectives for such a system can be summarized as follows:

(1) Measure the flux densities of 50 to 100 extragalactic and galactic sources at a number of frequencies around 1 MHz. These measurements will extend our knowledge concerning the spectra of these sources by three or four octaves in frequency. The spectra will thus be measured in the frequency range where self-absorption and plasma effects become of dominant importance. This should revolutionize our knowledge concerning the physical conditions in such sources, and the sample of sources should be large enough to allow us to distinguish between various categories of phenomena.

(2) Map the cosmic background noise level of the full sky at a number of frequencies from 0.5 to 10 MHz. These maps will yield detailed information concerning the distribution of free electrons in the galaxy. Once the galactic component has been correctly determined, the extragalactic component of the background can be found. Such data are of critical importance in determining the large-scale structure of the universe.

(3) Measure dynamic radio-astronomical phenomena, and in particular, to record variations of radio emission from the Sun, Jupiter, and other variable radio sources. The measurements of dynamic phenomena with a sensitive telescope employing high-time resolution will be most useful in the study of plasma processes in the astrophysical environment.

(4) Measure the brightness distribution across a few individual radio sources which are occulted by the Moon when the circumstances of the occultation are appropriate. The angular resolution of the telescope will not be sufficient to resolve most radio sources. It will be very important to obtain angular information as well as spectral descriptions for a number of sources. Lunar occultations appear to be the most feasible method of obtaining such information which relates directly to the mechanisms of radio emission.

(5) Obtain data on the statistical parameters of cosmic background noise fluctuation at a few frequencies near 1 MHz. These data can be used to test hypotheses concerning the distribution of radio sources in space and a large scale structure of the universe.

(6) Study variable interplanetary absorption and interplanetary scintillation effects. Such data will be used to determine the parameters of interplanetary medium in the inaccessible regions beyond the Earth's orbit. Scintillation data will also be used to

obtain rudimentary information concerning the brightness distribution across the radio sources under observation.

Table 1 summarizes the major objectives of the program. Galactic and extragalactic observations center on the large question of origins and destinations: how was the universe formed, of what is it composed, and how does it evolve. Radio sources are important probes for the far reaches of space, but their physical nature

TABLE 1.—*List of Problems in Space Radio Astronomy*

A. *Galactic and Extragalactic*

- Evolution of the universe: cosmology:
  - Steady state
  - Big bang
- Nature of radio galaxies:
  - Universe: shape
  - Evolution
  - Physical processes
- Quasars: what are they?
- Interrelation between quasars, Seyfert, *N*-type, and other radio galaxies
- Intergalactic medium
- Formation of galaxies

- Structure of the galaxy
- Interstellar medium:
  - Distribution of high-energy component
  - Thermal component
  - Magnetic fields
- Formation of stars and planetary systems: how common and how do they evolve?

- Specific objects:
  - Supernova remnants
  - Ionized hydrogen regions
  - Pulsars
  - Dust clouds
  - OH emission and absorption
  - Infrared objects

B. *The Solar System:* how did it begin and how does it work?

- Interplanetary medium and the solar corona
- Flares:
  - Solar
  - Coronal
- Jupiter and other bursts
- Jupiter nonthermal continuum
- Earth emission and plasma effects
- Thermal planetary radiation
- Radio line spectroscopy of planets

must be understood if definitive studies are to be made, by radio methods, of the structure of the universe. The Panel is convinced that low-frequency measurements will make an important contribution to this program. The objectives of the solar system observations concentrate on the understanding of the physical processes in the solar corona and in the magnetospheres of the planets, especially Jupiter and the Earth.

It should be noted that rockets and small satellites still play an important role in the program, both as a means of calibrating the large orbiting radio observatory and, in their own right, as experiments to extend observations to extremely low frequencies, and to observe the solar system.

The solar measurements should be particularly valuable in understanding the solar corona in the region 1 to 50 solar radii, a region particularly difficult to reach by optical observations or space probes. The study of specific classes of objects covers a wide range of broad objectives, including the understanding of highly energetic events such as supernova explosions and solar flares. The study of planetary atmospheres will be covered in a subsequent report, since millimeter and submillimeter observations are required.

Table 2 lists the classes of observations and measurements that should be made at low frequencies. It should be noted that these measurements often are related to other scientific disciplines

TABLE 2.—*Observations/Measurements in Radio Astronomy Requiring Space Platforms*

A. *Galactic and Extragalactic*

- Plasma conditions and energy spectrum
- Magnetic field strength, configurations and homogeneity
- Collective effects
- Galactic halos:
  - Steep spectra
  - Radio galaxies
  - Clusters of galaxies
- Relation of X-ray, gamma-ray, and radio emission
- Cosmic radio noise background

B. *Solar System*

- Dynamic plasma phenomena
- Radio emission processes and particle acceleration mechanisms
- Space distribution and temporal variations in plasmas
- Magnetic-field configurations and confinement of high-energy particles

within the space program. The interpretation of X-ray and gamma-ray measurements, for example, has required reference to radio data, and the present interpretation of gamma-ray observations depends upon the observed radio brightness of the sky at 1 to 2 MHz.

Specific experiments are outlined in table 3. The Orbiting Radio Observatory is the most complex and expensive, and also, in our opinion, has the highest priority. The other experiments represent a reasonable complementary program to support a balanced radio-astronomy program. Some of the experiments might be carried on radio-quiet spacecraft for other missions, or might be combined into one spacecraft. Table 4 lists two possible programs, the first being the preferred program, with the second program being presented as a less expensive, stretched-out program. Leadtimes and availability of personnel were considered in arriving at the suggested launch dates.

## APPENDIX: EXAMPLE OF A LARGE FILLED-APERTURE RADIO TELESCOPE

Among the possible configurations for a large orbiting radio observatory, the large filled-aperture antenna offers the most direct approach. The aperture need only be "filled" in an electromagnetic sense, since the sum of the real currents, flowing on wires, plus the displacement currents in space, can yield the equivalent of a completely filled aperture even though only a minute fraction is physically filled by wires.

One example of such a system is the KWOT (kilometer-wave orbiting telescope) whose feasibility was studied by the University of Michigan under a grant from NASA. The configuration of the system is shown in figure 1.

The central engineering problem in developing such a long-wave orbiting antenna is the realization of large size with light weight. Dimensions of several wavelengths are required to achieve a useful resolution, but the mass must be kept within the capabilities of the available launch vehicles. A structure 10 km in diameter, with a mass of less than 2000 lb, was proposed for the system. The antenna consists of a flat rotating network of thin filaments, held in an extended configuration by the centrifugal force exerted by four subsatellites attached to the outer ends of the filaments. One pair of filaments is made of conducting material, and forms an elongated rhombic antenna ten km long. Another ten km filament crosses the rhombic. Most of this filament is non-conducting, but there are six short conducting lengths which con-

TABLE 3.—*Outline of Specific Experiments*

| Experiment | Spacecraft and orbit | Equipment | Principal measurements | Cosmological and extragalactic | Galactic | Solar system |
|---|---|---|---|---|---|---|
| 1. High-apogee rocket | — | 1–5, 5– MHz receivers, dipole antenna | Absolute measurement of background | Calibration for 2(*b*), 3, 4 | Calibration for 2(*b*), 3, 4 | |
| 2. Single Explorer<br>(*a*) Solar system monitor | Eccentric $1^d$–$2^d$ 50,000–200,000 km 200 lb | 1–10-MHz wideband spectrometer; low-gain antenna | Continuously observe solar Jupiter noise bursts | — | — | Study structure of solar corona and plasmic radiation processes between 1 and 50 solar radii |
| (*b*) Cosmic background | Same as 2(a) | Same as 2(a), but spaced receivers | Low-resolution study of cosmic background (follow on to RAE A and B) | Resolve galactic and cosmological contribution to background—look for unexplained cosmological component | Study distribution of thermal and nonthermal radiation in galaxy. Links to galactic γ-rays | |
| (*c*) Ionospheric focusing magnetospheric noise | Circular polar orbit 500–800 km 150–200 lb | 2–3-MHz low-gain antenna | Study plasma effects in ionosphere and magnetosphere | | Medium-resolution observations, intermediate between 2(*b*) and 4. | Study refraction and radiation processes in plasmas—principally magnetospheric |
| 3. Multiple Explorer | 2 or more spacecraft 10 000 km 100–200 lb | 1–10-MHz receivers; low-gain antennas | Follow development of solar and Jupiter noise bursts in frequency and position | — | — | Follow-on studies to 2(*a*) to include precise position data |
| 4. Orbiting Radio Observatory | Circular polar synchronous, or at least 10 000 km 2000–4000 lb | 1–10-MHz 10-km filled aperture antenna | Observe low-frequency spectra of discrete cosmic radio sources.<br>Obtain higher angular resolution through lunar occultation measurements. | Through low-frequency spectra, establish physical conditions in radio galaxies should aid in setting luminosity criterion for cosmological distribution studies | Study relativistic plasma processes in supernova remnants. Pulsars studies | Study structure of solar corona beyond the Earth's orbit through scintillation observations |

TABLE 4.—*Alternate Flight Schedules* *

| Project | 1969 | 1970 | 1971 | 1972 | 1973 | 1974 | 1975 | 1976 | 1977 | 1978 | 1979 | 1980 |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Ideal program: | | | | | | | | | | | | |
| Rockets | ------ | ------ | (1) | ------ | (1) | ------ | (1) | ------ | (1) | ------ | (1) | ------ |
| Single Explorers | ------ | ------ | ------ | (2(*c*)) | ------ | (2(*b*)) | (2(*a*)) | ------ | ------ | (1) | ------ | (1) |
| Interference Explorers | ------ | ------ | ------ | ------ | (3) | ------ | ------ | ------ | (3) | ------ | ------ | ------ |
| 10-km filled aperture | ------ | ------ | ------ | ------ | ------ | ------ | ------ | (4) | ------ | ------ | ------ | ------ |
| Minimum program: | | | | | | | | | | | | |
| Rockets | ------ | ------ | (1) | ------ | (1) | ------ | (1) | ------ | (1) | ------ | (1) | ------ |
| Single Explorers | ------ | ------ | ------ | ------ | 2(*c*) | ------ | 2(*b*) (or 2(*a*)) | ------ | 2(*a*) (or 2(*b*)) | ------ | ------ | ------ |
| Interference Explorers | ------ | ------ | ------ | ------ | ------ | (3) | ------ | ------ | ------ | (3) | ------ | ------ |
| 10-km filled aperture | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | (4) | ------ | ------ | ------ |

* Numbers refer to experiments discussed in table 3.

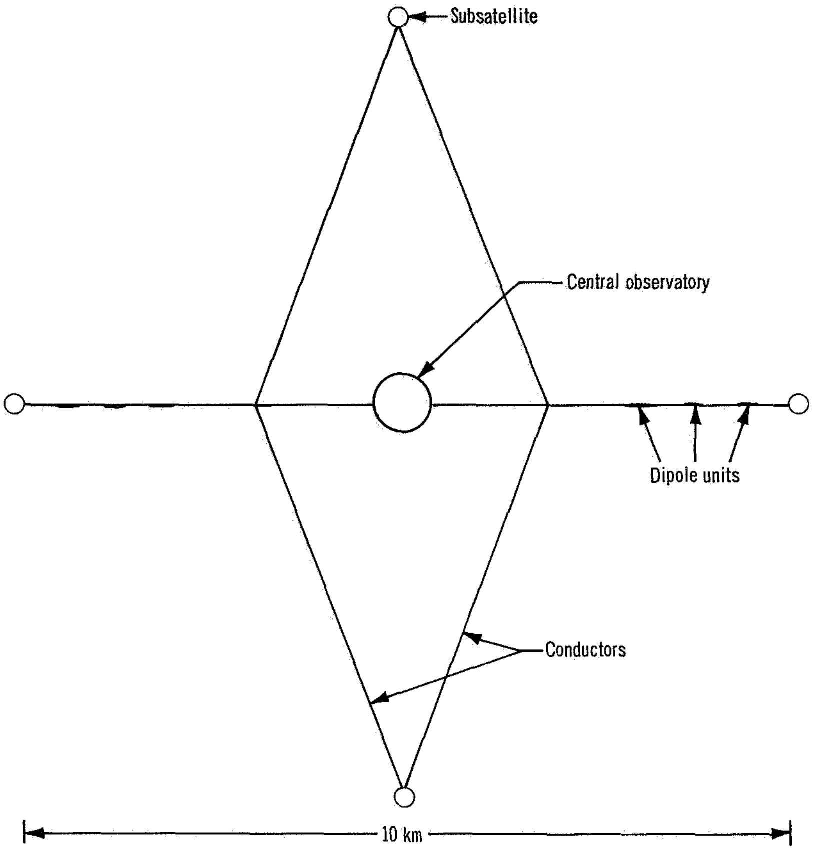

FIGURE 1. KWOT structure.

stitute an array of dipoles. A subsatellite is attached at each end of the rhombic, and another at each end of the crossmember. A "central observatory" at the center of the crossmember houses telemetry equipment, data-handling equipment, and the central control systems.

The rhombic antenna is the basic beam-forming element of the system. Its beam is parallel to its long axis, and so the same rotation which provides the centrifugal force also scans the beam across the celestial sphere along a great circle. A period of one revolution per hour provides adequate centrifugal force, together with a convenient scan rate. The plane of rotation can be changed slowly, and thus the entire sky can be observed.

The dynamics of the structure have been studied, and the results of these analyses are most encouraging. The various natural distorting forces—gravity gradient, solar wind, radiation pressure, etc.—do not produce serious distortions. A set of small rocket thrusters on each subsatellite is included, however, to deploy and spin up the system, to control the scan plane, and to provide vernier control on the configuration of the antenna. Fuel for a full year of operation would be carried.

The basic observational task of the design would be to map the entire sky at several frequencies between perhaps 0.5 and 4.0 MHz, with emphasis on frequencies near 1.0 MHz (300 m). At 1.0 MHz, the rhombic antenna beam subtends about 80 square degrees, or about 1/500th of the celestial sphere. Thus KWOT should be able to resolve at least a few dozen radio sources. The rhombic forms two such beams 180° apart, both unidirectional and independent. Thus a given point in the sky is sampled every 30 minutes, for a period of approximately 1 minute. Time variations whose period is much longer than 30 minutes, or much shorter than 1 minute, can be studied.

To study the many interesting phenomena whose characteristic periods lie between 1 and 30 minutes, it may be possible to stop the rotation of the structure for a period of time, relying upon the thrusters to keep the filaments extended. Enough fuel might be available for perhaps a dozen such stops and starts. This mode of observation would make it possible to keep the beam centered on an object, such as Jupiter or the Sun, to observe time variations, or to track the Moon to observe occultations of radio sources.

To suppress the secondary side lobes of the rhombic antenna, an auxiliary array of dipoles would be included with the rhombic, crossing the main structure and held extended to either side. The dipole signals can be combined with the rhombic signals in such a way as to reduce the contribution of the rhombic side lobes, while at the same time significantly improving the resolution in one plane.

The physical, dynamics, and electrical characteristics of the KWOT system are summarized in table 5. The configuration was selected for the following basic reasons:

(1) It lies entirely in a plane. The centrifugal force resulting from the spinning motion can be used to keep the structure extended. No compression members are required in the structure, but rather thin filaments which experience only tension.

(2) The rhombic antenna has a unidirectional beam which lies in the plane of the structure. Therefore, the same spinning motion will sweep the beam about the celestial sphere. A broadside array, on the other hand, requires a three-dimensional structure to be unidirectional.

TABLE 5.—*Summary of KWOT Parameters*

---

*Physical*

- Diameter (approx): 10 km, 6.2 mi, 30 $\lambda$ at 1 MHz
- Rhombic:
  - Leg: 17 $\lambda$
  - Semimajor axis: $=16.53\ \lambda = 4960$ m
  - Semiminor axis: $=\ 3.91\ \lambda = 1172$ m
  - Half angle at vertex: 13.3 degrees
- Central Observatory: 29 in. × 29 in. × 60 in., plus solar panels:
  - Wt: 635 lb
  - Power: 74 W ave, 272 max
- SMS: 37 in. × 25 in. × 18 in.
  - Wt: 212 or 157 lb
  - Power: 7.8 W ave, 94 W max
- Dipole unit: 9.6 in. dia. × 8.25 in. long
  - Wt: 7.5 or 9.1 lb
  - Power: 1.46 W
- Stowed configuration of entire system: "X" shape, with 4 SMS's attached by end faces to 4 side faces of C.O. length of X, tip-to-tip, 103 in. max thickness: 60 in.
- Total weight: 1421.44 lb

*Dynamic*

- Scan rate: 1 rev/hour = 6 deg/min = 0.001776 radian/sec
- Precession rate (max): ½ deg/hour:
  - = 12 deg/day
  - = 180 deg/15 days
- Centrifugal force at SMS's: $0.00159\ g \cong \frac{g}{630}$
- SMS velocity: 873 cm/sec = 28.6 ft/sec = 19.54 mi/hr
- Orbit: Synchronous (24-h period), circular, zero inclination

*Electrical*

- Rhombic: 17 $\lambda$ legs, 13.3° half-angle at apex:
  - Beam $6.3^\circ \times 16.1^\circ$
  - Ellipse ≅ 80 sq deg
- Dipoles:
  - Fringes of 30 $\lambda$ pair: 1.91°, peak to peak
  - First null, 30 $\lambda$ filled aperture: 1.91°
  - Period of highest spatial freq. (at 1 rev/hr = 6°/min → 10 sec/deg scan rate): 19.1 sec.

---

(3) The rhombic antenna has directivity in both the $E$- and $H$-plane, is relatively broadband, and requires interconnecting members.

(4) When the rhombic signals are properly combined with those from the dipole array, a pattern results which combines the $E$-plane directivity of the dipole array with the unidirectional character of the rhombic.

(5) The structure is compact when stowed, but is easy to deploy and adjust.

(6) Being composed of flexible filaments, the structure is not subject to serious deformation from temperature gradients, as are long rigid booms.

(7) There are no metallic structural members to distort the antenna field.

(8) The antenna generates a real beam, which is physically scanned over the celestial sphere in a controlled manner. Thus, the reduction of the data is simple and straightforward, with none of the uncertainties that arise in aperture synthesis due to changes in the strength of emitting objects, or in the parameters of the receiving system.

## Modes of Operation

The KWOT system would be injected into orbit in a compact configuration. Consequently, it must undergo two distinct phases prior to its utilization in radio-astronomy experiments. These phases are: deployment and steady-state operation.

### *Deployment*

Following separation from the launch vehicle, the cruciform, consisting of the Central Observatory and four SMS vehicles, is spun to an angular velocity of one revolution per hour. The rigid connections between the four subsatellites and the Central Observatory are subsequently severed, and the SMS's are commanded under active control to traverse along two orthogonal corridors emanating from the CO. Thruster firing in the radial direction is terminated when the distance between the CO and each SMS is approximately 500 m. Deployment of the antenna beyond this point is accomplished by the centrifugal force exerted by the mass of the subsatellites. Subsatellite thrusters are fired intermittently in a tangential direction in order to maintain the angular velocity of the system constant as the radius of the spinning antenna increases.

### *Steady-State Operation*

Upon completion of the deployment phase described above, steady-state operation is begun. In this mode, the KWOT is spinning at one revolution per hour and automatically precessed at the rate of one-half degree per hour. It is in this mode of operation that any position of the antenna must be kept within approximately ±50 m from a given reference datum (the CO). To achieve this goal, each of the SMS's is maintained within the 100- by 100-meter control volume, by the combined effects of the centrifugal force in the spin ($x$–$y$) plane, and by active control. The dipole units are extended by unpowered masses, but their excursions stay within acceptable limits.

## Configuration Adjustments for Frequency Coverage

The optimum frequency for a rhombic antenna depends upon the dimensions and particularly upon the apex angle. The KWOT antenna can be tuned over a wide frequency range by reeling the minor diagonal line in or out at the Central Observatory. To maintain the KWOT in dynamic equilibrium, the SMS vehicles located at the terminus points of the minor diagonal must reel-in or play out an amount of filament equal to adjustment made by the CO and thereby maintain the separation range between the CO and SMS constant.

## Launch Requirements

The proposed KWOT configuration operates in a mode which does not impose any restriction or support requirements on the launch vehicles beyond those of initial injection into orbit, checkout, and launch. After achieving the required orbit, the launch vehicle must place its longitudinal centerline along the Earth radius vector with the payload in the plane of the orbit prior to ejection.

There are several existing launch vehicles, such as Titan IIIC, that could inject the payload of 1421 lb into synchronous orbit. The system could also be included as an auxiliary payload on a Saturn S–V vehicle.

*Report of the Solar Space Astronomy Panel*
*March 13, 1969*
*Revised April 22, 1969*

# Solar Space Astronomy and a Solar Space Program

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## *Solar Space Astronomy*

### INTRODUCTION

The Astronomy Missions Board, advisory to the National Aeronautics and Space Administration, charged its subsidiary Solar Panel in early 1968 to produce a paper which would serve as a source of information and guidance to NASA in its program of solar space astronomy. In order to assure a broad representation from all aspects of solar research, the Solar Panel convened a larger Solar Working Group (SWG). Its purpose was to define the current scientific problems of solar astronomy, and determine, in the light of our present knowledge, the most effective role of solar space astronomy in the effort to solve those problems.

The SWG deliberated at the Goddard Space Flight Center, NASA, through July 19–21, 1968. The members decided that the most useful report should include an explanation of the nature of solar astronomy, the achievements of the program of solar space observatories up to the present, and their view of the future of solar space astronomy. It should then consider the specific space observations required, and the types and numbers of spacecraft that would acquire them most effectively and economically. On the basis of these considerations, the SWG should then summarize its conclusions in a set of definite recommendations.

To this end, the SWG divided into subcommittees to consider separately the problems of the solar photosphere, chromosphere, and corona. The subcommittees produced hastily written but substantial working papers, which, with minutes of the discussions of the whole SWG, are the basis of this report.

We, the SWG, have chosen to present the material in two distinct sections. Part 1, "Solar Space Astronomy," is intended to provide the broad background, stating the most effective role for space observations in the larger picture of all solar research. This is intended as a long-range paper, concerned with the relatively permanent aspects of solar astronomy. Part 2, "A Solar Space Program, 1969," proposes the specific observations required and a definite program of spacecraft flights designed to obtain them.

We wish to acknowledge here our indebtedness to the Solar Working Group at the 1965 Woods Hole Summer Study and its excellent report, chapter III, part II of "Space Research, Directions for the Future," issued by the Space Science Board of the National Academy of Sciences. Our present study covers much the same ground, since the objectives of solar astronomy and the observations required are essentially unchanged. The parts that are new are primarily concerned with program modifications suggested by the experience of the last 3 years. We have not hesitated to lift a number of sections from the 1965 Woods Hole report, quoting verbatim or with minor alterations.

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## SOLAR ASTRONOMY

Solar astronomy is a significant and fascinating part of man's uest for understanding of the universe he lives in. In observing ıe Sun and describing it in precise physical terms, the solar stronomer finds a full measure of that intellectual adventure that ıarks every challenging field of research. But solar physics is nportant for more than adventure. Other disciplines depend on it n fundamental ways. In many respects the Sun serves as a Rosetta Stone for understanding the stars. It is the dominating body of the olar system, controlling the physical state of interplanetary pace and the atmosphere of the planets. Here the physicist oberves directly the behavior of matter under extremes of temperaure, density, magnetic-field strength, and sheer scale in combinaions found nowhere else, in the laboratory or in nature. In short, olar astronomy is an exciting and challenging discipline, and one vhich contributes at the most basic level to other branches of ıstronomy and to pure physics.

The Sun is a typical specimen of the most numerous class of stars in the universe, and the only star of any type near enough for detailed observational study. In it we see structural features that are hopelessly unobservable in any other stars, the nearest of which are hundreds of thousands of times as far away. Had we not had this convenient prototype before us, our conception of stellar physics would have advanced far less rapidly in the past, and would now be far less complete than it is. Everything we learn about the Sun is ultimately a contribution to our knowledge of the stars.

The quality and quantity of the energy output of the Sun is the basic factor in the physics of the Earth, the other planets, and the interplanetary medium. The Sun is the source of virtually all the heat and light a planet receives. Photon flux from the Sun causes dissociation, excitation, and ionization of atoms and molecules in planetary atmospheres, produces ionospheres, maintains planetary heat budgets, powers the escape of atmospheres from the planets, is responsible for weather, and controls any life that exists on the planets. Particle flux from the Sun affects planetary ionospheres and magnetic fields, produces auroras, populates the interplanetary region, and changes chemical abundances by producing nuclear reactions in planetary atmospheres. In addition, solar radiation (photon and particle) affects the Moon, comets, meteoroids, and asteroids, as well as grains in interplanetary space. In fact, the whole solar system can be viewed as being embedded in the outer solar corona. A full understanding of any of these phenomena requires an exact knowledge of solar radiations of all kinds.

Solar energy ultimately sustains every living organism—both plant and animal. Without it there would be no possibility of life. Through the ages since the Earth was formed, radiation from the Sun has controlled the atmosphere and environment in which life developed, and thus has been a major agent in shaping the course of evolution. If there is extraterrestrial life in the solar system, its development must also depend on the Sun. The biologist, therefore, is interested in the history of the total flux and spectral quality of solar photons and particles, from the remote past up to the immediate present.

A very practical example of the effect of the Sun on the environment is the problem of protecting astronauts from high-energy solar protons emitted at the times of large solar flares. The most elementary protection requires a continuous monitoring of the Sun, and advice to the astronaut to terminate extravehicular activities and take cover when dangerous solar radiations are expected.

The Sun is a showcase of spectacular physical phenomena which are far beyond the capacity of any earthbound laboratory. In the past, investigators have exploited this fact to some extent. One can cite Janssen's discovery of helium, Edlén's identification of the coronal lines as forbidden transitions of highly ionized atoms, the modulation of cosmic rays by fluctuations in coronal magnetic fields, the existence of a solar wind, accelerations of energetic particles, and other discoveries of interest to the physicist. Important as these have been, they are only a beginning.

Investigators have been slow to realize the tremendous potential of the Sun as a source of purely physical data, a potential now much enhanced by the new accessibility of the far-ultraviolet and X-ray spectrum to space observations. Any present catalog of those solar phenomena which are potentially interesting as ready-made physical experiments is bound to be incomplete, but we can point to a few.

The characteristics of the Sun which make it important to the physicist are the tremendous range of combinations of temperature, density, and magnetic-field strength it displays, and the enormous volumes over which these quantities are relatively constant for time periods long enough for easy observation (as opposed to the microsecond duration of some high-temperature laboratory phenomena). The solar atmosphere and corona are the only plasma regions intermediate between the laboratory scale and the galactic scale. Here astrophysical processes involving high-temperature plasmas, magnetic fields, and energetic particles can be observed and understood.

In the corona, for example, the spectroscopist finds lines originating in material at a million-degree temperature and densities far below that of the best laboratory vacuum, distributed over a light path long enough to include an observable number of atoms. Their spectra are very peculiar and informative, and have already added much to our quantitative knowledge of the high ions.

The whole solar atmosphere is a plasma permeated by extended magnetic fields varying in strength from zero in quiet regions to thousands of gauss in the enormous active centers around sunspots. In the regions of low field strength, and in such features as the spicules, corona, and granulation, spectacular aerodynamic and hydrodynamic phenomena develop before our eyes. Granulation and the more subtle supergranulation are examples of convection, which appear to excite acoustic waves observed in the overlying stable layers of the photosphere and chromosphere. The sunspots, prominences, flares, and coronal condensations display the interaction of plasmas with strong magnetic fields. Here we find magnetically controlled mass motions, Alfvén waves, and a variety of shock phenomena, as well as emission of cosmic rays, hard and soft X-rays, and probably $\gamma$-rays. The combination of low densities, magnetic fields, and turbulent motions results in the acceleration of electrons and nucleons to relativistic energies by a process which is as yet unclear. By observing the indicators of these processes, such as X-rays, $\gamma$-rays, and nonthermal radio emission, as well as the particles in interplanetary space, the processes important in high-energy astrophysics can be understood on the solar scale.

In some branches of aerodynamics, hydrodynamics, magnetohydrodynamics, and plasma physics, theoretical developments have outstripped experimental confirmation because of laboratory limitations. Investigators are increasingly looking to the Sun, where many of the critical phenomena can be observed with the proper equipment and perseverance. To realize the full potential of the Sun as a readymade physical laboratory, we must develop new techniques and achieve better spectroscopic and angular resolution, and extend the wavelength coverage. However, with the present ground facilities and the growing space capabilities, we can look forward to a natural collaboration between solar astronomy and several areas of pure physics that will surely be fruitful for both disciplines.

In summary, solar astronomy is a scientific discipline of great intrinsic interest, and one which contributes substantially to the fields of stellar astronomy, planetary astronomy, and studies of the interplanetary medium, the Earth's ionosphere, biology, and several branches of physics. It occupies a prominent place in the edifice of scientific research, and the reasons for its study are identical with the reasons for science in general.

## SOLAR OBSERVATION FROM SPACE

The purpose of solar research is to advance our understanding of the physical state and evolution of the Sun in quantitative terms. Solar space astronomy has a very exciting and essential part to play in this enterprise. Since it is, and will continue to be, by far the most expensive part, it is important to have clearly in mind exactly what the role of solar space astronomy should be in the broad picture of solar research.

The most fruitful approach to any solar problem is to bring all available kinds of relevant data to bear, those obtained from space, from the ground, from radio telescopes, and from aircraft and balloons. Each of these yields information unobtainable in any other way, without which our picture of the Sun is incomplete in some important particulars. Among these several ways of looking at the Sun, space observation, in cultivating entirely unexplored terrain, promises the most spectacular advances. It is our only means for studying the upper solar atmosphere, the scene of the most spectacular solar activity, particulate emissions, and plasma phenomena, where all of the nonthermal energy radiated by the Sun is either generated or modified in transit.

One prime necessity for progress in solar research is the improvement of the observational data, both by refinement (greater photometric precision, angular resolution, spectroscopic resolution, etc.), and by finding new approaches to circumvent observational roadblocks. Space observation belongs to the latter category, the roadblock in question being the Earth's atmosphere.

In the past, ground-based solar astronomy reached an impasse in the study of many important problems, halted by the impenetrable atmospheric barrier. Ground observers had no hope of seeing the ultraviolet spectrum, or the small solar features where much of the action is, or the evolution of white coronal structures. Space observations surmount the barrier. Not only are they breaking recognized bottlenecks, but in addition, they uncover strange new phenomena of the greatest import for physics and solar astronomy. As these observations become more refined, we can expect a quantum jump in the advance of solar astronomy similar to those initiated by the introduction of spectroscopy and radio observations. Like these, solar space astronomy adds a powerful and revolutionary new tool to the research kit, but it does not replace any of the other techniques where they are adequate.

The observations and measurements already achieved, as well as those we confidently predict, qualify solar space astronomy as an exceedingly important and crucial branch of solar research, supporting and supported by ground-based solar astronomy, in advancing our understanding of the Sun.

The unique virtue of solar space observation is its freedom from the deleterious effects of our atmosphere. These are, briefly:

(1) Complete absorption of all radiations at wavelengths short of 2900 Å and through most of the infrared spectrum long before they reach the ground.

(2) Degradation of the sharpness of the telescopic image by optical inhomogeneities in the atmosphere (poor seeing, or atmospheric blurring) which act like waves in a swimming pool that prevent a clear view of the bottom. The smallest resolvable details visible on the Sun from the ground are about 300 km in size when seeing is at its best.

(3) Scattered light (i.e., the blue sky) that spreads a veil like a window screen over the solar image, reducing the contrast and distorting photometric measurements. Its brightness on a clear day varies from about ten millionths that of the solar disk (good days at the best locations) to several thousand millionths.

(4) Interruptions of sequential observations at night and by cloudy weather or the onset of intolerably poor seeing.

All of these problems are avoided by observing from above the atmosphere, and it is worthwhile to consider for a moment the advantages in this.

(1) At the present stage of solar physics, the study of the Sun in the UV ($\lambda = 1500-3000$ Å), EUV ($\lambda = 300-1500$ Å), and X-ray ($\lambda = 1$ to 300 Å) spectral regions is the most important and interesting task of space observation. Combinations of space observations of the corona in the EUV and X-ray wavelengths, energetic particles and solar-wind measurements, and radio and coronagraph observations from the ground will provide a near discontinuity in the growth of our understanding of the outer corona. Levels in the solar atmosphere at gradually increasing heights are visible in the UV continuum up through the temperature inversion layer in the low chromosphere (where the continuum fades out at about 1500 Å). The resonance lines of most elements, neutral and multi-

ply ionized, are accessible, important because of their unambiguous responses to the physical state of the solar atmosphere and sensitivity to abundances. Details of the upper chromosphere and lower corona, and the abrupt interface between them, can be seen in projection against the solar disk, and the greatly enhanced effects of active centers in these regions are observed. These are some of the things we know about from the early results of space research. There are doubtless many important surprises still to come. Their importance lies in the fact that here we can learn about a part of the solar atmosphere which had previously been hopelessly out of reach, and in which the most interesting nonthermal phenomena occur. Understanding here will remove an otherwise impenetrable barrier between what is known about the lower solar atmosphere and the interplanetary medium.

Our knowledge of the far-infrared solar spectrum is limited to little more than the first measurement of energy distribution in 1968. We believe that it is too early to predict the role of infrared observation from rockets and satellites. Balloon and aircraft have been very successful in carrying equipment above the infrared absorption of the atmosphere, and are fully capable of carrying out the exploratory work.

The hard X-ray and $\gamma$-ray range ($\lambda<1$ Å) is in a similar exploratory stage. X-rays above about 5 keV ($\lambda<2.5$ Å) have not been observed from the quiet Sun, while photons having energies up to several hundred keV have been observed only as extremely transient bursts during large solar flares. Predictions of nuclear $\gamma$-ray line emission (0.3–10 MeV), bremsstrahlung and 100-MeV photons from $\pi^\circ$ meson decays during particle-acceleration flares are on a reasonably firm theoretical basis, but have not yet been observed. Some of this exploratory work will be carried out from balloons, as auxiliary instruments on small satellites, or as part of the program in high-energy astronomy. As in the infrared range, discovery of new and unexpected phenomena here are likely, and the exploitation of these discoveries will require flexibility in the program, with ability to take quick advantage of new techniques.

(2) A crucial advantage of space observation is the potential for increased resolution (clarity) of small features on the Sun. The developments of the past 20 years in the theoretical and observational study of the dynamical and magnetic properties of the quiet solar atmosphere and the many features of solar activity focus increasingly on the smallest observable structures. Every improvement we achieve in spatial resolution reveals new detail of the greatest significance. The size distribution of granulation,

the fine structure of magnetic fields, the stranded structure of loop prominences, the local variations in line profiles, the turbulent velocity fields, and the minute structure in flares are examples. Ground-based observation has carried us to the verge of solutions, but in most instances falls short of the theoretically decisive resolution. While there will doubtless be interesting details too small to be seen with any resolution we may attain, many features of the solar atmosphere should have a size scale not less than about a half width of the contribution function, or perhaps one scale height. Both of these quantities are somewhat uncertain, but are probably in the 40- to 100-km range, corresponding to 0.06 to 0.14 arcsec. This is near the diffraction limit of a 1.5-m aperture for visible wavelengths. The practical resolution limits for ground-based solar observation are about 0.5 sec of arc for direct photographs and 0.8 sec of arc in the best spectra. Resolution of this grade is the kind one achieves only on very rare occasions in one lucky photograph or spectrogram. The limit is set by poor seeing in the Earth's atmosphere. This barrier is removed completely in space observation.

(3) The sky near the Sun, even at the best observing sites, is rarely less than 10 times the brightness of the inner corona. Nevertheless, ground-based observations outside of eclipse have successfully detected a few of the coronal emission lines and the polarized component of the brightest streamers of the white corona out to a height of one solar radius above the limb. However, extension of such direct measurements to higher levels appears impossible. Thus, any continuous ground-based observation of the outer corona to discover its three-dimensional structure, evolution, and relation to the interplanetary medium and geomagnetism is out of the question. It is only through use of space observatories, where the daytime sky is dark, that the necessary synoptic observations of the intermediate and outer corona can be achieved.

(4) The time development of many solar features is one of their most significant and interesting characteristics. The sequential observations of high quality over periods of hours, days, or even months, required to define temporal changes must ultimately be made from satellites, where they will be uninterrupted by diurnal and meteorological effects.

Having enlarged on the most important advantages of solar space astronomy, we are impelled to correct a misconception about it that we occasionally encounter. This is to the effect that as solar space astronomy matures, observation from the ground will become obsolete, eventually to be abandoned by all serious solar astronomers. Not so. It is true that most of the ground observations could be made better from above the atmosphere, with a certain amount of pain and very large amounts of money. But economy of means is an imperative in this, as in most other fields of endeavor. For example, printing can produce a more elegant letter to Aunt Angelina than typing, but the printed letter conveys no more information, and in the name of economy and convenience most correspondents use the typewriter. So it is with solar astronomy. Economy requires that the available resources be used to yield the greatest possible increment in knowledge about the Sun. While the optimum course may not always be self-evident, the most elementary considerations dictate that the expensive space observations be confined to those characteristics of the Sun observable by no other means; that they be backed by relatively inexpensive ground observations, simultaneous or otherwise, whenever this adds to their information content; and that ground observations be used in the very large field of solar research for which they are adequate. The flexibility, controllability, convenience, and short leadtimes of ground-based observations, and their comparatively negligible cost in human effort and money, are advantages that can never be matched by any presently conceivable space equipment.

In spite of their limited spectral range, the ground observations have a very respectable capacity in both angular and spectroscopic resolution (far superior, in fact, to those of current space equipment). The ground observers continue to discover and study previously unknown solar features, and to define the known features with increasing precision. One has only to recall such things as the discovery of supergranulation, and the small-scale oscillatory motions of the solar atmosphere, the determination of chemical abundances, the discovery of K-line "umbral flashes" in sunspots, and the important advances in the study of magnetic fields in active centers and over the quiet solar disk. These and many other results like them are not only important, but they are also essential to our understanding of the Sun, and their significance will be enormously enhanced when complementary observations from space are available.

In short, while ground-based solar astronomy would be here to stay quite independently of the space program, the important thing is that the availability of both ground and space observation notably multiplies the effectiveness of both in learning about the Sun, and both will always be necessary in a world of limited resources.

## PROBLEMS OF SOLAR ASTRONOMY

The attention of the solar astronomer is focused on the observable part of the Sun, termed, rather arbitrarily for a wholly gaseous body, the "solar atmosphere." This exceedingly minute piece of the Sun, containing only about $2 \times 10^{-10}$ of the mass, is the only handle we have for determining the character of the Sun as a whole. The physical state of the solar atmosphere is the boundary condition in space and time from which the analysis of the solar interior and its evolution must start. In the solar atmosphere we find a fascinating array of complex physical activities. They are the immediate concern of the solar astronomer, who often finds his satisfaction in untangling the physics of the solar atmosphere without too much worry about the more remote significance of his work. Every sound research result, however, does contribute toward an understanding of the basic problems of solar physics. Investigators differ in their personal catalogs of the basic problems. We believe, however, that every list would include the following.

(1) *Energy transport,* and conversion, layer by layer, from the nuclear source at the core of the Sun to the photosphere, and uphill beyond the temperature minimum ($\sim 4500°$ K) into the hot chromosphere and still hotter corona ($\sim 2\,000\,000°$ K). By what extraordinary process can some of this energy be converted into energetic photons and relativistic particles (analogous to using the energy of a slowly moving battleship to propel machinegun bullets)?

(2) *Solar magnetism,* its origin, evolution, and distribution through the body of the Sun and the solar atmosphere.

(3) *The nonuniform rotation of the Sun.*—The identifiable features of the Sun rotate faster at the Equator than in higher latitudes. What happens below the observable layers? How is angular momentum distributed through the interior of the Sun and the solar atmosphere (particularly the corona)? What mechanisms sustain the nonuniform rotation?

(4) *The solar cycle,* most evident in the 11-year sunspot cycle. What processes in the solar interior excite the large quasi-periodic variations of all forms of solar activity?

(5) *Solar activity.*—What are the physical processes underlying sunspots, flares, flare-related nonthermal processes, and the loop and surge prominences? And those underlying less-active transient features like plages and the long-lived prominences?

(6) *The solar chromosphere.*—What is the physical character of the chromosphere? What are the quantitative physical differ-

ences between the spicules and the medium in which they are embedded? Why is that a chromosphere at all?

(7) *The solar corona.*—How much and by what mechanisms do various forms of underlying activity influence the physical character of the corona? What are the details of the origin of the solar wind? What are its interactions with interplanetary magnetic fields and other material components? Why is there a corona?

(8) *Interplanetary medium.*—This consists of weak magnetic fields and outward-moving plasma (solar wind). How do sunspot magnetic fields, and the general solar field, configure to form the interplanetary fields? How are these coupled to the solar wind? Their relation to coronal streamers? What conditions in the lower coronal interface result in the solar wind?

(9) *High-energy phenomena.*—Solar flares produce very hot gases and relativistic particles. What impulsive process heats these gases to an effective 100 million °K? How are particles accelerated to extreme energies at this time? Do these same particles, or another population, produce the radio emission? How are they stored, released to the interplanetary medium, and propagated to the Earth? Do these particles produce $\gamma$-ray emission as has been predicted?

(10) *Abundances.*—What are the abundances of all of the elements in the Sun? Are any of the short-lived transuranium atoms present? Do the abundances differ in different layers of the Sun or features of the solar atmosphere? What can the abundances tell us about nucleogenesis and the evolution of the Sun (and of sunlike stars)?

(11) *Solar evolution.*—What track has the Sun traced through the temperature-luminosity diagram since its origin and on what time scale? What is its future course to be?

The problems are not independent. It is inconceivable, for instance, that magnetism in the solar interior is unrelated to the nonuniform rotation, and probably to the solar cycle and the production of solar activity. We now have partial solutions for some of these problems. None of them is susceptible to direct attack. All must be approached through a variety of subsidiary studies, both theoretical and observational, and it is difficult to imagine any solar research that does not contribute to more than one.

We will not try the reader's patience (or our own) with an attempt to outline the research required to advance our knowledge in each of these problem areas. Instead we give an example, the investigations that will contribute to one small corner of the problem of energy transport, predicated on our present knowledge, and sure to require modification as that knowledge grows.

The chromosphere and corona are much hotter than the lower photosphere. Because heat always flows from hot regions to cooler regions, they cannot be heated from below by any normal thermal process. There must be some other process, therefore, and our problem is to find out what it is. Through a constant interplay of theoretical and observational studies, we believe we have acquired a general picture of what happens. Convection brings both thermal and mechanical energy to the lowest visible level of the photosphere. It is manifested in the fine solar granulation. Most of the thermal energy escapes by radiation, but the upward motion of the convecting gas penetrates into the stable middle photosphere, and excites upward-moving acoustic waves. These waves carry the energy into the chromosphere, and through it into the corona, where the mechanical energy is dissipated in the form of heat. The observational evidence supports this basic picture, but also uncovers some sticky problems of detail. We see remarkably regular waves in the photospheric layers, coherent over small areas, with sharply defined periods of 5 minutes. However, they are apparently not simple acoustic waves, and it is not yet clear that they transport sufficient energy to supply the chromosphere and corona. Furthermore, the physical details of the conversion of the wave energy to heat at these higher levels are uncertain. Several processes have been described, but their efficiencies depend on unknown details of the physical state of the chromosphere and corona. Against this background, what now must be done?

To determine the flux of mechanical energy through the photosphere and into the chromosphere, we must have sequential observations of mass motion and magnetic fields. These depend on ground-based spectroscopic measurements of doppler and Zeeman shifts, with improved angular resolution. From these we can obtain a more detailed description of the character of the photospheric waves, their magnetic environment, and their penetration into the lower chromosphere. Combined with a model of the solar atmosphere (which also needs improvement), this information will, hopefully, define the available flux of mechanical energy at every level, and settle the question of its adequacy.

The problem of the transformation of this mechanical energy in the chromosphere and corona, possibly through several steps, is much more complex, and requires a wide variety of observational information. The temperature profile, particularly through the temperature minimum and the chromosphere-corona interface, is fundamental. The observations required are measurements of the absolute brightness of the continuum originating in the temperature minimum at wavelengths around 1600 Å and 20 to 500 μm, the profiles of XUV lines originating at all higher levels, and the mass motions, periodic or otherwise, at all levels. All of these observations must be made from above the atmosphere by rocket or satellite spacecraft and eventually they must be made with sufficient angular resolution to show the spectroscopic character of the marked inhomogeneities in the solar atmosphere. We now have a promising start with the present space observations, and a clearer idea of the problems ahead.

In addition to the solar observations, we must have a great deal of new laboratory information on atomic *f*-values and cross sections. Finally, the theoreticians must define the processes by which the observed waves in the lower layers of high density and low temperature are converted to heat in the high-level plasma of low density and high temperature, in the presence of magnetic fields.

This program, aimed at the solution of a part of the energy-transfer problem, is clearly a long one. It calls for many subsidiary investigations involving ground-based and space observations of a refinement not now possible; the technology necessary to make them possible; the astrophysical laboratory research by means of which the observations can be interpreted; and the theoretical developments required to define the specific physical processes which convert the observationally defined mechanical energy into the observationally defined temperatures in the inhomogeneous chromosphere and corona. Along the way we will have made important advances in the study of solar magnetism and solar activity, as well as a number of nonthermal plasma phenomena not accessible in terrestrial laboratories. We will also have a firm base for understanding the further transfer of nonthermal energy from the upper solar atmosphere into interplanetary space in the form of XUV radiation, solar wind, high-energy particles, and possible hydromagnetic and shock phenomena.

The foregoing example is typical of the ramifications of the research required to approach any of the basic problems. Few of the subsidiary problems, or the more basic ones beyond them, will be solved by any sudden observational breakthrough. Advances in our understanding of the Sun comes, like understanding in most mature scientific fields, through a process of interweaving and interpreting our gradually accumulating fund of observational data. However, exciting new discoveries of hitherto unsuspected phenomena of great significance do occur. Solar space observations, in the course of industriously breaking longstanding bottlenecks, frequently lead to such discoveries. The consequent flow of problems solved and new problems posed will yield some of the most impressive gains in solar astronomy for many years to come.

## CONTRIBUTION OF SPACE RESEARCH TO SOLAR PHYSICS

Growth in solar physics has been very rapid during the past two or three decades and has been steadily accelerating. Contributions from space research to our understanding of the Sun began at about the middle of this period. They are clearly responsible for much of the sustained rate of growth in the past 10 years. Several new areas of solar research owe their existence to space programs. Through these programs, important new attributes and features of the Sun have been discovered. New light has been shed on almost all of the major problems of solar physics. Questions of longstanding interest can now be asked more intelligently and more pointedly. New questions are brought to our attention as new phenomena are discovered or as previously known phenomena are observed more completely.

We are still a long way from understanding the Sun satisfactorily, even in the so-called classical areas of solar physics. The statement is even more to the point when applied to our understanding of the basic phenomena of the chromosphere and corona or to such transient events as solar flares and related energetic processes.

In addition to their bearing on solar physics, ultraviolet, X-ray, and energetic particle radiations from the Sun are of utmost importance to a wide variety of geophysical and biological problems. If one asks about the character of such radiation, how intense it may have been in the past or is likely to be in the future, one must first answer several questions. Two such questions are:

> Why does the sun have a chromosphere and corona?
> Why does it produce flares?

The real contributions of the solar space observations should be judged in the light of these types of questions, and only when viewed from this aspect does the impact of the space program come into proper perspective.

In general terms, one can say that the chromosphere, corona and flares exist because energy is transported from the Sun's interior in a form that cannot be conveyed in its entirety to outer space from a quiescent atmosphere at a temperature near 6000° C.

There exists some fraction of the Sun's energy that cannot be removed to space without the chromosphere, corona and flares. A major portion of the contributions of space programs to solar astronomy are contributions to our understanding of the energy that drives the chromosphere, the corona and solar flares, and all the nonthermal processes associated with them.

As a result of observations conducted from space vehicles, we now have for the first time a reasonably good, although still somewhat crude, understanding of the energy losses from the chromosphere and corona. Since we know the energy losses, we know what net energy is required to drive these regions of the atmosphere. This provides the theorist with the basic data he needs in his search for the source of the energy.

The solar corona loses energy through four major sinks: (1) Radiation, (2) solar wind, (3) thermal conduction back to the chromosphere, and (4) thermal conduction to space. Our quantitative knowledge of the first three of these energy sinks is due almost entirely to space observations. After a decade of fluctuations in the accepted theory, it was not known which of these energy sinks is the most important, prior to our direct confrontation with an observed far-ultraviolet solar spectrum. Measurements of coronal emission lines together with in situ measurements of the solar wind clearly elevate the radiation and thermal conduction sinks to the forefront.

Perhaps the most serious gap in our understanding of the coronal energy budget was in our knowledge of the chromosphere-corona transition region. There were no effective methods for studying this region prior to the availability of the ultraviolet spectral data. Consequently, very little was known. We now recognize this region as being of unusual importance and interest. The temperature rise from about 50 000° at the top of the chromosphere to about 2 000 000° in the corona occurs so abruptly that it becomes, of itself, a major problem to understand. Thermal conduction through this transition region represents both a major energy loss from the corona and a major energy source for the chromosphere. Spicules erupt through the transition region carrying energy from the chromosphere up into the corona at a rate comparable to the thermal energy flowing downward. They are evidently intimately linked to the transition region itself.

Analyses of the far-ultraviolet spectrum indicate that iron is probably much more abundant, relative to other elements, in the corona than in the photosphere. This claim has sparked a lively controversy among solar astronomers. Some believe that the ultraviolet data have been incorrectly interpreted and that coronal abundances agree with those in the photosphere. Others have looked for errors in photospheric analyses. There seems to be no disparity between photospheric and chromospheric abundances. Thus, if the coronal abundance of iron is indeed different from the photospheric abundance, the difference evidently arises in the chromosphere-corona transition region, and we have a new problem in explaining the difference.

Solar-wind energy losses, though not the largest for the corona as a whole, do become dominant sources of energy loss in the outer corona where radiation losses are less important. Here, too, our ability to estimate these energy losses with reasonable precision rests heavily on space observations of the solar-wind energy density at the orbit of Earth together with our improved knowledge of the structure of the corona near the Sun resulting from spectroscopic data obtained from space.

As a result of space observations, we know that energy escapes from the upper chromosphere into outer space primarily in the form of Lyman-$\alpha$ radiation. A variety of theoretical predictions have indicated that the major loss should occur through radiation from ionized helium. Once again a direct confrontation with the observed spectrum shows unmistakably that this is not the case. The reason for this disagreement between theory and observation is evidently tied to the abruptness of the transition region, and again one is faced with the question of why the transition region is so abrupt.

Prior to the acquisition of data on the ultraviolet spectrum of the Sun, we have had virtually no effective way of studying either the upper regions of the chromosphere or the low corona. Radio data, although extremely valuable, have proven ambiguous insofar as solar models are concerned. The major radiations from the nonthermal processes in the chromosphere and corona produce and control the terrestrial ionosphere where they are absorbed. They are therefore of vital interest to both geophysicist and solar physicist.

A layer of the solar atmosphere, perhaps of greater importance to the energy budget than the corona and upper chromosphere, is the region of temperature minimum. We are still not able to estimate accurately the energy required to maintain the lowest layers of the chromosphere. To make such an estimate, we must know both the actual run of temperature with height through these layers, and the theoretical temperature that would exist in the presence of radiative energy alone, to an accuracy of 100° or better. While we have not yet achieved this accuracy, the space observations have set us far along the road where we had had hardly a beginning, and there is little doubt that they will ultimately solve the problem. The radiative equilibrium temperature depends heavily upon our knowledge of both the Fraunhofer lines and the emission lines in the far ultraviolet, and the measured brightness of the UV continuum is the critical datum for the derivation of the true temperature distribution. While it is true that other data are available for this atmospheric region, they are much less sensitive and do not yield a precise temperature determination. The great virtue of the ultraviolet continuum data lies in its remarkable sensitivity to temperature. With such data measured with precision, we have the best opportunity to achieve the precision required in the temperature distribution. Current ultraviolet continuum data have already indicated that the true temperature distribution in the low chromosphere is much closer to the radiative equilibrium temperature than was supposed. Consequently, our estimated energy requirements for the low chromosphere have dropped rather sharply, much to the relief of theorists searching for the possible modes of energy transport and dissipation.

It would perhaps be an overstatement to claim that we had made large progress toward the final understanding of transient phenomena of the Sun as a result of space observations simply because we still appear to be so far removed from such an understanding. On the other hand, space observations have greatly increased our factual knowledge about what happens when such phenomena as flares or active coronal regions occur. The intense, hard X-rays observed in association with such events provides unmistakable evidence of energetic particles in the corona, as was previously evident from radio data, and provides an additional important tool for their study. The entire phenomenon of energetic particles in the solar atmosphere, how they are produced and why, is a major challenge in solar physics and in general astrophysics as well. Our ability to study such events, to determine particle fluxes and energy distributions, has been greatly enlarged by X-ray detectors and particle experiments in space. Our major effort to date has been devoted to exploring the phenomena and acquiring data as necessary forerunners of theoretical interpretations of the phenomena.

Space observations have provided data from which the energy losses from active regions can be deduced in much the same manner as for the quiet chromosphere and corona. The data are not, however, as complete as for the quiet Sun. It is known from these data that active regions require a considerably larger energy supply than quiet regions, although exactly how much more has yet to be determined. The energy required to supply a 1-cm-square column of average chromospheric and coronal material is of the order of $10^6$ ergs $\sec^{-1}$. In a class 3 flare the energy required increases usually by about a factor of 10 and in very intense flares by a factor near 100. Active regions during periods without flares seem to require an energy supply intermediate to flares and the quiet Sun. It seems clear also from the observed enhancement of chromospheric and coronal lines in active regions that the relative brightening of the active region increases with increasing temperature. This implies that in active regions a larger fraction of the available energy is dissipated in the corona and a smaller fraction in the chromosphere. None of this would be known had we not conducted space observations.

The solar chromosphere and corona are highly structured regions in which a knowledge of the geometry of the emitting regions is essential to any detailed analysis of data. Unfortunately, the size scale of most of the structures is smaller than we have been able to resolve with the current spacecraft. Remarkable progress has been made with X-ray telescopes and the scanning capabilities of recent OSO experiments. White-light coronagraphs in space have demonstrated their ability to provide a patrol of the major coronal structures, data impossible to obtain from the ground, which will be invaluable for understanding coronal structure. Existing data on the intensity and extent of coronal regions as observed in different spectral regions are highly useful and add greatly to our knowledge of the active region phenomena. Much remains to be done, however. We still need order of magnitude, and in some cases even greater, improvement in resolving power.

The simple correspondence between the polarity of interplanetary magnetic fields observed in space and photospheric magnetic fields has provided solar astronomers with valuable, and somewhat startling, information about the nature of solar magnetism. Magnetic fields obviously are part of the basic phenomena of active regions and they play an important, perhaps essential, role in the energy linkage between the solar interior and the chromosphere and corona. Space experiments are providing a vital part of the study of solar magnetism.

Perhaps not the least important of the contributions of the space program to solar physics is the indirect contribution that comes from developing new techniques and discovering new phenomena.

Much of the sustained growth of solar physics from ground-based observatories during the space era can be attributed to the space effort. We have, as a result of space programs, for example, improved the design of coronagraphs. We have renewed interest in atomic physics and spectroscopy to provide fundamental atomic parameters, wavelength standards, and line identifications. We have stimulated interest in high-energy laboratory light sources for the study of highly ionized atoms. We have developed new techniques in ultraviolet spectroscopy, in X-ray imaging devices, and in low-energy particle detectors. The wealth of data provided by space experiments has played an important role in stimulating renewed interest and new theoretical work in radiative transfer as it pertains to the boundary layers of stars.

## THE FUTURE OF SOLAR SPACE ASTRONOMY

The important results of solar space observation so far come from telescopes and spectrographs of very modest capacity carried above the atmosphere by rockets and satellites, the largest of which are the Orbiting Solar Observatories (OSO's). The OSO's have the flexibility to carry exploratory experiments inexpensively, permit quick response to new discoveries, and allow a wide participation of the scientific community. Universities of medium size find opportunities for the involvement of graduate students, and the training of the next generation of solar space astronomers who will be the principal participants in the larger projects of the future. These small vehicles have been the instruments for spectacular and important new discoveries. More prosaically, they provide the experience in technology and define the objectives for more advanced observations, as well as telling us a great deal about the Sun that was previously quite out of reach.

A vigorous future for solar astronomy depends on progressively increasing the refinement of observations from space and from the ground. By this we mean extended wavelength coverage, better angular and spectroscopic resolution, time resolution in sequential observations varying from seconds to hours, and sequence durations sufficient to define the evolution of solar features, from high-frequency velocity fluctuations to the slowly changing coronal structures. A corollary to the three resolution requirements is a large increase in entrance apertures, to reduce diffraction limitations and provide the necessary energy from smaller solar surface elements in narrower wavelength bands and shorter times. In space, larger vehicles of improved guiding stability, weight capacity, information capacity, and electric power will be necessary to carry these instruments.

A second important requirement is the program flexibility to permit rapid exploitation of frequent new discoveries and consequent modification of subsequent programs.

We suggest that following OSO, a reasonable series would include spacecraft of capabilities compatible with resolutions of 5 arcsec and 1 arcsec, culminating in a National Solar Space Observatory (NSSO) of the 0.1-arcsec class, a milestone beyond which we do not now attempt to predict the needs of solar astronomy. Any graduated series of this sort will provide for a steady logical development of our knowledge of the Sun and the observational requirements for further progress, as well as the design of the observing equipment, and the space vehicle that carries it. It also enables us to determine the proper role of men in space for the functions of equipment assembly, maintenance, subsystem replacement, renewal of expendables, and actual operation. Without the intermediate steps, we are quite unprepared to face any of these problems soundly.

Because we are unprepared, it is futile to guess what NSSO will be like, beyond a few features that are automatically dictated by the requirement for 0.1-arcsec resolution. We picture a versatile space facility with all the capabilities required to fully utilize this resolution, equipped for a variety of spectroscopic and polarimetric observations, and both single-line spectroheliograms and broadband imaging of the Sun throughout the whole spectrum, from the shortest solar X-rays to the infrared. Like a large ground observatory, it will doubtless require several telescopes with apertures in the 1- to 2-m range and a large array of interchangeable accessories. Because of its high cost, it must be maintainable in space and should be designed for decades of operation, with provision for replacement of accessories and renewal of expendables. These functions will undoubtedly require the service of men in space. Present concepts favor ground-controlled operation of NSSO, which places the instrument directly in the hands of the experimenter himself. This inclination is reinforced by NASA's demonstrated virtuosity in the arts of remote control and data transmission, and the present high cost of space operations by men. However, we may well see revolutionary advances in the next few years that could change the technical and economic factors drastically.

The most important single factor in the progress of solar space astronomy will be the interest and enthusiasm of the best qualified investigators. These depend completely on the prospect for discovery at every step along the way, and the sustained opportunity to acquire new and significant information about the Sun. This must be the basic consideration around which the whole future program must be designed.

## INTRODUCTION

Part 1, "Solar Space Astronomy" was an effort by the 1968 Solar Working Group (SWG) to explain what solar astronomy is about and the very prominent part observations from space will play in advancing our understanding of the Sun. The character of "Solar Space Program, 1969" is different. This is intended as an ongoing document to present the recommendations of the SWG for dealing with the current practical problems of obtaining solar space observations necessary to advance our understanding of the Sun in the most effective manner. Since current practical problems change continually, and sometimes abruptly, the program will doubtless require frequent revisions.

In "Solar Space Program, 1969," the objective is to assess the present situation from which the program must proceed; identify the specific observations and measurements needed, and designate their priorities; translate these into terms of spacecraft and instrumental requirements and priorities; and to make specific recommendations to NASA, including flight schedules. To this end the SWG divided into subcommittees corresponding to the major solar problem areas: photosphere, chromosphere, and corona. Each subcommittee then undertook to meet the objective in its own area.

The subcommittees then met jointly to combine their findings, and devise a program for space solar astronomy that makes the best use of the several classes of spacecraft to solve the problems of solar physics in the most orderly and efficient manner. The end product will consist of—

(1) A proper priority sequence of flights, timed with respect to larger and smaller spacecraft, rockets, ground-based observations, and the solar cycle.

(2) Minimal and maximal flight schedules (which have been requested by AMB) representing, respectively, the smallest effort in space observation that would enlist the efforts of good experimenters, and the most rapid rate of progress that can be sustained by the spacecraft designers and the experimenters.

(3) Recommendations to NASA for implementing the proposed program.

## TERMINOLOGY—CLASSIFICATION OF EXPERIMENTS AND SPACECRAFT

Before launching into the discussion of the program, we digress for a moment to clarify some terms that we use repeatedly. We have chosen to indicate the degree of sophistication of an observing instrument by stating its angular resolution $\rho$, and of a spacecraft by its peak-to-peak angular pointing stability $\pi$ (surprisingly, $\rho$ for "resolution" and $\pi$ for "pointing"), or its peak-to-peak absolute pointing accuracy $\alpha$ (generally larger than $\pi$). We will use terms like a "5″ (arcsec) experiment" or a "5″ (arcsec) spacecraft." A 5″ experiment is an experiment with $\rho = 5''$ designed with all the size and refinement of telescope and accessories necessary to achieve and fully utilize 5″ resolution. Similarly, a 5″ spacecraft is a spacecraft with $\pi = 5''$ and sufficient capacity in every respect to accommodate and support 5″ experiments (including whatever $\alpha$ is required).

Generally, an observing instrument and spacecraft are compatible if $\pi \leq \rho$. There will be exceptions, when, for instance, a telescope has an internal guiding system more accurate than the spacecraft pointing stability.

Spectroscopic resolution is the customary $\lambda/\Delta\lambda$.

Duration of a series of observations is $t$.

Time resolution, the interval between successive observations in a sequence, is $\Delta t$.

We shall occasionally use the term "video systems." By it we mean a system consisting of—

(1) A photon sensing element that simultaneously and continuously senses the intensity of all discrete picture elements of a two-dimensional image (equivalent in this respect to a photographic film) and converts it into an electrical signal.

(2) A transmitting system that ends the signals, either in real time or from an onboard storage unit, to a ground station or an orbiting station.

(3) A receiving system to permanently record the data quantitatively with a minimum of degradation, and to display them for visual inspection.

In this report we define the wavelength regions of the spectrum as follows:

IR—9000 Å to 1 mm (the region between the photographic limit and the "shortest radio waves")

V—3000 to 9000 Å (limited by atmospheric absorption and the longwave limit of photographic material)

UV—1500 to 3000 Å (from V to the shortwave limit of the solar continuum)

EUV—300 to 1500 Å (from UV to extreme shortwave limit of normal reflection optics)

X-ray—1 to 300 Å (requiring grazing incidence optics or other imaging devices)

XUV—1 to 1500 Å (the region containing nearly all of the chromospheric and coronal emission lines)

High-energy spectrum—$<1$ Å, or $>10$ keV. This region includes three reasonably distinct ranges:

- 10–300-keV bremsstrahlung continuum from active centers.
- 300-keV–10-MeV $\gamma$-ray lines from nuclear processes (predicted, but not yet observed in the Sun).
- $\sim$100-MeV bremsstrahlung from extremely relativistic electrons and $\gamma$-rays from $\pi^\circ$ decays (predicted, but not yet observed).

For brevity, $1''$ or $1'$ means 1 arcsec or 1 arcmin, respectively (not 1 inch or 1 foot).

## REVIEW OF ACTION TAKEN ON THE 1965 RECOMMENDATIONS OF THE SOLAR PANEL AT THE WOODS HOLE SUMMER STUDY

The report of the Solar Panel at Woods Hole contained 12 recommendations as follows:

*Recommendation 1.*— That the recommendation of the Iowa meeting (1962) concerning fine pointing be given immediate attention and that highest priority be given to the development of triaxially stabilized rocket attitude controls, leading, as soon as possible, to a fine-pointing system capable of an accuracy of 5 sec of arc and optimally designed for solar use. (This recommendation is essentially a reaffirmation of that made by the Iowa Summer Study, and is restated here to reflect the importance which the Working Group attaches to this matter: "We recommend that the sounding rocket program continue to receive full support; and that both the inertially guided Aerobee with fine pointing at selected stars, and the inertially guided Aerobee with fine pointing at the Sun controlled by an optical sensor, be made available at the earliest possible time.")

*Comment:* The proposed improvement (1) in sounding rockets for solar research has been largely accomplished, and the rocket experimenters are pleased with the technical improvements. The recommended increase in the number of rockets available and supporting funds for their payloads has not been realized. (See Comment on 1965 Recommendations 7 and 8).

*Recommendation 2.*—(*a*) That the presently approved Orbiting Solar Observatory (OSO) program be augmented by at least four additional launchings during the period 1970–72, inclusive; (*b*) that no decision be made to terminate the OSO program after 1972 without further review at an appropriate time; (*c*) that NASA make every effort to implement such desirable improvements in the OSO spacecraft as increased power, offset pointing, localized raster scans, provision for slightly longer instruments, greater data capacity and more flexible data format, and improved pointing accuracy (15–30 sec of arc); (*d*) that consideration be given to injection of one or more OSO spacecraft into a polar retrograde orbit in order to provide continuous surveillance of the Sun.

*Comment:* NASA is planning for three additional OSO launches: OSO's I, J, and K. Although the planning goes no farther at present, NASA has indicated a willingness to consider further OSO's if there is further useful work for these relatively inexpensive spacecraft. Some of the improvements recommended have been made. OSO now has the capability for offset pointing and raster scanning. Further upgrading recommended by the Ad Hoc Committee of NASA's Solar Subcommittee is presently being considered for OSO's I, J, and K.

*Recommendation 3.*—(*a*) That a satellite with Advanced Orbiting Solar Observatory (AOSO) specifications is an indispensable step in NASA's solar program, and must be flown close to the coming solar maximum; (*b*) that the AOSO program be accorded all the priority necessary to maintain the launch schedule shown in the Prospectus.

*Comment:* AOSO was canceled, and no comparable ground-controlled solar spacecraft of the 5″ guiding accuracy class has even been planned. Because of its relatively short life, during which continuous operation of several different instruments is hardly feasible, the ATM is not a substitute for AOSO.

*Recommendation 4.*—(*a*) That manned missions in the 1968–72 time period, such as the Astronomical Telescope Orientation Mount (ATOM) in the Apollo Extension Systems, are desirable to supplement AOSO, but cannot replace it; (*b*) that because it offers the prospect of providing answers to critical questions relating to the technology of manned space telescopes and data recovery, the ATOM concept merits vigorous support.

*Comment:* The ATOM spacecraft has been renamed "ATM (Apollo Telescope Mount)," and Apollo Extension Systems is now the Apollo Applications Program (AAP). The manned ATM has been approved and is scheduled for a 28- to 56-day flight in 1972.

*Recommendation 5.*—That solar space observation be included in the manned space science program of the Apollo Extension

Systems in order to develop the technology of manned space astronomical operations. Such observations, which could attain resolving power of 1 sec of arc in the wavelength region 500–3000 Å, mark the next logical step beyond both AOSO and ATOM.

*Comment:* NASA has made definite and commendable progress in introducing the role of scientist-astronaut into the manned space astronomy program. However, utilization of man's unique capabilities in operating and maintaining a space observatory appears far from realization. For example, the ATM mission will provide an initial evaluation of men as onboard observers operating semiautomatic experiments. But man's unique ability to assemble, repair, and replace experiments or vital components has not been incorporated into the design of any existing or planned mission.

*Recommendation 6.*—That feasibility and design studies begin immediately on orbiting solar telescopes of at least 1-m aperture designed to obtain a resolution of 0.1 sec of arc at visible wavelengths and 0.5 sec of arc at far-ultraviolet wavelength ($\lambda > 500$ Å). Very large and complex accessory instruments will be necessary to analyze the solar image. Erection, operation, and maintenance of this telescope will require full utilization of astronaut-engineers and scientists.

*Comment:* Meaningful feasibility and design studies of telescopes of the 1-m aperture and 0.1″ guiding accuracy class have not begun.

*Recommendation 7.*—That provision be made for a continuing, uninterrupted experimental program while the more advanced manned flights are in preparation, with many flights of various spacecraft, so that a scientist will have frequent opportunities for observation.

*Recommendation 8.*—That NASA find means to continue a strong program with relatively inexpensive rockets and small unmanned satellites at the same time the large manned projects are underway, since the former are indispensable to the latter.

*Comment (7 and 8)* :

NASA has provided rockets for most worthwhile experiments through fiscal year 1968, but has failed to meet the increased demand in fiscal years 1969 and 1970, a time period that is crucial to preparations for the more advanced solar spacecraft like ATM. The OSO series is to be extended by at least three additional spacecraft of improved performance.

*Recommendation 9.*—That the relationship between scientists and astronaut-observers be studied and clarified. In particular, we recommend that when a single, large scientific instrument is carried, the scientific observation be designated the primary mission for the flight.

*Comment:* The designation of the acquisition of scientific observations as the primary mission objective has been generally followed, and scientific requirements have been met whenever the requirement can be achieved within certain general Apollo Applications Program constraints.

*Recommendation 10.*—That NASA bring more scientists into the space-flight program as astronauts or observers.

*Comment:* See comment on Recommendation 5.

*Recommendation 11.*—That NASA move to provide additional support for ground-based solar studies. As the flight program grows in sophistication and success during the next several years, the demands on ground-based work will also increase, and NASA should in turn anticipate an increased demand upon its resources for support of ground-based facilities and operations. In addition, in the next few years, NASA should expect, and respond favorably to, proposals for a few major ground-based solar installations.

*Recommendation 12.*—That increased support be given to physical research in the laboratory, as required to develop improved space instrumentation for solar-physics research, to assist in the data reduction, and to make possible a full interpretation of the results.

*Comment* (*11 and 12*) : NASA has continued to support ground-based solar astronomy, astrophysical laboratory research, and theoretical investigations related to solar research. The dollar amounts have remained about constant, near 2 million/yr, with no provision for the increasing costs, although in the face of reduced solar program funds in fiscal years 1968 and 1969 have been reduced. Included in this support have been about 1.7 million for the construction and operation of two major ground-based solar installations.

## PRESENT STATUS OF SOLAR SPACE RESEARCH

NASA's program in solar research has showed modest progress from the beginning. The opportunities for sounding rocket research have, at times, exceeded the demand, but the program is presently falling behind the demand rather seriously. The upgrading of sounding rocket capabilities appears to be well in hand, after a considerable delay. Aircraft have proved to be practical and very useful for observations of eclipses and the far-infrared solar spectrum. Although the OSO program is behind its originally planned schedule, it has gone ahead steadily. Five OSO spacecraft have been successfully launched. There have been spacecraft and experiment failures but, on the whole, the OSO's are a tremendous success. They are the most advanced spacecraft so far flown for solar astronomy, and have amply justified the effort NASA and the many experimenters have put into them.

The first four OSO's were 60″ spacecraft and those presently planned are upgraded to 30″. The scientific results have been invaluable and the experience acquired has prepared us for the next step, observations from 5″ spacecraft.

The current status of NASA's future program for orbiting solar spacecraft is as follows:

OSO's F, G, H are definitely approved and the construction of both experiments and spacecraft is underway. NASA plans to extend the series by three more spacecraft which will incorporate improvements recommended by the 1968 Ad Hoc Committee of the Solar Subcommittee. They are OSO's I, J, and K for the 1972–75 period.

ATM–A is definitely approved and scheduled for flight in 1972. This will be the first of the much-needed 5″ spacecraft, and the first experiment in manned operation of solar observing instruments. It is unquestionably the most important solar spacecraft now planned.

## PRIORITIES OF SPACE OBSERVATIONS

We come now to the problem of priorities in the solar space program, the order in which particular observations should be made (rather than their relative importance). We consider first the priorities of the observations needed, confining the list to those things that appear feasible either with current technical means, or with the aid of likely developments in technology and spacecraft. These are the scientific priorities which will be translated into the priorities of practically realizable instruments and spacecraft, a program where scientific desirability is tempered by considerations of timely feasibility, expense, and leadtime.

The scientific objectives and the space observations that contribute to them are discussed above in "Solar Space Astronomy." The logical progression consists of observations of solar spectra and images (preferably single-line spectroheliograms) of increasing refinement in angular resolution and spectroscopic resolution ($\rho$ and $\lambda/\Delta\lambda$), and sufficient time resolution and duration ($\Delta t$ and $t$) to describe the life histories of changing phenomena over the whole XUV, UV, and V spectrum. For solar radiation in the high-energy spectrum ($\lambda < 1$ Å), the immediate problem is to detect its existence, and to measure its intensity and spectral distribution in relation to solar active features. The structure of the corona, and the features at lower levels which appear to define that structure, vary with phase in the sunspot cycle. Hence, many of the observations should be continued or repeated every 1 to 3 years (depending on phase in the cycle) throughout at least 1 cycle.

It is of utmost importance that an approximate parity be maintained in the different measures of experimental sophistication, $\rho$, $\lambda/\Delta\lambda$, $\Delta t$, and $t$. The fundamental and ultimate data we seek are the spectra of the different features of the Sun together with their spatial and time variations. Experiments designed, for example, to give precise time variations of line intensities or precise profiles of lines should be accompanied by experiments that accurately define where the particular radiations are coming from in the solar image. Thus, spectra and monochromatic images are complementary data, each profiting greatly from the information contained in the other. Supplementary ground observations are usually important, and since they are inexpensive, they should always be planned.

*Spectra.*—The solar spectrum has now been completely *surveyed* down to $\lambda \approx 1.8$Å, with sufficient resolution to show its bright line character, and wavelength determinations accurate enough to permit the identification of nearly a thousand lines. The region 1200–3000 Å has been *mapped* with resolution $\lambda/\Delta\lambda \sim 10^4$–$10^5$. This is enough to eliminate instrumental blends (as opposed to physical blends where the line profiles overlap) and show all the lines in the spectrum.

The logical next steps in the observations of the spectrum in order of priority are as follows:

(1) Improve the absolute photometry in the XUV (presently uncertain by factors of 3 to 10 at most wavelengths) and reduce spectroscopic impurities which can seriously degrade the photometry.

(2) Obtain time sequences of spectra in all the UV and X-ray wavelengths with good angular resolution to show the variations occurring in active centers.

(3) Measure line profiles for the quiet Sun, and their time variations in active centers. For the latter, the angular resolution will be energy limited, but should be pushed as far as possible. The profiles will be well determined if the spectroscopic resolving power $\lambda/\Delta\lambda$ exceeds 10 $\lambda/\delta\lambda$, where $\delta\lambda$ is the line width. They will be determined usefully but roughly if $\lambda/\Delta\lambda$ is as little as 3 $\lambda/\delta\lambda$. The line widths appear to be roughly 0.1 Å through the whole EUV and UV spectrum, exceeding the minimum widths for pure doppler

thermal broadening by a considerable factor. Thus in the 300- to 3000-Å region $\lambda/\delta\lambda \approx 10\ \lambda$ (in angstroms). This rule of thumb might possibly be extended to shorter wavelengths, but more experience in the X-ray region is needed before any safe estimates of $\lambda/\delta\lambda$ are possible.

(4) Determine center-to-limb variations of the intensities of the continuum and bright lines of all ionization potentials. The critical part is the measurement of the limb profile, the accuracy of which depends primarily on the angular resolution.

(5) Obtain spectra in the near-UV and visible wavelengths (1200 to 10 000 Å) with very high angular resolution, $\rho \leq 0.4''$, to show the velocity and line profile variations from point to point on the Sun and with time.

(6) Map the whole $\lambda < 1200$-Å region with sufficient spectroscopic resolving power $\lambda/\Delta\lambda$, to eliminate instrumental blending. According to the rule of thumb suggested above, $\lambda/\Delta\lambda = 10\ \lambda$ should suffice at $\lambda > 300$ Å.

(7) Map the infrared spectrum, free of instrumental blends.

At the higher energies, hard X-rays and $\gamma$-rays, the existence of solar radiations must be determined. It they are present, the next problem is to measure their spectral distribution and correlation with flares and possibly other forms of activity. Ultimately it would be important to determine their areas of origin on the Sun, but this is a task considerably beyond the foreseeable technology.

*Images.*—Present observations of XUV images of the Sun now include: slitless spectrograms taken from rockets showing overlapping images in the brightest lines between 150 and 400 Å; broadband filtergrams from rockets at 171–400 Å with $\rho \sim 1'$, and at several X-ray bands between 3 and 60 Å with $\rho \approx 5''$; and single-line raster scan images in bright lines, 300–1300 Å, with $\rho \approx 1'$. The tremendously important results of these observations emphasize the urgency of imaging the smaller solar features with better angular resolution. Any improvement in $\rho$ is sure to be important, especially in single-line and Lyman continuum (of H) spectroheliograms. Since the active regions are among the more interesting features, continuous series of observations with $\Delta t$ between 10 sec and 1 hr are also important. The priorities are as follows:

(1) Obtain EUV spectroheliograms in selected single lines and the Lyman continuum. Exploratory rocket observations are useful, but sequential exposures that show the life histories of active features are the real objective.

(2) Obtain sequential single-line X-ray spectroheliograms from

$\lambda = 300$ Å down to the shortest wavelengths within reach of the technology.

(3) Observe the white-light corona over the longest practical time intervals (up to a full solar cycle). Angular resolution should surpass $\rho = 30''$, and eventually approach 2″. Eclipse observations suggest that this is adequate to show the smallest white-light structures.

(4) Obtain very-high-resolution ($\rho \leq 0.3''$) time sequences throughout the whole spectrum from X-rays through the V regions, in broadband images (V and UV), and in single-line spectroheliograms (whole spectrum).

## PROGRAMS AND SCHEDULES

The scientific priorities of the previous section are the starting point for laying out a practical solar space observing program. Our objective is a specific schedule for the next 10 years for the following items:

- Sounding rockets
- Spacecraft in the 20″ to 1′ class (primarily OSO)
- 5″ (5 arcsec) spacecraft
- 1″ (1 arcsec) space observatory
- 0.1″ (0.1 arcsec) space observatory (the National Solar Space Observatory, abbreviated "NSSO")
- Observations from aircraft (airplanes and balloons)
- Ground-based activities:
  - Ground observation (equipment and operation)
  - Astrophysical theoretical and laboratory research (equipment and operation)
  - Technology aimed at solar research

We make the distinction here between "spacecraft" designed for a specific array of unalterable experiments and "space observatory," a "permanent" facility for observations which may be defined after launch by guest investigators.

To avoid the uncertainties attending future unpredictable budget fluctuations, we propose a program in which the money allotted to NASA's solar physics program each fiscal year is divided between two funds. The first is a small relatively stable Preferred Fund (PF), which is to be funded fully, regardless of the state of the budget. It is expected to support rockets, OSO's, aircraft and balloons, and the ground-based activities. The second is a larger Solar Space Observatory Program (SOP) for the development and flights of the 5″ spacecraft and the 1″ and 0.1″ space observatories. In a given fiscal year the total allotment $T$ is divided between the two as follows:

$$\text{PF (\$M)} = 15 + 0.15T$$
$$\text{SOP (\$M)} = 0.85T - 15$$

(where "M" stands for millions of dollars of 1969 purchasing power). This division reflects our conviction that the PF items are scientifically important, relatively inexpensive, and very vulnerable to annual funding fluctuations. They should, therefore, enjoy a fairly constant level of support. The SOP items, on the other hand, will necessarily consume the bulk of the money and cannot avoid the annual funding fluctuations. We adopt a program structure in which the three space observatory projects are assigned various fractions of the SOP funds which vary systematically as the total, $\Sigma$ (SOP), grows in steps of 100 million (rather than fiscal years). Thus budget fluctuations are absorbed by changes in pace rather than changes in the structure of the program. We hope that the resulting solar physics program will weather the budget fluctuations in good health, and proceed in an orderly manner to the desired ends.

### Preferred Fund

We suggest the distribution of the PF shown in table 1 for the next few years, with the expectation that it will be varied from year to year to meet the current needs and accommodate to changes in budget levels. The important thing is to sustain the less expensive projects efficiently, and regardless of the amount allocated to the solar program, be sure that in continuing programs no breaks resulting in the disbanding of effective research teams occur, and that effective training of a future generation of solar astronomers continues.

We particularly urge that the technology be vigorously advanced. Perhaps the most important single development is a really effective spacecraft to ground video system of a quality and

TABLE 1.—*Distribution of Preferred Fund*

| | *Percent* |
|---|---|
| Sounding rockets | 15 |
| OSO | 60 |
| Aircraft and balloon observation | 8 |
| Ground observations and facilities | 17 |
| Theoretical research | |
| Astrophysical laboratory, equipment and research | |
| Technology | |

capacity sufficient to replace photographic film as a recording medium. Such a system would enormously reduce the per bit cost of solar data, since expensive film recovery and replenishment missions would be eliminated. It would also permit the investigator himself to see what is being recorded and to control the equipment. In addition, there will be much preliminary research required on subsystems for future advanced spacecraft, such things as maintaining the figures and alinements of optical elements when exposed to sunlight in the space environment. This kind of research is a necessary preliminary to the starting of real design of the post-ATM–A 5″ spacecraft, and the 1″ and 0.1″ solar space observatories.

### *Solar Space Observatory Program* [1]

The solar program is now approaching a most important milestone, the manned 5″ spacecraft, ATM–A. The improvement in pointing and the resolution of the observing instruments will open a new era of solar research where, for the first time, we can study the evolution of the white corona and the XUV structures within active centers on the Sun, instead of defining merely their sizes and positions. This will initiate the series of major solar spacecraft that are included in the SOP.

In planning the SOP, we consider it useful to look ahead to the first flight of the 1″ space observatory and recovery of its data. By that time we expect that, in addition to a steady stream of results from the PF projects, the following major spacecraft will have flown, and preparations for the succeeding generations of the solar program made:

5″ (5 arcsec) spacecraft: ATM–A and three later spacecraft.
1″ (1 arcsec) space observatory: First flight in progress ($\lambda > 300$ Å). Construction of second observatory underway.
0.1″ (0.1 arcsec) NSSO: Preparatory technology (under PF) well advanced, but no final design work started.

To assign realistic priorities to these items, the order of magnitude of their costs to the solar program should be known. The planning group in the Office of Space Science and Applications has provided cost figures, but with the warning that they are very uncertain. No real study of any of the post-ATM–A solar vehicles has as yet been made. The estimated costs are $90 million for a 5″ spacecraft, $130 million for the 1″ solar space observatory, and $1 billion for the 0.1″ NSSO (including observational equipment

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[1] See app. I for last-minute second thoughts before press time.

for the whole spectrum in a single spacecraft). We have assumed that successive spacecraft in the 5″ and 1″ classes will increase in cost as the experiments become more sophisticated. The figures we use are those of table 2, which shows the costs of the successive flights, A, B, . . . etc. (The $60 million for ATM–A is the remaining cost to the solar program after fiscal year 1970.) The money for each item is intended to pay for spacecraft development and construction, payload, launch, and postlaunch activities (operation, monitoring, maintenance, etc.). We recognize that these figures are only rough approximates. Unless they are grossly in error, however, they suffice for deciding broadly on a program with the greatest scientific yield.

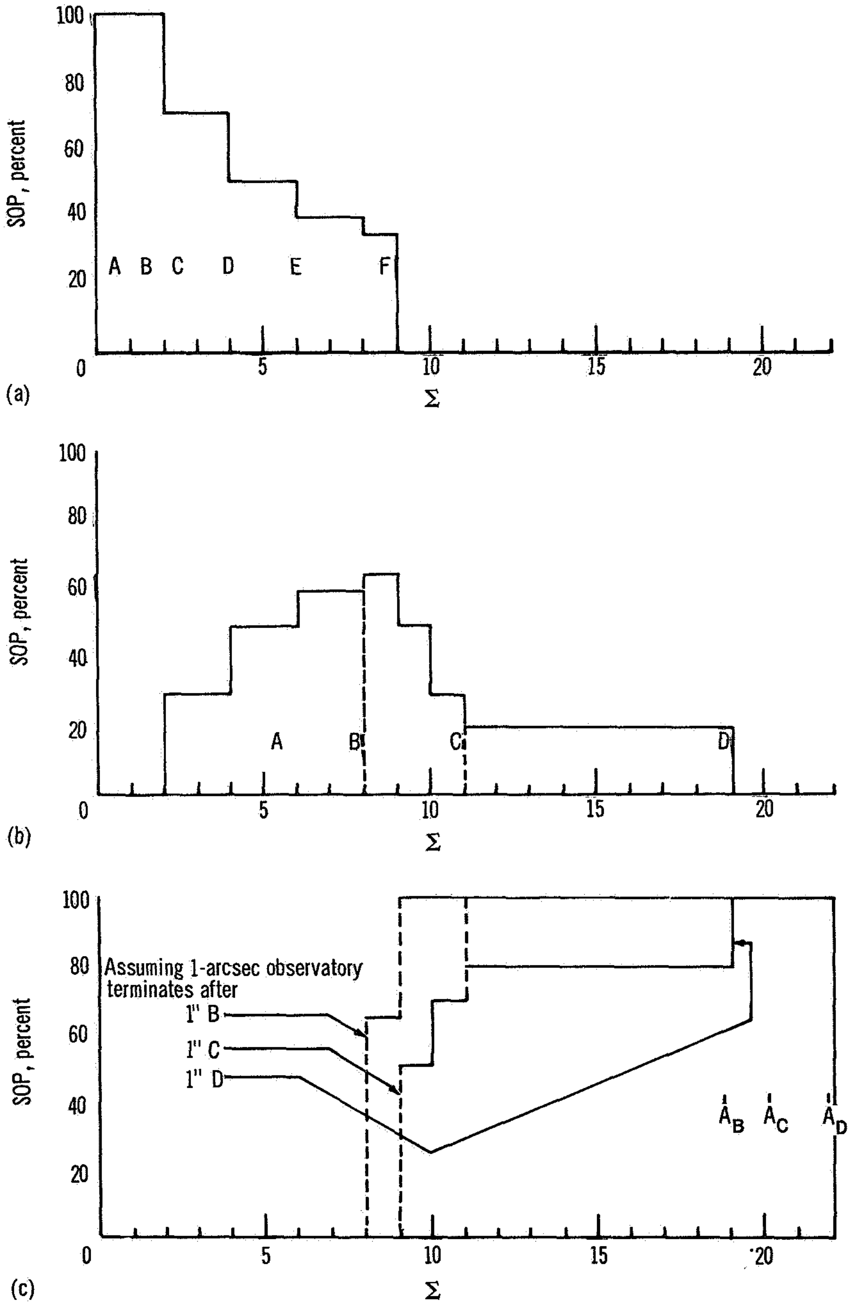
The SOP itself is known graphically in figure 1. The horizontal coordinate $\Sigma$ is the cumulative total of money allotted to the SOP since the beginning of *FY* 1971, in units of $100 million. The ordinates of graphs (*a*), (*b*), and (*c*) represent the percent of SOP funds assigned to the 5″, 1″, and 0.1″ projects at each $100 million step in $\Sigma$. Figure 1(*d*) presents the same information in a single diagram. Below figure 1(*d*) we attach an estimated scale of the total funds accumulated in the whole solar program, labeled $\Delta T$, calculated on the shaky assumption that in an average fiscal year, the SOP will be about 70 percent of the total ($0.7T$), after the PF takes its bite. ($T = \mathrm{PF} + \mathrm{SOP}$) in a given fiscal year. The letters A, B, C, etc., indicate for each spacecraft series the launch $\Sigma$'s, calculated from table 2.

Beyond $\Sigma \approx \$600$ million, we do not take the exact distribution of funds in figure 1 very seriously because of the high probability of revolutionary technical advances before $\Sigma$ reaches that level. They will reduce the cost of data and permit favorable modifications of the whole subsequent SOP. However, to show the later objectives, figure 1 carries the program in visionary fashion to the launching of the 0.1″ NSSO.

The SOP is supposed to be a guide for planning the budgeting and expenditure of solar program funds each fiscal year. For example, at the end of fiscal year 1974, say, $\Sigma = \$420$ million. For

TABLE 2.—*Cost Estimates for Solar Space Observatory Program*

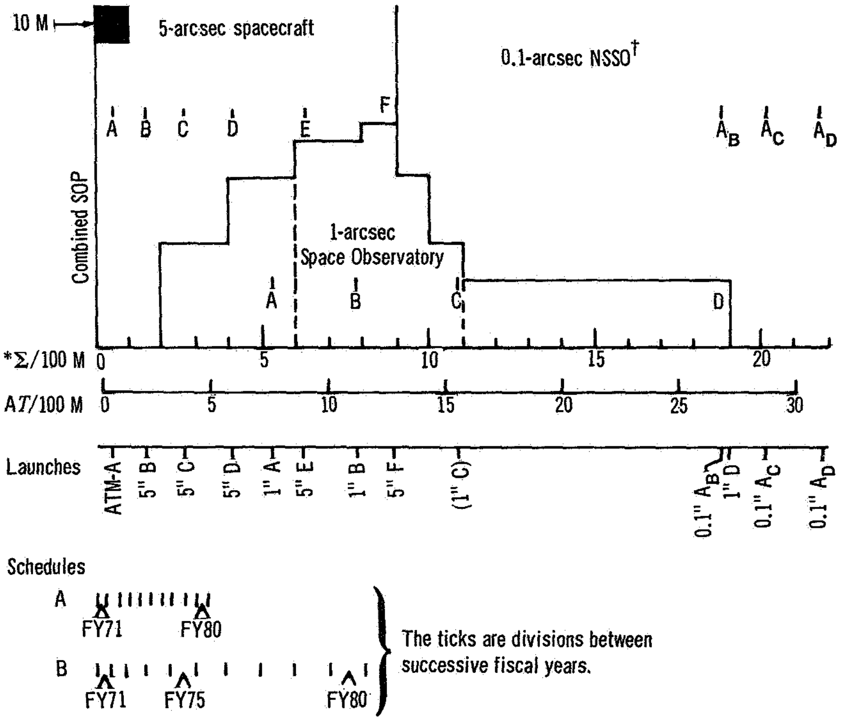
| | A | B | C | D | E | F |
|---|---|---|---|---|---|---|
| 5″ spacecraft | 60 (ATM–A) | 90 | 95 | 100 | 105 | 110 |
| 1″ solar space observatory | 130 | 140 | 150 | 160 | ----- | ----- |
| 0.1″ NSSO | 1000 | ----- | ----- | ----- | ----- | ----- |


SOP, percent
100
80
60
40
20
0
A B C D E F
5
10
15
20
(a)
Σ
SOP, percent
100
80
60
40
20
0
A
B
C
D
5
10
15
20
(b)
Σ
SOP, percent
100
80
60
40
20
0
Assuming 1-arcsec observatory terminates after
1" B
1" C
1" D
$A_B$
$A_C$
$A_D$
5
10
15
20
(c)
Σ

*FY* 1975, the funds allotted to the solar program are $T = \$110$ million, which breaks down into PF=\$31.5 million and SOP=\$78.5 million. In *FY* 1975, therefore, $\Sigma$ advances from \$420 to \$498.5 million, and figure 1 shows for each project the following amounts:

| | | | |
|---|---|---|---|
| 5″: | 50 percent | or | \$39.25 million |
| 1″: | 50 percent | or | \$39.25 million |
| 0.1″: | 0 | or | 0 |

Naturally, it will not be possible to adhere strictly to such a simple system, for a number of highly practical reasons. However, the SOP does indicate the priorities in the long run, and deviations at one time can be corrected by compensating deviations later.



*Σ and A*T* represent cumulative totals of funds in SOP and PF, respectively.

†NSSO $A_B$, $A_C$, $A_D$ show launch points assuming that the 1-arcsec project is terminated after 1-arcsec B, C, or D, respectively.

(d)

FIGURE 1.—Solar space observatory program. (*a*) 5-arcsec spacecraft; (*b*) 1-arcsec observatory; (*c*) 0.1-arcsec NSSO; (*d*) combined SOP.

The thinking behind the division of funds presented in figure 1, shorn of minor subtleties, is quite simple:

(1) Sound specification of the observational objectives, instruments, and spacecraft for a given grade of resolution requires adequate experience with less complex spacecraft. This is unavoidable. It calls for an orderly program of steadily increasing sophistication rather than an immediate jump to the 0.1″ space observatory. The division of funds shows, accordingly, a shift in emphasis from the 5″ spacecraft at the beginning toward the 1″ and 0.1″ observatories as $\Sigma$ increases.

(2) We assume that the scientific yield per dollar generally increases with the sophistication of the spacecraft. This is clearly true of the yield of information bits, especially with the use of photography or an equivalent video system. More important than this, however, is the much greater insight into the physical processes of the Sun which accrues from even modest improvements in quality of the data. There is no way around the necessity for better resolution, for example, if we are to understand the changing phenomena of an active center. Hence economy urges that the solar program advance to the more sophisticated instruments and spacecraft as rapidly as possible.

(3) The time spent in the design and construction of the first spacecraft of a given class should be as short as possible. This is the only protection from obsolescence of major components. Accordingly, the SOP shows a fast start for each class, rather than slower growth from a modest beginning.

(4) It may be possible to maintain the 1″ observatories into a ripe old age. Hence it is very difficult to guess how many are desirable after the first two (which are probably required to cover the whole spectrum). Since the third and later 1″ flights would come in the highly conjectural future, we equivocate by showing a schedule for 1″ C and D, and three launch points for the 0.1″ NSSO corresponding to termination of the 1″ launches after 1″ B, C, or D.

### Solar Program Schedules

Scheduling the activities in the solar program by fiscal year calls for an estimate of the funding each year. Once this is assumed, the distribution of funds each year is determined by writing a scale of dates at the appropriate $\Sigma$'s on figure 1. In response to instructions from the AMB, we consider schedules A and B resulting, respectively, from low and high funding levels for a 10-year period beginning with fiscal year 1971: The suggested low and high funding levels average $57 million and $113 million per year.

TABLE 3(*a*).—*Schedule A (Low Funding)*

[M=million]

| Fiscal year | 71 | 72 | 73 | 74 | 75 | 76 | 77 | 78 | 79 | 80 | 10-year total |
|---|---|---|---|---|---|---|---|---|---|---|---|
| Annual funding (M) | 57 | 57 | 57 | 57 | 57 | 57 | 57 | 57 | 57 | 57 | 570M |
| Rockets | 3M/yr → 12–15 rockets/yr | | | | | | | | | | 30M |
| 5″–30″ OSO | ------- | I | ------- | J | K | ------- | L | M | ------- | N | 140 |
| 5″ spacecraft | ------- | ATM–A | ------- | ------- | B | ------- | ------- | C | 23M | 24M | 295 |
| 1″ space observatory | ------- | ------- | ------- | ------- | ------- | ------- | 10M | 10M | 10M | 10M | 40 |
| 2.1″ NSSO | ------- | ------- | ------- | ------- | ------- | ------- | ------- | ------- | ------- | ------- | 0 |
| Aircraft and balloons | 2M/yr | | | | | | | | | | 20 |
| Ground activity | 4.5 M/yr | | | | | | | | | | 45 |

TABLE 3(*b*).—*Schedule B (High Funding)*

[M=million]

| Fiscal year | 71 | 72 | 73 | 74 | 75 | 76 | 77 | 78 | 79 | 80 | 10-year total |
|---|---|---|---|---|---|---|---|---|---|---|---|
| Annual funding (M) | 65 | 75 | 85 | 100 | 115 | 125 | 135 | 140 | 145 | 145 | 1130 |
| Rockets | 3.5M | 3.5 | 3.5 | 4 | 4 | 4 | 4 | 4 | 4 | 4 | 39 |
| 5″–30″ OSO | H | ------- | I | J | ------- | K | L | ------- | M | N | 146 |
| 5″ spacecraft | ------- | ATM–A | ------- | B | C | ------- | D | ------- | E | 50M | 500 |
| 1″ space observatory | ------- | ------- | ------- | ------- | ------- | ------- | ------- | A | ------- | B | 270 |
| 2.1″ NSSO | ------- | ------- | ------- | ------- | ------- | ------- | ------- | ------- | ------- | 40M | 40 |
| Aircraft and balloons | 2.5M | 2.5 | 2.5 | 3.5 | 4 | 5 | 5 | 5 | 5 | 5 | 40 |
| Ground activity | 6.5M | 6.5 | 6.5 | 7.5 | 9.5 | 10 | 11 | 12 | 12.5 | 13 | 95 |

In schedule A, we assume a constant level of 57M. In schedule B, on the other hand, we assume $65 million in *FY* 1971 with an increase to $145 million in fiscal years 1979 and 1980.

The time scales for schedules A and B are shown as pips at the bottom of figure 1(*d*). The resulting schedules appear in tables 3(*a*) and 3(*b*). The tables show the total annual funding (SOP+PF), the launch dates, and money allotted to projects for which launches are not specified before *FY* 1981.

Even the minimal schedule A shows ATM–A launched in *FY* 1972. We consider this a crucial necessity. Failure to carry out this project would erode confidence in NASA's ability to follow through, disastrously and irreparably. In addition, we firmly believe that no matter how low the funding is, some money should go toward bigger and better things for the future. The relatively large fraction of the total funds assigned to the PF projects in schedule A emphasizes the "lesser means" in these hard times, but we consider it imperative to retain a significant effort ($40 million) in the development of the 1″ space observatory, even though it is not scheduled for flight by *FY* 1980.

## COMMENTS ON THE VARIOUS SPACECRAFT AND THE RESULTS EXPECTED FROM THEM

Schedules A and B call for launches of various spacecraft and certain ground activities. While it is too early to specify payloads, which are, in any event, the responsibility of NASA's Solar Physics Subcommittee, we can say in general terms what data we expect to acquire from the proposed program, and how far along the scientific priority list we should advance.

### Rockets

Solar observations from rockets have provided the first information about the XUV and UV spectrum and have been an exceedingly fruitful source of important data. Now, the more powerful Aerobee 170 and 350 rockets are about to become operational, equipped with a system providing three-axis stabilization and solar pointing with a stability of at least ±10″ and perhaps approaching ±1″. They make possible a new generation of solar experiments that will provide an order of magnitude advancement in solar XUV and UV research. Of course, rockets will always suffer from the handicap of a short time for observation. In this respect they cannot compete with the OSO spacecraft which is presently unsurpassed for studies of life histories of solar features like flares and prominences and changing activities. Neither can they compete with ATM which will carry instruments too large and heavy for these rockets.

Rocket payloads are recoverable, usually in reusable condition, which allows the use of photographic recording with its enormous information capacity, in a fashion not available to the OSO experiments. These characteristics of rockets very well define their uses during the next few years. They are:

(1) Extend the survey of lines in the solar spectrum to wavelengths shorter than 10 Å, and the continuum shorter than 1.8 Å.

(2) Map the spectrum with sufficient resolution to eliminate instrumental blends at $\lambda < 800$ Å, in active centers and for the quiet Sun.

(3) Obtain line profiles for the brightest lines in active centers and on the quiet Sun, in the range 300–1300 Å and possibly at shorter wavelengths.

(4) Improve the absolute photometry especially at $\lambda = 100$–$1300$ Å. Further work is needed at all wavelengths less than 3000 Å.

(5) Obtain further X-ray images of the Sun with grazing incidence telescopes and filters, or by Fourier-transform methods. It may be possible to improve the resolution to 2″ or 3″.

(6) Obtain further slitless spectroheliograms in single important lines, without image overlap, and with higher dispersion and better angular resolution than those now available.

(7) Continue noneclipse photography of the outer white-light corona (to $R \approx 10$ solar radii) with increased angular resolution.

(8) Record the XUV spectrum at different heights above the limb in the chromosphere and corona.

(9) Study limb darkening or brightening in the 1450–1650 Å region originating in the photosphere-chromosphere temperature minimum.

(10) We should expect an increase in the use of rockets as test platforms for some of the subsystems designed for the more advanced spacecraft.

## OSO

The 1970–75 OSO's will be very important for flare observations. During this time period they will be the only platforms above the atmosphere likely to see any major flares. Unfortunately, the Sun will approach its activity minimum (1975 or 1976) and the probability of catching a large flare during a month-long ATM flight is discouragingly low. An OSO, however, with a duration of about a year, will almost surely see one or two, and can record the spectroscopic characteristics and the life history of each flare as a whole. The planned improvements for OSO's I, J, and K in pointing stability, offset pointing, and data capacity will permit better angular resolution and greatly enhance the value of the flare observations. This work would be performed by scanning spectroheliographs and spectrometers regularly used to record the time development of the active centers and coronal condensations. The following is a systematic list of the information expected from the OSO observations:

(1) Absolute photometry of the XUV spectrum in active centers and the quiet Sun, particularly in regions where the duration of a rocket flight is too short to allow the recording of a sufficient number of photon counts.

(2) Measurements of time variations of line intensities, profiles, and doppler shifts occurring in active centers and possibly in the quiet Sun, for a series of lines of selected ionization potentials.

(3) Measurement of center-to-limb variations in spectral lines and the continuum.

(4) Spectroheliograms (by scanning) in lines outside the spectral regions so far observed, and in fainter lines.

(5) Extend spectral surveys and spectral mapping for active centers and the quiet Sun to wavelength regions that are beyond the rocket capabilities, particularly toward shorter wavelengths and in the high-energy spectrum.

(6) Time variations of line profiles in the radiation from an active region as a whole.

### The More Advanced Spacecraft

SWG believes that in planning the later 5″ spacecraft, and the 1″ and 0.1″ observations, NASA should study the feasibility of operation and control from the ground combined with manned maintenance to prolong the useful lives of these expensive vehicles. We are encouraged by the brilliant success of the control system of the Orbiting Astronomical Observatory, launched in January 1969.

Adequate control involves four primary requirements:

(1) A display showing exactly what solar feature or what part of the solar disk the instrument is pointed to.

(2) The means to point accurately to any desired feature or position.

(3) Means for operating the equipment, once it is pointed.

(4) Transmission of the observational data, either from onboard storage or directly.

The utility of a video system capable of transmitting quantitative data is obvious. It shares with photography the overwhelming advantage of continuously and simultaneously recording the brightness of every point in a field, whether an image of the Sun or a spectrum, but without the disadvantage of requiring film recovery. The potential improvement in speed over the point-by-point scanning methods in use on OSO is of the order of hundreds of thousands at least. We again emphasize the importance of making every effort to develop such a system. Until it is available, however, the recovery and replenishment of photographic film should be included in the function of manned maintenance.

We prefer ground operation for three reasons:

(1) Control from the ground more nearly places the observing equipment in the hands of the observers, who know what is required better than anyone else, and can achieve their results with a minimum of fumbling.

(2) The ground-controlled vehicle can operate continuously when it is in the sunlight, over a long lifetime. A comparable performance by men in space would require at least two operators, with relief at comparatively frequent intervals.

(3) At present, it appears that a ground-controlled vehicle, in spite of its requirement for a very complex control system, will acquire observations less expensively than a manned vehicle.

This preference for ground-controlled operation over manned operation in space is based on current concepts of the technology and costs. Experience with ATM–A and future technical advances could alter the picture completely.

### 5-Arcsec Spacecraft

The step from $\rho = 30''–60''$ to $5''$, with good time resolution over long intervals, will initiate a vastly fruitful new era in solar research. Since the information density in a solar image (bits per square arcmin) is proportional to $\rho^{-2}$, we expect to gain a factor of at least 35. The result will be quite spectacular, both quantitatively and in the visual impression. The flight of the first $5''$ experiments on ATM–A is the most exciting prospect in solar physics today.

Solar XUV images, spatially resolved spectra, and profiles of fainter lines and lines at shorter wavelengths than any presently available, all as functions of time, will give a new grip on the problems of solar activity. It will be possible to determine the densities, temperatures, velocity vectors, and intensity fluctuations of the small features that contain collectively the nonthermal energy of active centers, at levels in the solar atmosphere hitherto inaccessible. Further, the spacecraft are big enough to carry powerful white-light coronagraphs. We expect coronal monitoring to detect outward-moving disturbances along the coronal streamers and determine the quantities of matter involved and phase velocities. Hopefully, the problem of coronal rotation will be solved.

It may also be feasible for the 5″ spacecraft to carry a telescope for snapshot observations of extremely high resolution ($\sim 0.2''$) in the visible spectrum. Studies of the evolution of photospheric granulation and the fine structures of sunspots and faculae then become possible.

These kinds of data will begin to define the mechanisms of the flare phenomenon, nonthermal particle emission, solar wind, upward energy transport, and the rotation of the corona, to mention only a few.

In order of priority, the observations expected from the 5″ spacecraft are as follows:

(1) XUV line profiles of active center features (especially flares) and from center to limb in the undisturbed Sun. The 5″ spacecraft with its larger instruments can measure the profiles in smaller features, for fainter lines, and at shorter wavelengths than the corresponding OSO experiments.

(2) XUV single-line spectroheliograms in time sequences showing the spatial and temporal characteristics of active center details and the chromospheric network in lines of selected ionization potentials.

(3) At wavelengths $\lambda < 30$ Å, high-resolution time sequences of filtergrams.

(4) Monitor the white-light corona over the longest available time intervals, from 1.5 solar radii out to the limit of detectability (probably $< 20\ R_\odot$).

(5) Extend spectral mapping to shorter wavelengths than those accessible from OSO.

(6) Visible ($\lambda = 1500$–$10\,000$ Å) very-high-resolution images and spectra of photospheric and chromospheric structures. This requires a large (>60-cm aperture) telescope with internal fine pointing. (Last in priority only because of its practical difficulty.)

After putting this list down, SWG considered the problem of how many 5″ spacecraft would be necessary to make all the observations called for. We outline in table 4 sample payloads for three spacecraft, including that already defined for ATM–A, that would meet the requirements reasonably well. It should be realized, however, that new observational requirements will develop as the series progresses, and most experiments should be flown repeatedly, at

TABLE 4

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*ATM–A*

1. Scanning spectrometer and spectroheliometer (300–1300 Å)
2. Slitless spectroheliograph, photographic (300–650 Å)
3. Spectrograph, photographic, high $\lambda/\Delta\lambda$ (900–4000 Å)
4. Small field large scale X-ray telescope, and slitless spectroheliograph, photographic (2–60 Å)
5. Large field X-ray telescope with filters, photographic (2–60 Å)
6. White light coronagraph, photographic.

*5 Arcsec Spacecraft B*

1. High resolution ($\rho \sim 0.2''$) internally pointed telescope, with filters and spectrograph (1100–30 000 Å)
2. Spectrometer for line profiles and spectrum mapping (300–1000 Å) [This is probably a full load.]

*5 Arcsec Spacecraft C*

1.–2. Spectrometers for X-ray spectrum mapping and absolute photometry (2–300 Å) [Probably at least two instruments]
3.–4. X-ray line profile spectrometers to measure at least the strongest lines (2–300 Å) [Probably at least two instruments]
5. X-ray imaging instrument (pinhole? Fourier shadowgraph? something else?) ($\lambda < 3$ Å).

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different phases of the solar cycle (unless manned maintenance can keep them functioning efficiently over time periods approaching 11 years).

NASA has approved ATM–A for flight during fiscal year 1972. It will be the first spacecraft for manned astronomical observation, the first to use photographic recording over long time intervals, and the first to provide for an angular resolution of $\rho \leq 5''$. The character of the 5″ spacecraft after ATM–A is still undefined. In addition to ground control, we urge that it have a minimum payload weight and volume of 1500 lb and 12 by 3 by 3 ft. NASA should carefully weigh the relative advantages of using available Apollo hardware for remotely controlled operation, either after a crew has left the spacecraft, or from the beginning of a flight without a crew (or something in between), and the alternative of an entirely fresh start.

### 1-Arcsec Solar Space Observatory

The nature of the 1″ observatory is at present purely conjectural, and should remain so until experience with the first two or three 5″ spacecraft provides guidance. We picture a cluster of space telescopes, which may occupy one or more vehicles, designed to observe images and spectra of the Sun through the whole spectral range from the X-rays at $\lambda < 1$ Å to $\geq 10\,000$ Å. The apertures will probably have to exceed 60 cm to deliver enough energy for high-resolution observations in a reasonable time. (A quantitative video system could drastically reduce this requirement for some purposes, but not all.)

Whatever form the 1″ observatory takes, it will be a most important scientific tool for solar research. The observations for which 1″ resolution is important are much the same as those discussed for the 5″ spacecraft. The information density will be increased by a factor of 20 or 30, and we see a new regime of solar features in active centers and on the quiet Sun. Ground-based observers have found that features that are apparently well defined with 5″ resolution usually break down into ordered arrays of finer details when seen with 1″ resolution. The beautiful structure of parallel fibers in the strands of loop prominences is an example. Furthermore, the fine details show explicitly and directly many of the most important dynamic and magnetic properties of the solar atmosphere and solar activity, things which would be unsuspected in 5″ observations, and can only be inferred vaguely from their statistical effects. We fully expect the XUV features to exhibit the same fine structure as resolution improves, with the same potential for revealing the most basic aspects of solar physics. Of course, 1″ resolution is not the end of the line, but there can be no doubt that one good 1″ observation will show details of the utmost significance that are far beyond the reach of any number of 5″ observations.

### 0.1-Arcsec National Solar Space Observatory (NSSO)

The primary purpose of NSSO is to observe smaller details of the solar atmosphere in all regions of the accessible spectrum. To do this, it must be big. With current sensing devices and optics, the limits of angular resolution in the XUV range are set by the number of photons per second that can be collected from a very small area of the Sun. In the visible region, the diffraction from a finite aperture is the limiting factor. In either case, improvement in resolution will result from increased aperture, regardless of other improvements in reflecting surfaces and sensors which we hope will be forthcoming. We believe that apertures of 1 to 1.5 m are the smallest capable of yielding the substantial step increase in resolution that alone would justify a facility larger than the 1″ solar space observatory.

The results to be realized from NSSO observations cannot be predicted. We repeat our belief, supported by experience and theory, that many of the most important physical mechanisms of solar atmospheric dynamics and nonthermal processes occur in features smaller than any we can presently see. The NSSO will show for the first time details smaller than the photospheric scale height, a critical level in the hierarchy of characteristic sizes of solar features. This kind of resolution, therefore, is probably sufficient to reveal decisively most of the important dynamic elements in the solar atmosphere, and permit unambiguous studies of their evolution in configuration, magnetic field, velocity, and spectrum. The list of features to be examined in this kind of detail is very long, and will doubtless be considerably modified by the launch date of NSSO. For the present, we content ourselves with saying that we fully expect NSSO to yield more per dollar expended, measured in terms of advancing our understanding of the Sun, than any of the smaller spacecraft.

## RECOMMENDATIONS, 1968–69

On the basis of the foregoing considerations, the 1968 Solar Working Group framed five recommendations to NASA. The text of the recommendations is presented below.

*Recommendation 1.*—(*a*) We note with the greatest satisfaction the NASA plan to extend the OSO series through OSO's I, J, and K in the 1971–74 period. (*b*) We urge that the series be further extended to include four additional OSO's in the 1975–80 period. (*c*) The usefulness of the OSO program will be greatly enhanced by the inclusion of spacecraft improvements recommended by the Ad Hoc Committee headed by Dr. Gordon Newkirk. We recommend that the engineering and hardware modifications called for be instituted forthwith, and that they be applied to the OSO spacecraft as early in the program as is practicable. (*d*) The extension of the OSO program well through the next solar maximum deserves a very high priority, and is, in our opinion, worth a 1-year delay in the start on a 5-arcsec spacecraft (after ATM–A) if such a delay is an unavoidable alternative.

*Recommendation 2.*—The next major step in spacecraft development in an orderly program of solar space observation is a spacecraft at the general level of sophistication commensurate with a guiding accuracy of 5 arcsec ($\pm 2.5''$ transverse and $7'$ in roll). This need is met in part by the ATM, and could also be met by an unmanned ground-controlled spacecraft of the 5-arcsec class. Concerning these two possibilities we make the following recommendations:

> We urge that NASA conduct a study at once to determine the relative merits of using the ATM leftover hardware for further solar research after ATM–A, or making a completely new start on a ground-controlled spacecraft of the 5-arcsec class. Included should be consideration of: (*a*) The merit of conducting further research from duplicates of ATM–A (but with different payloads). (*b*) Revisits to ATM. (*c*) The possibility of controlling ATM from the ground for use after the astronauts have left and before a possible revisit. (*d*) The possibility of converting ATM into a fully ground-controlled spacecraft. (*e*) The advantages of a completely new start.

*Recommendation 3.*—We believe that for observation of the Sun, the next step in sophistication beyond the 5-arcsec class spacecraft should be one commensurate with 1-arcsec pointing stability. We recommend that—

> (*a*) NASA begin a full-scale effort to design and construct a 1-arcsec solar spacecraft after the first two 5-arcsec spacecraft have flown.
>
> (*b*) A high priority be assigned to the first launch of a 1-arcsec spacecraft as soon after the fourth 5-arcsec launch as possible.

*Recommendation 4.*—The advance of our understanding of the Sun and the technical experience acquired from the 5- and 1-arcsec-class solar vehicles will set the stage for a National Solar Space Observatory (NSSO) of a size and sophistication compatible with angular resolution of 0.1 arcsec. It will be required for further scientific progress, and the technical means for its achievement will be at hand. While its realization is doubtless 10 to 20 years in the future, it is necessary that NASA consistently include the NSSO in its long-range planning along with the National Astronomical Space Observatory. We believe that Recommendation 6 of the Solar Panel of the 1965 Woods Hole Summer Study, calling for immediate feasibility and design studies of an orbiting telescope of this class, should be modified. We recommend that—

> (*a*) Technological requirements peculiar to NSSO be identified, and the technical research necessary to meet them begin at once, but at a pace which takes due account of expected advances in the state of the relevant arts.
>
> (*b*) The start of firm design of NSSO be timed to take

advantage of experience gained from several flights of 5-arcsec spacecraft and one or two 1-arcsec observatories.

(*c*) The elapsed time between the start of design and the launch date be as short as possible. This is preferable to an earlier start and slower development leading to the same launch date. A later start and fast development permit the incorporation of the most advanced technology with a minimum of design revision, resulting in a less expensive and more capable NSSO.

*Recommendation 5.*—The future vitality of the solar space program is absolutely dependent on a continuous close collaboration with ground-based activities in solar observation, theoretical studies, and astrophysical laboratory measurements. The ground-based activities are quite indispensable and relatively inexpensive (like rocket fuel). We believe it is highly uneconomic and wasteful to neglect them while pursuing an expensive solar space program. We therefore recommend that NASA increase its support for the following:

(*a*) Construction and operation of ground-based solar observing facilities.

(*b*) Theoretical studies relevant to solar physics.

(*c*) Construction and operation of astrophysical laboratory facilities.

## APPENDIX

Comments on the final draft of this report by members of the SWG suggest a change in the 5″, 1″, 0.1″ series of SOP spacecraft. We do not believe that it would lead to any serious immediate alterations in the program. To avoid further delay in issuing an already tardy report, therefore, the Solar Panel chairman appends this short note to indicate that a second edition of the Solar Space Program is in gestation. Its content will be considered fully at an early meeting of the Solar Panel or the full SWG. We emphasize that this note is very tentative and does not presently represent the firm opinion of the Panel.

We note that NASA's cost estimates for the post ATM 5″ spacecraft and the 1″ space observatory are 90M and 130M, respectively. At these rates the cost of three 5″ spacecraft would buy two 1″ observatories. If it is as simple as that, the two 1″ observatories would unquestionably be the better buy. We suspect, however, that NASA and we may not have the same picture of the relative complexity of the two spacecraft, or that the cost of the 1″ observatory is contingent on the experience of having developed the 5″ spacecraft.

We intend to discuss with NASA the advisability of some variant of one of the following alternatives:

(*a*) Eliminate the five post-ATM 5″ spacecraft and substitute an additional two or three 1″ space observatories.

(*b*) Plan on a single basic Intermediate Spacecraft (IS) to replace the 5″ and 1″ categories. The IS would be designed for progressive upgrading from a relatively simple first model (which could presumably be launched almost as soon as a 5″ spacecraft) to the full-fledged 1″ space observatory, or better.

The possible gains might include a saving of money, a more rapid scientific advance, and a consequent more rapid approach to the 0.1″ NSSO.

*Report of the Planetary Astronomy Panel*
*December 24, 1968*
*Revised April 15, 1969*

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# A Program for Planetary Astronomy from Space Telescopes

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## SUMMARY OF MAJOR SCIENTIFIC OBJECTIVES FOR PLANETARY SPACE ASTRONOMY

### Composition and Temperature Structure of the Atmosphere

Spectrophotometry of the bright planets with fairly low-resolution longward of 1800 Å is possible now with rockets. Several technological improvements and the use of small pointed satellites will expand the observable spectral range and provide spatial as well as improved spectral resolution. Such measurements indicate atmospheric constituents not detectable in the visual spectrum. Further, it is likely that Venus and the major planets have hazy atmospheres, so that their thermal structure may be derived from spatial scanning and from combining observations in different spectral regions (which arise from different depths, as in a stellar atmosphere). Albedo measurements over the spectrum provide a basis for understanding the heat budget—for example, we should know the extent to which solar radiation is absorbed or merely degraded to longer wavelengths through Raman scattering. Infrared observations from high-altitude aircraft are needed to examine the exciting questions of internal energy of the major planets.

### Character of Cloud Structure

A cloud model (mainly the optical thickness of an atmospheric scale height) is fundamental to interpretation of spectroscopic measurements yielding compositions, temperatures, and pressures. The nature of the clouds may have a dominating influence on the heat budget. In the absence of in situ measurements, models may be derived by comparing the predictions of various models with the observations, but this procedure requires measurements over widely separated spectral regions, to sort out the gaseous and particulate components.

### Upper Atmosphere and Escape Temperature

Good spatial resolution (at least 1 arcsec) may provide measurements across the disk and off the limb of the planetary corona in Lyman-$\alpha$ ($\lambda$ 1215). If feasible, such measures provide at the minimum a simple measure of the escape temperature. This quantity is important for evolutionary considerations and it provides an important data point for deriving ionospheric structure. The variation of escape temperature with the solar cycle is also an important matter to understand. Other free atoms, such as O and He, may be detectable by scattering in their resonance lines in the far UV.

### Surface or Cloud Detail and Meteorology

Large-aperture instruments in space may eventually provide good spatial resolution, now obtainable only from deep-space probes. Long-period monitoring, in a manner similar to that now used by terrestrial meteorological satellites, would probably be necessary to obtain the essential features of a planet's meteorology.

## SUMMARY OF RECOMMENDATIONS

### Sounding Rockets

Sounding rockets are useful for the spectral region 1800–3600 Å; either high spatial or high spectral resolution (but not both at once) can be obtained. Limited exploratory work is also possible in the resonance-line region (1200–1300 Å). *We recommend* that NASA give increased attention to the recently acquired potential of relatively inexpensive fine-pointed rockets in planning a comprehensive program of planetary research. *We further recommmend* the continuation of an active program in the improvement of accurate pointing controls, and we specifically *endorse* the effort at GSFC to provide improved offset tracking and accurate inertial pointing of sounding rockets, useful for the fainter planets.

### Small Satellites: Use on Planets

Small satellites, optimized for planetary work and scheduled to take advantage of the limited observing opportunities, are essential to a comprehensive planetary UV program. A satellite of the

SAS class, but with three-axis stabilization to about 1 arcmin, should be suitable. *We recommend* that NASA give high priority to development of such a small satellite specifically designed for UV planetary astronomy, with the aim of a first launch in 1973 at the latest.

### Small Satellites: Pointing System

We urge NASA to undertake immediately the engineering development of a pointing system capable of three-axis stabilization to about 1 arcmin for a satellite of the SAS class.

### Use of OAO's

Important planetary observations from OAO's are possible, but opportunities occur only occasionally. Although planetary opportunities should not generally regulate launch dates, *we recommend* that, in planning the observing programs of OAO's, due consideration be given to any planetary opportunities that may occur during the active lifetime of the spacecraft if the particular instrumentation available could provide significant planetary data.

### IR Observations

Infrared spectra of the planets, obtained with telescopes operated in the Earth's stratosphere by means of jet aircraft, approximate the quality of spectra that can be expected from small orbital instruments and are vastly superior to spectra obtained from the ground. Therefore, *we recommend* (*a*) vigorous support of current efforts to utilize existing small airborne telescopes, (*b*) construction of a 36-inch class telescope in a suitable aircraft, to be made available as a national facility, and (*c*) development and operation of new observational, airborne facilities of the maximum feasible size.

### Use of Large Space Telescopes

In the time scale appropriate to the construction and use of a large space telescope, important planetary applications for it will continue to exist, and *we recommend* that these applications be considered in any planning for such an instrument.

## INTRODUCTION—SCIENTIFIC OBJECTIVES OF PLANETARY ASTRONOMY FROM SPACE TELESCOPES

Planetary exploration by unmanned spacecraft that fly nearby, orbit, or land on the planets occupies a major role in the U.S. space program. A summer study, organized by the Space Science

Board, produced an extensive report that proposed recommendations for the future of this program.[1] That report was recently revised and updated,[2] and we subscribe to the general direction for planetary exploration advocated therein.

There has inevitably been an associated upsurge of interest and effort devoted to planetary research in general. The interaction between space exploration and ground-based planetary astronomy has been reviewed by a panel of the Space Science Board, which made recommendations for appropriate expansion of ground-based facilities.[3] Study of the planets is but one dramatic example of how space science and conventional astronomy give impetus to one another.

There is a third and intermediate approach to planetary observations that has been mentioned but not thoroughly discussed in the previous studies. It consists of telescopic measurements of the planets conducted from space immediately above the Earth. This approach we shall call "planetary space astronomy." In our usage, "astronomy" connotes remote observation (specifically from Earth or near the Earth), whereas "exploration" connotes deep-space penetration of the sensory apparatus.

We shall attempt to outline a program of planetary space astronomy that will complement a vigorous exploration program in much the same way that ground-based and space studies complement and interact with one another. The prime objective of this study is to recommend specific actions to be taken in the area of space astronomy to insure a well-rounded planetary program. Hence, we have intentionally restricted the study to optical observations. Observations of some of the planets from space might also be made at long radio wavelengths and with X-rays. However, such survey observations of the planets could be made from astronomical satellites planned primarily for other purposes,[4] [5] and it does not now seem justifiable to plan radio or X-ray spacecraft mainly for planetary monitoring.

Similarly, we emphasize here relatively small telescopes that

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[1] "Space Research: Directions for the Future," report of a study by the Space Science Board, 1965, NAS/NRC Publication no. 1403, 1966.

[2] "Planetary Exploration, 1968–1975." report of a study by the Space Science Board, 1968, NAS/NRC, July 1968.

[3] "Planetary Astronomy: An Appraisal of Ground-Based Opportunities," report of the Panel on Planetary Astronomy, Space Science Board, NAS/NRC Publication no. 1688, 1968.

[4] "Recommended Program in High-Energy Astronomy," report of the X- and $\gamma$-Ray Panel of the AMB, 1969. (p. 16 this volume)

[5] "Low Frequency Radio Astronomy in Space," report of the Radio Astronomy Panel of the AMB, 1969. (p. 102 this volume)

night be specially built for planetary work. We have mentioned riefly what planetary observations might be done with larger rbiting telescopes, designed primarily for other purposes, and this may help provide a frame of reference for evaluating the desirability of having the smaller, specialized instruments.

Useful measurements could clearly be made of any bright comets and asteroids that might appear while an astronomical space telescope is in operation, but again we do not recommend specific instruments for this purpose at this time. Further discussions of planetary observations are given in other AMB reports.[6] [7]

### Fundamental Problems

There is a wide range of fundamental problems concerning the planets that will likely be solved only by in situ measurements by landing probes. For example, it seems unlikely at present that the nature of Martian life or the composition of the clouds of Venus will be clarified appreciably with foreseeable techniques for remote sensing. Nevertheless, the astronomical observation of the planets from above the atmosphere can provide certain data, which we regard as having the first priority, that are essential to the most effective planning of exploratory missions, whether they consist of landers, orbiters, or flyby spacecraft. To other data, of importance in their own right but not obviously required for exploratory planning purposes, we would assign second priority.

The ultimate objective, as a fundamental goal, is an understanding of the origin and evolution of the planetary system, with particular emphasis on the comparative development of life under the variety of conditions that have existed through the solar system. Such an ambitious objective is scarcely amenable to a head-on attack, and intermediate objectives must be focused upon.

Astronomical observations are particularly valuable for detecting compositions and, possibly, ascertaining abundances. Such measurements have strong ramifications on the question of the evolution of a planetary atmosphere. We may cite, as two examples, the recent ground-based discovery of HCl and HF in Venus' spectrum, which seems to indicate rather strongly that the atmosphere is a reducing (i.e., nonoxidizing) one, and the flyby measurements of Venus' atomic-hydrogen corona, which seem to imply a large deuterium/hydrogen ratio (by terrestrial standards).

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[6] "Position Paper on Optical Space Astronomy," report of the Optical Astronomy Panel of the AMB, 1969. (p. 46 this volume)

[7] Report of the Infrared Panel of the AMB, 1969. (p. 77 this volume)

Opening of the ultraviolet spectrum to astronomical observations greatly expands the possibilities for detection of substances not detectable in the visual spectrum. It also complicates somewhat the interpretations in terms of abundances, since most planetary atmospheres are optically thick (but not totally absorbing) in the middle ultraviolet. The data must then be interpreted with radiative-transfer techniques, which yield only single-scattering albedos (or ratios of abundance of the absorbing and scattering matter), rather than absolute abundances above a specified ground or cloud deck. Suffice it to say that the theory for such spectra has received a large amount of attention in recent years, and there seems to be no fundamental difficulty in straightforward interpretation.

In the general neighborhood of 2000 Å, a number of atmospheric gases begin their strong continuous absorption. Detailed studies of the varying albedo in this region give direct spectral measurements of the absorbing substances, as demonstrated by rocket measurements of Venus and Jupiter. Shortward of this cutoff and longward of about 1300 Å, the planets are likely to be nearly black, except for some weak fluorescent scattering. But if long integration times can be used, the identities (and some information on relative abundances) of the fluorescing gases can be ascertained. Again, radiative-transfer theory has been applied to this problem.

In the far ultraviolet, atomic resonance lines (such as those of H, He, N, and O) become detectable. The presence of a strong illuminating source in the solar spectrum, at the Lyman-$\alpha$ line at 1215 Å, makes possible in principle the detection in the far UV of Raman scattering, as contrasted with Rayleigh scattering, which could give indications of molecular constituents.

The infrared offers totally different kinds of opportunities. Certain molecules (e.g., $O_2$, $O_3$, $CO_2$, $NH_3$, $CH_4$), known to be important in various atmospheres, are important in the IR and are sometimes obscured to a greater or less degree from the ground. As with ultraviolet spectra, these absorptions are not always interpretable directly in terms of abundances above a ground, but radiative-transfer theory must be applied. In the general region of the planets' thermal emission, IR spectra are of particular importance to thermal-structure analysis, as well as to abundance determinations. The heat budget of a planet is a matter of increasing interest. With adequate data of this sort, pertaining to various heights in the atmosphere, one may construct thermal profiles (with height) for the atmosphere. A physical understanding of the albedo and thermal emission of a planet as a function of wavelength would then allow definite answers to the intriguing question of internal heat generation in the major (i.e., Jovian) planets. This problem is, of course, central to the whole matter of planetary evolution.

These, then, are some of the outstanding problems directly accessible to astronomical investigation. With improved spatial or spectral resolution, the value of the information will be enhanced, as we shall indicate below. It will often be the case that the highest quality data can be best obtained from exploratory probes. In these cases, the principal value of the astronomical space observations will be the "first look," to provide the initial assessment of what to expect. As we indicated earlier, this objective should normally be considered at least as important as observations which are not particularly relevant to exploratory probes.

### Short-Range Objectives

At the present time, sounding rockets provide an effective means of conducting planetary observations in the ultraviolet ($\lambda < 3000$ Å), although the means of acquiring and accurately tracking a preselected bright planet (or star) have been reliably available only since December 1967. In the infrared, high-altitude aircraft offer remarkable possibilities for expanding the accessible part of the spectrum.

These techniques have certain limitations, which are treated more fully below. On the other hand, their potentialities have not been fully appreciated for observations which can be done without spatial resolution across a planet. Indeed, some prior knowledge of a planetary spectrum is desirable to ascertain the value of making spatial scans in narrow spectral bands. In a region where there is planetary absorption (for example, by $CO_2$ at 15 $\mu$m) or emission (as by H at Lyman-$\alpha$, at 1215 Å), spatial scanning with adequate resolution can provide important information on the vertical (or radial) structure of the relevant portion of the atmosphere.

The use of a planetary probe or a large space telescope to make high-resolution measurements cannot be optimized, nor the expense and effort involved justified, unless there is good reason for expecting the planet to have spectral features that can be so exploited.

The near ultraviolet (say, 1800–3000 Å) has been fairly well explored in a preliminary way for the brighter planets. This region yields albedo information that contains information on the surface or cloud-particle reflectivity, the optical thickness of the atmosphere, and the relative importance of gaseous and particulate components. Absorption continua, especially at the shortward end of this region, may also appear. All these measurements bear on the heat budget of the atmosphere and its thermal structure. For example, ultraviolet albedos of Uranus and Neptune should show to what extent solar radiation is absorbed or merely degraded to longer wavelengths by Raman scattering.

The far ultraviolet ($\lambda < 1800$ Å) is less explored. The reflected intensities are much less than at longer wavelengths, and observations exceeding the 5 minutes or so permitted by current sounding rockets will most likely be needed. The spectra will in most cases be due to resonance and fluorescence scattering in the upper atmospheres and could provide important data not only on compositions but on temperature structures as well.

The infrared spectrum is largely indicative of molecular species in the relatively high-density portion of the atmosphere, but not necessarily near the planetary surface. The submillimeter region holds special promise for observations of the lowest levels of dense atmospheres, but as yet this technique has not been fully exploited even from the ground.

The thermal emission spectra of the planets provide a powerful means for remotely sensing the temperature, pressure, composition, and structure of their atmospheres. Localized and temporal variations such as storms are observable. Physical and compositional properties of the surface material can also be determined.

Taken altogether, the range of spectrum opened by telescopes in space—so far, on rockets and high-altitude aircraft, but possibly supplemented by satellites in the near future—offers much valuable information.

We have emphasized in this report the potentialities of aircraft, sounding rockets, and small satellites, not only because of their availability but because in many instances their use can yield data of great scientific interest and of decisive value for the planning of planetary space probes.

## Long-Range Objectives

Like deep-space instrumentation (either planetary flyby or planetary orbital), Earth-orbital telescopes will have the benefit of freedom from terrestrial atmospheric interference. The space-probe experiments have the advantage of proximity, which offers both sensitivity and spatial resolution. Earth-orbital astronomy has its own advantages that make it a desirable complementing approach. Regular (seasonal) as well as unusual temporal variations require more or less continuous observing opportunity. Earth-orbital studies can be continuous in operation and rather ambitious in instrumentation. At the very least, such remote studies can provide the setting within which the data from space probes must be evaluated. For some measurements, orbiting telescopes may conceivably achieve sensitivities that are not possible in space-probe experiments because of tighter constraints of power and weight on the latter, and they will provide timely responses to phenomena that are not accessible to planetary probes because of rarity of opportunity.

Over the long range, the primary means by which orbital planetary astronomy can best supplement planetary exploration is to exploit the possibility of long-period studies of the planets in spectral ranges masked by the terrestrial atmosphere, with more elaborate instrumentation than can be readily accommodated on space probes.

It is obvious that the light collection, pointing accuracy, and stability should be as high as possible. Weight and communication capabilities should be sufficient to permit such ambitious instrumentation as interferometers and mappers (infrared and ultraviolet). Telescopic design should preferably be appropriate for both IR and vacuum UV applications. These needs can probably be achieved at a performance level sufficient to make Earth-orbital planetary astronomy a fruitful field, both in its own right and as complementary to an exploration program.

## AN ULTRAVIOLET PLANETARY PROGRAM FOR THE IMMEDIATE FUTURE

### Objectives

Atmospheric absorption hinders observations shortward of 3000 Å from balloons or aircraft. The instruments to be considered here are therefore those flown on sounding rockets, on Scout-launched astronomical Explorers (such as SAS), and the current series of OAO's.

Pointing to 1 arcsec or better is required for most work, but this pointing can profitably be done in two stages. If the whole spacecraft is pointed to about 1 arcmin, the secondary mirror of the Cassegrain telescope can readily be controlled to stabilize the image to 1 arcsec, and better performance can probably be achieved. This technique is already proven with sounding rockets and can be extended to Explorer satellites. It may not be necessary for some OAO's and future larger telescopes, in which highly accurate pointing of the whole spacecraft may be provided.

Ultraviolet spectra of planets can be expected to yield three kinds of data about the atmosphere: (1) Albedo variability with wavelength, which indicates the relative importance of the atmosphere, the clouds, and (possibly) the surface in reflecting and absorbing sunlight; (2) absorption bands or thresholds, which may permit identification of major constituents and strongly absorbing molecules, such as ozone; (3) resonant and fluorescent scattering, which has been shown on Earth to permit sensitive detection of a wide range of major and minor constituents.

A study of potential observations of type (3) has been made by Dr. L. Wallace, who has kindly made his results available to us. Table 1 shows his estimates of counting rates to be expected from the various planets with a 20-inch telescope. To estimate the Lyman-alpha scattering by H, the planetary corona was assumed to be similar to the Earth's. This assumption has been verified for Venus by the Mariner V observations. Resonance lines and bands of other atoms and molecules will be much weaker and may require integration times of many hours. The table also shows estimates of the counting rates to be expected in the continuum. Where measurements are lacking, the albedo was assumed to remain constant at the last measured value. Even with this optimistic assumption, the rates expected for 1500 Å are extremely small.

Above 2000 Å, useful work is possible, and is being done, with

TABLE 1.—*Estimates of Planetary Count Rates*

[These are calculated for a 20-inch aperture telescope, an overall system efficiency of 0.04, and a 15-arcsec by 15-arcsec entrance aperture]

| Target | Count rate (per second) | | Continuum count rate (per angstrom per second) | | | |
|---|---|---|---|---|---|---|
| | Lyman-$\alpha$ resonance scattering | Lyman-$\alpha$ Rayleigh scattering | 1500 Å | 2000 Å | 2500 Å | 3000 Å |
| Venus at greatest elongation | 700 | --------- | (30) | (3000) | 20 000 | 500 000 |
| Mars at 150° elongation | 150 | --------- | (10) | (1000) | 4 000 | 20 000 |
| Jupiter at 150° elongation | 13 | 0.1 | (3) | 300 | 1 500 | 8 000 |
| Saturn at 150° elongation | 4 | 0.04 | (1) | (80) | (400) | (2 500) |

TABLE 2.—*Sounding Rocket Characteristics (Present and Near Future)*

| | 150 | 170 | 350 |
|---|---|---|---|
| Altitude (300-lb payload) | 108 statute miles | 153 | 260 |
| Time above 50 miles (300-lb payload) | 275 sec | 368 | 525 |
| Rocket diameter | 15 inches | 15 | 22 |
| Maximum telescope diameter | 13 inches | 13 | 20 |
| Relative light-gathering power for 300-lb payload: (Time) × (telescope diameter)$^2$. | 1 | 1.3 | 4.5 |
| Maximum altitude | 200 statute miles | 220 | 260 |
| Payload weight for maximum altitude | 100 lb | 150 | 300 |
| Time above 50 miles minimum weight | 440 sec | 472 | 525 |
| Maximum payload weight | 400 lb | 400 | 1000 |
| Altitude for maximum payload weight | 85 statute miles | 125 | 150 |
| Time above 50 miles for maximum payload weight. | 214 sec | 314 | 362 |

sounding rockets. At shorter wavelengths, only the most intense resonance lines offer any prospect for observation in such a short time. Serious study of this region will require the availability of long integration times, perhaps several days for a single spectrum. A large, general-purpose spacecraft such as OAO may not be able to afford the necessary time, and unless designed with these problems in mind, it would probably not be optimized for the particular observations required. Planetary work at short wavelengths appears to require a satellite intended for the particular job, and we consider below the use of a smaller, Explorer-class satellite, as well as the OAO's.

### Sounding Rockets

Until the present, the only space telescopes that have actually observed the planets have been carried by sounding rockets. Appendix 1 summarizes the available results and those to be expected in the near future. This vehicle will by no means be superseded by the first orbiting telescopes, and we now discuss its possibilities.

The altitude and observing-time performance of the three sounding rockets that will be primarily used for planetary observations over the next decade are listed in table 2. These rockets are the Aerobees 150, 170, and 350.

The design and operation of an experiment to observe the planets is limited by the ability of the experiment to be "pointed" at the planet to be observed. As a result of experimental requirements by scientists from many fields, but especially by astronomers, stabilization systems have been developed for sounding rockets. The earliest (and still being used) is the inertial three-axis system, which is capable of ±2° pointing accuracies with a ¼° limit cycle. Highly accurate tracking systems have been developed for looking at stars and planets by GSFC and for planets by Kitt Peak National Observatory (KPNO). They both use active "star trackers" and can stabilize the entire rocket or experiment section to accuracies of about 1 arcmin with limit cycles from ±3 to ±25 arcsec (brighter objects produce cleaner error signals which permit better guidance). Accuracy and guidance to better than 1 arcsec have been achieved by using "active elements" within experiments controlled by secondary trackers. The use of secondary experiment trackers to gain extremely accurate pointing will be generally continued in the future because aerodynamic torques at rocket altitudes and unavoidable misalinements have now placed the present pointing capability at a state of diminishing returns. Inertial attitude-control systems are available for sounding rocket work through the European Space Research Organization. Sounding rocket attitude-control systems are also being developed in France and Germany. Other than for the Aerobees there are some attitude-control systems available for the Skylark, Black Brant, and Veronique sounding rockets. Also, there are several attitude control systems designed for looking at the Sun which are not presently useful for looking at the planets, but with some degree of modification might be useful for observing Mercury or Venus.

The use of highly accurate rate integration gyros together with a star tracker to look at a bright star for inertial updating purposes has been considered by the Sounding Rocket Branch at GSFC. This system would have a performance similar to the present tracking systems and could be used to observe the planets Uranus and Neptune, as well as stellar objects, which are too faint for the present generation of star trackers. Such a system could be implemented within a very short period if funds were made available for that specific purpose.

Two general types of planetary observations are possible from sounding rockets: They require spectral resolution and spatial resolution. High spectral resolution and spatial resolution are not possible simultaneously because of the low flux levels and the limited times of flight available from sounding rockets.

The limitations on the use of sounding rockets for observing the planets are based on the maximum size of possible telescope apertures (13 to 20 inches in diameter), the limited times of observation (275 to 525 sec), and the state of the art of ultraviolet photon detectors available. Within the next decade it does not now appear likely that the size and flight time of the available sounding rockets will be greatly increased. There is the likelihood, however, of improved ultraviolet detectors, especially of the photoelectric-image-tube type. Ultraviolet photocathodes, mainly developed as a result of the GSFC detector program, have reached quantum efficiencies of 10 to 20 percent in the 1200–3000-Å region for semitransparent photocathodes, although higher efficiency is possible for bare cathodes. Photographic films have quantum efficiencies much lower, bordering on 1 or 2 percent at the maximum. Films also suffer from the fact that the maximum spectral sensitivity occurs in the blue and near-ultraviolet regions of the spectrum, which is likely to increase contamination by scattered solar light.

Image tubes with high quantum efficiency should push sounding-rocket techniques to their maximum UV sensitivity within the next decade. With image tubes of 10 percent quantum efficiency, spectrographs and telescopes with 20 percent overall optical transmission, maximum telescope diameters of 13 to 20 inches, and flight times of 275 to 525 sec, the minimum detectable signal levels should be 0.04 to 0.2 photon (resolution element)$^{-1}$ sec$^{-1}$ cm$^{-2}$. The use of such image tubes will permit observations with high spectral resolution of the planets in the region 1100–1800 Å. Observations of the planets with low spectral resolution in this spectral region have already begun.

> *Recommendation 1: Sounding rockets.*—Sounding rockets are useful for the spectral region 1800–3600 Å; either high spatial *or* high spectral resolution (but not both at once) can be obtained. Limited exploratory work is also possible in the resonance-line region (1200–1300 Å). *We recommend* that NASA give increased attention to the recently acquired potential of relatively inexpensive fine-pointed rockets in planning a comprehensive program of planetary research. *We further recommend* the continuation of an active program in the improvement of accurate pointing controls, and we specifically *endorse* the effort at GSFC to provide improved offset tracking and accurate inertial pointing of sounding rockets, useful for the fainter planets.

### Astronomical Explorers

As discussed in section II.1, planetary observations below 2000 Å will require long observing periods with an instrument optimized for the purpose. For emission features, wide slits are necessary to give high sensitivity; absorption features require narrow slits. The requirement may be well satisfied by an Explorer-class satellite (such as the current Small Astronomical Satellite (SAS)) capable of pointing to about 1 arcmin for extended periods. Four of the planets are so bright in the visible that they may be optically tracked with no difficulty; the sensitive guiding and pointing system of the OAO is not needed. (For Uranus and Neptune, optical guiding is more difficult; advantage could be taken of inertial pointing, if provided.) Long observing times can make up for the reduced light-gathering power compared with an OAO.

Guiding within the telescope can provide the necessary image stability, and will also permit substantial offsets from the center of the image. Such capability is of interest for mapping the planetary surface; but a more fruitful field is likely to be the observation of atmospheric scattering or emission off the limb. Applied to Lyman-$\alpha$, this measurement gives a direct inference of the exospheric temperature. Such measurements have been made by Mariner V on Venus and are expected from the 1969 Mars missions. The close approach of a planetary probe is needed for very precise measurements; but Earth-orbiting telescopes with fine guiding and good optical resolution can make similar measurements, with somewhat reduced precision, at least for Mars and Venus. Observations from Earth orbit could disclose large time variations of exospheric temperature, which are already known to occur for the Earth.

> *Recommendation 2a. Small satellites: Use on planets.*—Small satellites, optimized for planetary work and scheduled to take advantage of the limited observing opportunities, are essential to a comprehensive planetary UV program. A satellite of the SAS class, but with three-axis stabilization to about 1 arcmin, should be suitable. *We recommend* that NASA give high priority to development of such a small satellite specifically designed for UV planetary astronomy, with the aim of a first launch in 1973 at the latest.
>
> *Recommendation 2b. Small satellites: Pointing system.*—We urge NASA to undertake immediately the engineering development of a pointing system capable of three-axis stabilization to about 1 arcmin for a satellite of the SAS class.

### The Use of OAO's

For planetary work, the present OAO program has two limitations. The less serious is that the spectroscopic instruments are not optimized for the measurement of faint emission in resonance lines of planetary atoms across the disk and off the limb. More important is the availability of planetary targets during the useful lifetime of the spacecraft. The launch dates are not chosen to provide planetary opportunities; Mars and Venus are available only one-third of the time, and Jupiter one-half. Table 3 shows the situation for the currently expected OAO launch dates. For each spacecraft and planet, the table gives the probability of not being able to make observations, and also the same probability on a random basis. The working lifetime of each OAO is taken as 6 months; the result would be more favorable for 12 months. Both calculations give essentially the same result: The overall probability, for the whole of the present OAO program, of encountering all three bright planets, is only about one-fourth. Moreover, it is not assured that any OAO will be able to point close enough to the Sun to observe Venus at all.

Nevertheless, OAO–A2 could make highly valuable albedo measurements, especially of Uranus and Neptune in the range 2000–

TABLE 3.—*OAO Program and Planetary Observations*

[Probability of planet not being observable within 6 months of launch. Random probabilities shown in parentheses]

| OAO | Proposed launch date | Venus | Mars | Jupiter |
|---|---|---|---|---|
| A2 | December 1968 | (*) | (*) | ---------- |
| B | Late 1969 | 1.0 (2/3) | 0.5 (3/4) | 1.0 (1/2) |
| C | Late 1970 | .5 (2/3) | 1.0 (3/4) | 1.0 (1/2) |
| D | Late 1971 | 1.0 (2/3) | .0 (3/4) | .5 (1/2) |
| Product | --------------- | .5 (0.30) | 0 (.42) | .5 (1/8) |

Probability of successful observation of all 3 planets: 0.25 (schedule); 0.36 (random).

* As this report was being completed, plans were being formulated by NASA to observe Mars, Jupiter, Saturn, and Uranus in December 1968 or January 1969, possibly Neptune and Venus somewhat later. The observations by OAO–A2 will be limited to broadband photometry.

3300 Å, and the powerful spectrometers of the later OAO's should be used when possible for planetary work.

> *Recommendation 3: Use of OAO's.*—Important planetary observations from OAO's are possible, but opportunities occur only occasionally. Although planetary opportunities should not generally regulate launch dates, *we recommend* that, in planning the observing programs of OAO's, due consideration be given to any planetary opportunities that may occur during the active lifetime of the spacecraft if the particular instrumentation available could provide significant planetary data.

## AN INFRARED PLANETARY PROGRAM FOR THE IMMEDIATE FUTURE

Ground-based observations are limited by three properties of the terrestrial atmosphere: absorption, emission, and turbulence. If the telescope is carried above the troposphere, nearly all turbulence is avoided and both emission and absorption may be greatly reduced at many wavelengths in the infrared where water vapor is the source of opacity. After a brief review of past and current programs which used balloons and jet aircraft as stratospheric observing platforms, we will suggest what is needed for the future.

Stratoscope I proved that high-resolution photographs can be made with balloon techniques. Stratoscope II, a 36-inch high-resolution telescope, was designed to extend this program to fainter objects. Another large balloon-borne telescope, the Polariscope operated at the University of Arizona, has been used to make UV polarization measurements of planets and stars. The problem in the UV is to overcome ozone absorption which is only partially reduced at the highest balloon altitudes. One of the earliest attempts to use balloons to overcome absorption in the infrared was made by John Strong's group at Johns Hopkins in their spectroscopic studies of Venus in the near IR. At AFCRL a balloon-borne IR telescope is used to observe the Moon. Recently, the French group at Meudon Observatory has made excellent far IR spectra of the Sun.

Interest in using jet aircraft for infrared observations in the stratosphere has greatly increased in recent years. At the NASA Ames Research Center, two aircraft are currently operated for this purpose: (1) a Convair 990, four-engine transport; and (2) a twin-engine Lear Jet. The Convair 990 reaches an altitude of 41 000 feet and has been used for a variety of astronomical observations. Dr. G. P. Kuiper and coworkers at the University of Arizona have succeeded in making near IR spectra of Venus, using the Moon as a comparison source to further reduce the effect of the residual telluric absorption lines. The result was a much lower limit to the amount of $H_2O$ in the Venus atmosphere than had been achieved via ground-based or balloon-borne instruments. The Arizona group has succeeded in flying both interferometers and grating spectrometers, attaining a resolution of 60 000 with the latter type of instrument on the Sun. Dr. J. A. Eddy and coworkers at the High Altitude Observatory have recently observed the Sun interferometrically beyond 80 $\mu$m. These are only a few of the IR observations that have been made with the Convair 990. The chief limitation, so far, has been the use of thick windows necessary to maintain cabin pressure.

The Lear Jet flies at altitudes up to 50 000 feet and is a factor of 10 less expensive to operate than the Convair 990. F. Low has constructed a 12-inch-diameter IR telescope that flies in the Lear Jet "open port"; that is, without a window in front of the telescope. With only a few microns of precipitable water above the telescope, the measured sky brightness beyond 15 $\mu$m is about 70° K. Both the spectral bandwidth and limiting sensitivity of this type of instrument are much greater than for ground-based instruments. With the crude pointing system now in operation, the instrument has a bandwidth extending from 2 $\mu$m to greater than 200 $\mu$m and is used to measure bolometric luminosities. Of current interest are direct measures of the total power emitted by Jupiter and Saturn. More detailed spectral measurements will be possible when inertial stabilization to about 1 arcmin has been added to the instrument.

A detailed discussion of the pros and cons of balloons and aircraft is not possible in the present context. The aircraft is far more serviceable and flexible than the balloon, in that fully manned operation is possible and the instruments are generally recovered intact. Since the "open port" technique has been proven, the sole advantage of the balloon seems to be its greater altitude capability. This must be weighed against the problem of contamination by the balloon. If economics are put aside, the overriding consideration is how high in the stratosphere does a given measurement need to be made? To answer this question correctly, many more measurements are needed. The water-vapor content of the stratosphere is still in debate, nor do we yet know the limiting source of noise in the existing instruments.

Simply through the logical employment of existing apparatus and techniques, we can expect observational answers to many fundamental problems concerning the planets. For instance, the internal energy of the planets Jupiter, Saturn, and Uranus and the major sources of infrared opacity in their atmospheres can now be determined along with absolute temperatures at various depths within the atmosphere.

A telescope with a mirror diameter three times larger than the 12 inch now available would permit higher spectral resolution and significant spatial resolution. For this reason, the 36-inch "open port" telescope now undergoing advanced design and study at the Ames Research Center should be supported.

It is difficult to foresee what direction this type of research will take in the next 5 years. The most sensitive infrared telescopes for broadband work will probably be small satellite-borne instruments with cooled optics. These will be of little use for planetary research, which will then be in a position to use both large collecting area and high spatial resolution. An ideal vehicle for such a telescope has been suggested by Robert Cameron of the Ames Research Center: it would be an airship or dirigible capable of nearly continuous operation high in the stratosphere, thus combining the advantages of both balloons and aircraft. Much of the necessary technology already exists and the development cost appears to be less than that of any equivalent orbital vehicle. This concept should be carefully studied over the immediate future as a possibility for infrared planetary astronomy. It would, of course, be of great value in other areas of IR astronomy.

> *Recommendation 4: IR observations.*—Infrared spectra of the planets, obtained with telescopes operated in the Earth's stratosphere by means of jet aircraft, approximate the quality of spectra that can be expected from small orbital instruments and are vastly superior to spectra obtained from the ground. Therefore, *we recommend* (*a*) vigorous support of current efforts to utilize existing small airborne telescopes; (*b*) construction of a 36-inch class telescope in a suitable aircraft, to be made available as a national facility; and (*c*) development and operation of new observational airborne facilities of the maximum feasible size.

## LONG-RANGE PLANNING: PLANETARY ASTRONOMY FROM RELATIVELY LARGE SPACE TELESCOPES

Three aspects of planetary studies are likely to be critically dependent on large space-telescope capability.

(1) In the UV there are problems requiring large light-gathering power or high spectral resolution, or both, such as the search

for atmospheres on smaller bodies and for trace constituents in the denser atmospheres.

(2) Scans of the planets could be made in the infrared at higher spatial resolution than can be obtained from the ground or from aircraft.

(3) Higher optical resolution than can be attained from the ground will be useful in studying diameters and surface details of objects larger than several tenths of a second of arc. High resolution is also important in synoptic meteorological monitoring of entire hemispheres of the major planets over very long periods of time.

In planning large space telescopes, the usual consideration of resolution, control, spectral range, and instrumentation of an LST would automatically render it suitable for the appropriate solar-system problems.

In addition, we note that for solar-system research the UV below the Lyman limit will be very useful. Hence, if possible, mirror reflections and detector capabilities should reach well below 900 Å.

> *Recommendation 5: Use of large telescopes.*—In the time scale appropriate to the construction and use of a large space telescope, important planetary applications for it will continue to exist, and *we recommend* that these applications be considered in any planning for such an instrument.

## APPENDIX 1: ULTRAVIOLET PLANETARY OBSERVATIONS THROUGH 1968

Ultraviolet observations of the planets Venus, Mars, Jupiter, and Saturn have been made from aboard 13 sounding rockets between March 28, 1957 and May 16, 1968. A list of these experiments is included in table A.1. Of all these rockets, six have observed the planets only, two have observed stellar objects in addition to the planets (for calibration and primary research), and five have been free-spinning observations that were fortunate enough to observe the planets among the many stellar targets which were observed. Since early 1967, active star-tracking systems have been available and have been used for all flights but one. Also, nearly half of the observations (6 of 13) were made between May 1967 and May 1968. Of the 13 rocket observations: nine were supported by NASA [the primary experimenters were from Goddard Space Flight Center (GSFC), 6; universities, 2, and a GSFC research associate, 1]; three by Kitt Peak National Observatory (KPNO); and one by the Naval Research Laboratory (NRL).

TABLE A.1.—*Observations of the Planets From Rockets*

| Planet | Date observed and rocket designation | Experimenter | Technique | Reference no. |
|---|---|---|---|---|
| Jupiter, Mars | Mar. 28, 1957<br>NASA 4.04GG | Boggess and Dunkelman | Free-spinning filter photometer (2700 Å, 300-Å resolution) | 1 |
| Jupiter | July 23, 1963<br>Nov. 14, 1964<br>NASA 4.22GG and 4.110GG | Stecher | Spin-scanning objective grating spectrophotometer (2000–4000 Å, 50-Å resolution) | 2 |
| Venus, Jupiter | Aug. 22, 1964<br>NASA 4.126GG | Evans | Objective grating spectrograph (2300–3700 Å, 35–50-Å resolution) | 3 |
| Saturn | Sept. 2, 1964 | Bless, Code, and Taylor | Free-spinning filter photometers (2450, 2800, 2950 Å: 300-Å resolution) | 4 |
| Mars | Mar. 19, 1965<br>NASA 4.57GG | Evans | Objective grating spectrograph (2400–3500 Å, 50-Å resolution) | 5 |
| Mars | Mar. 19, 1965<br>NRL, ND–3.159 | Packer | Free-spinning filter photometers (2150, 2625, 2945, 5390, 5420, 5577, 6245, 6300, 7320, 8350, and scotopic) | 6 |
| Venus | May 4, 1967<br>NASA 4.157GG | Evans | Double-grating spectrograph (2600–3050 Å, 15-Å resolution) | 7 |
| Venus, Jupiter | June 9, 1967<br>KPNO, KP–3.21 | Jenkins and Morton | Objective grating spectrograph (2200–3200 Å, 1-Å resolution) | 8 |

TABLE A.1. (con't)

| | | | | |
|---|---|---|---|---|
| Venus | Dec. 4, 1967<br>KPNO, KP–3.20 | Anderson, Broadfoot, and Wallace. | Scanning Spectrophotometer (1600–3200 Å, 17-Å resolution) | 9 |
| Venus | Dec. 5, 1967<br>NASA 4.197 UA | Moos, Fastie, and Bottema | LiF prism spectrophotometer (1200–1800 Å, 50–100-Å resolution) | 10 |
| Jupiter | May 16, 1968 | Anderson, Broadfoot, and Wallace. | Scanning spectrophotometer (1600–3200 Å, 27-Å resolution) | 9 |
| Jupiter | May 16, 1968<br>NASA 4.221GG | Kondo | Scanning spectrophotometer (1800–4150 Å, 10–20-Å resolution) | 11 |

*References to Table A.1*

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The data gathered have been used to determine the general ultraviolet reflectivity and selective absorption characteristics of the planets Venus, Mars, Jupiter, and Saturn. The problem of the determination of the abundance of hydrogen and helium on Jupiter and Saturn has been addressed by determining the general characteristics of the ultraviolet reflectivities of those planets. The abundance of hydrogen on Jupiter and Venus has been estimated from observations of those planets at Lyman-alpha (1216 Å) and atomic oxygen on Venus has apparently been detected by the detection of fluorescent emission in that planet's spectrum near 1304 Å. Determination of the reflectivity of the planet Mars at wavelengths longer than 2000 Å has led to an evaluation of its surface pressure between 5 and 20 millibars, has established upper limits on the ozone abundance, and has shown that its atmosphere is transparent to ultraviolet radiation in the 2000–3000-Å spectral regions.

Some problem areas have been uncovered by the observations themselves: One is an absorption feature, as yet inconclusively identified, in the spectrum of Jupiter near 2600 Å; another consists of the effects of $NH_3$ absorption in the upper atmosphere of that planet; and a third comprises the general characteristics of the ultraviolet spectrum of Venus (which may eventually be found to be widely variable). Because almost half of the available planetary observations have been made since May 1967, the total results of these observations have not been completely reduced nor fully evaluated.

Observations of Venus, Mars, Jupiter, and Saturn have been made with low spectral resolution. Moderate spectral resolution has been used on Venus, Mars, and Jupiter, and observations with high spectral resolution are available for Venus and Jupiter.

During the 1969 opposition, observations of Mars with high spectral resolution will be attempted. Also during 1969 observations of Venus and Jupiter near Lyman-alpha will be repeated with improved experimental apparatus. Tentative plans for 1-arc sec spatial resolution of Mars, Venus, and possibly Jupiter at low spectral resolution in the middle ultraviolet are being considered for the 1971 and 1973 time period. Several spectrophotometer observations of the planets are tentatively being considered by various experimenters throughout this country, but the short lead-times for sounding-rocket work make these plans very preliminary. One to two years is the time usually necessary for the start and completion of a sounding-rocket experiment, with no more than

TABLE A.2.—*Summary of Planetary Information Obtainable From Sounding Rockets (Present and Proposed prior to 1972)*

| Planet | Type of information * | | | | | | |
|---|---|---|---|---|---|---|---|
| | Lyman-alpha photometry | λλ1000–1800 Å at low resolution, photoelectric | λλ1800–4000 Å at low resolution, filter photometer | λλ1800–4000 Å at 5–50-Å resolution, scanning spectrometer | λλ1800–4000 Å at 30–50-Å resolution, photographic | λλ1800–4000 Å at 0.6–1.0-Å resolution, photographic | High spatial resolution, 1 arcsec |
| Venus | 10, P | 10 | X | 9 | 3, 7 | 8 | P |
| Mars | X | X | 1, 6 | P | 5 | P | P |
| Jupiter | 10, P | 10, 11 | 1 | 2, 9, 11 | 3 | 8 | P |
| Saturn | P | P | 4 | P | X | X | X |

* Number refer to available data (table A.1); "P" refers to experiments that have been proposed or planned by one or more active rocket groups in the United States for a flight prior to 1972; "X" means no information expected at present.

30 days being necessary to prepare one payload, a spectrograph put in the field to observe the comet Ikeya-Seki in 1965.

The type of information concerning the planets that has been gathered and is expected to be gathered prior to the end of 1972 is summarized in table A.2. Mercury, Uranus, Neptune, and Pluto are intentionally omitted from that table: Mercury, because it is always too close to the Sun, and the other three because they are too faint for the present and expected generations of trackers being developed for sounding rockets. However, with star-tracker or sun-tracker update information and the inertial-attitude-control techniques under development by the Sounding Rocket Branch at GSFC, observations of all the planets except Pluto could be obtained.

It seems likely that even if sounding rockets alone were to be used for planetary observations from space, the bulk of the useful observations could be obtained during the 1970's. To the extent that orbiting instruments become available for use on the planets, the sounding rocket as an important planetary instrument may become obsolete somewhat sooner.

## APPENDIX 2: PROPOSED LAUNCH SCHEDULE

Observing opportunities from 1970 to early 1976 are shown in the figure for the four brightest planets. Included are OAO's B, C, and D, according to the present tentative launch schedules, and with a 6-month operating life assumed. The expected arrival times of Pioneers F and G at Jupiter are also shown. The schedule of sounding-rocket launches is necessarily irregular, but the average rate is four per year.

Proposed launches of Planetary Explorers are shown both in figure 1 and in table A.3. The dates are chosen to optimize, with a 6-month lifetime, the availability of suitable targets, and are the best for the 6-year period. Many other dates are possible, but permit observation of only one or two planets.

TABLE A.3.—*Suggested Launch Dates for Planetary Explorers and Their Target Planets*

| | |
|---|---|
| July 1971 | Mars, Jupiter, Saturn |
| August 1972 | Venus, Jupiter, Saturn |
| September 1973 | Venus, Mars, Jupiter, Saturn |
| November 1975 | Venus, Mars, Jupiter, Saturn |

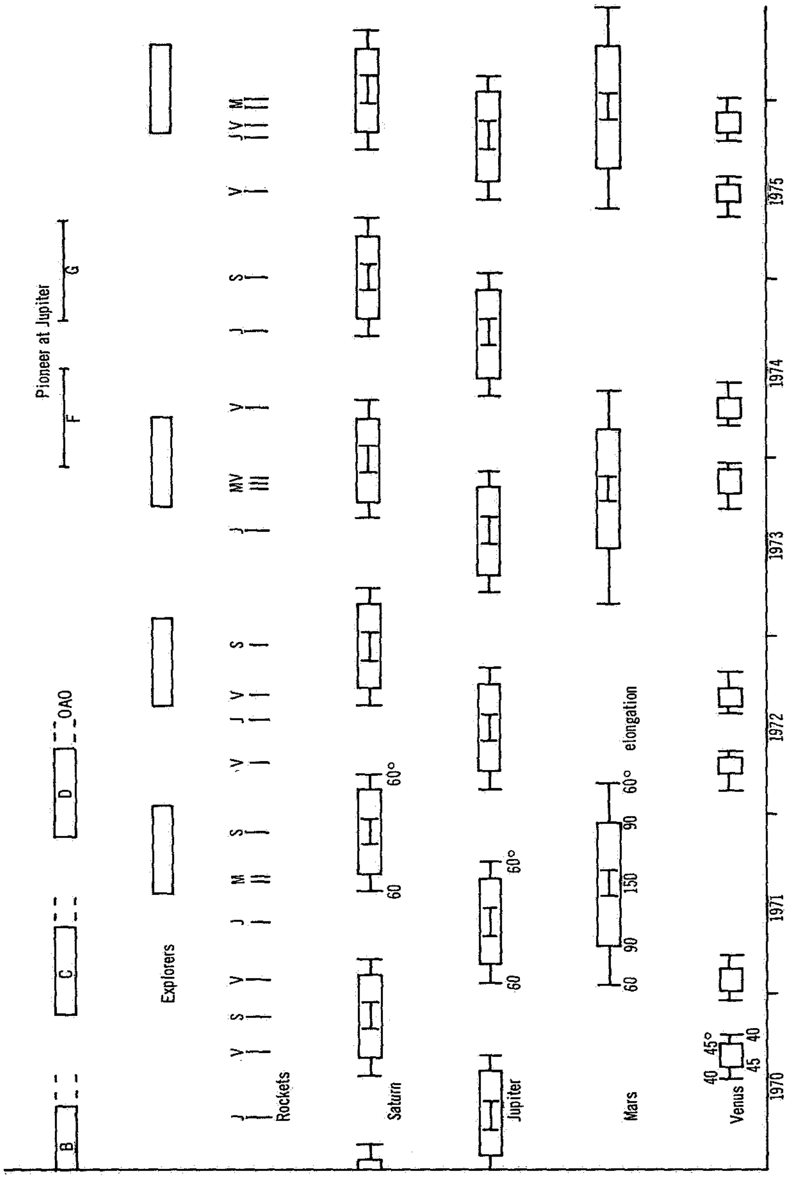

FIGURE 1.—Planetary opportunities through 1975.

*Report of the Fields and Particle Astronomy Panel*
*March 10, 1969*
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# Fields and Particle Astronomy

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## INTRODUCTION AND SUMMARY

The past 10 years have demonstrated the importance of magnetic fields and energetic particles in understanding astrophysical phenomena on all scales from the magnetosphere to the galaxy. Observations within the solar system of magnetic fields and particles with energies from 0.5 keV to many GeV yield information closely related, and complementary, to X-ray, gamma-ray, and radio astronomy. The information has immediate application to such diverse astrophysical problems as the 3° microwave radiation, supernovae, the interstellar medium, the dynamical behavior of the galactic disk, nucleosynthesis and the origin of elements, stellar abundances, etc. *The information from fields and particle experiments is not available through any other kind of observations or experiments.* The exploitation of particle and field observations has become one of the most fruitful directions of astrophysical research at this time. Section II of this report discusses how particle and field observations and experiments contribute to the solution of some of the more outstanding astrophysical problems.

It should be noted that the investigators in field and particle research have provided direction and stimulus to a large fraction of the U. S. science program in space during the past 10 years, and provided leadership in attaining the technical level required for a wide variety of experiments. References are given in appendix I for reviewing the work accomplished in these fields up to 1968. At the present time the instrumentation for particle and field observations has developed to a high level of sophistication, reliability, and compactness, and is progressing rapidly into new concepts for the more detailed observations demanded by present astrophysical questions. The scientists in the field are actively engaged in space research, and they and their technical staffs are working in preparation for the next generation of space observations. The instrumentation for most of the experiments is presently developed and available. The availability of designed experiments and competent scientific investigators eager and available at this time is an important factor in sustaining the vitality of field and particle astronomy in the 1970's at modest cost. Therefore it is essential to establish a program of experiments and observations for the decade of the 1970's which enables the most productive and first-rate investigators to carry out experiments important for the solution of major problems in astrophysics. A program which will fill these minimum requirements constitutes the definition of a minimum program in this field. A minimum program is spelled out in detail and takes account of—

(1) The value of simultaneous observations.

(2) Compatibility of different experiments on a single mission.

(3) The time scale for performance relative to the state of readiness of the experiments and spacecraft design, as well as the phase of the solar activity cycle.

Measurements bearing on the solution of galactic problems, on solar and interplanetary physics, and upon collisionless astrophysical plasma are outlined in table 1 along with information on the state of readiness of the various experiments, recommended space missions, and critical times for their execution. Admittedly, there are many value judgments involved in preparing this report and the Panel recognizes that there are various ways in which these experiments and missions can be combined together to produce a workable minimum program in field and particle astronomy.

In preparing this report, the Panel has recognized the tight budgetary constraints under which NASA will operate in these next few years. Accordingly the philosophy has been to combine into unified missions where possible the mission objectives of field and particle astronomy along with the Lunar and Planetary and/or Space Physics Programs of NASA. *Only under the assumption that fields and particle astronomy missions generally be shared with other programs in NASA will it be possible to hold the minimum program to the modest cost levels which this Panel proposes in this report.* This philosophy is basic to the Space Science Board reports on unmanned spacecraft missions to the planets. ["Report of a Study on Explorations in Space with Sub-Voyager Systems,"

## TABLE 1.—*Field and Particle Astronomy*

[Experiments recommended 1971–80]

| Investigations | Astrophysical importance | Experiment readiness | Spacecraft | Timing | Number of experiments |
|---|---|---|---|---|---|
| *A. Galactic* | | | | | |
| 1. Chemical and isotopic composition of nuclei:<br>For $Z = 1$ to 30 and isotopes H through B: | Information on (*a*) source (supernova, flare stars, etc.) composition and nucleosynthesis, (*b*) interstellar propagation paths and lifetime in the galaxy (*c*) dynamical effect on galaxy<br>(*d*) origins of cosmic rays in the galaxy<br>(*e*) heating of H1 regions<br>(*f*) production of X-rays, $\gamma$-rays, electrons in the galaxy<br>(*g*) separate cosmic ray components with separate and nearby sources<br>(*h*) collisional effects in interstellar space and (a)–(g) above. | | | | |
| *a*. 1–$10^3$ MeV/nucleon | | 1*a*. Available | A1*a*. IMP/Explorer and Galactic/Jupiter Pioneer[b] | Near solar minimum simultaneous with deep space probes | 3–5 (1973–1975) |
| *b*. 1–$10^2$ GeV/nucleon | | 1*b*. Available | A1*b*. IMP/Explorer | Open | |
| *c*. $10^2$–$10^5$ GeV/nucleon | | 1*c*. Development for 1971 | A1*c*. 3–5-ton payload AAP or substitute[a] | Open | 1 preliminary +1 coop. facility |
| For $Z = 40$ to 110: | | | | | |
| *d*. 0.1 to $10^2$ GeV/nucleon | | 1*d*. Beginning development | A1*d*. 1-ton payload AAP or substitute[a] | Open | 2 |
| *e*. Search for antiparticles | | 1*e*. Development for 1973 | A1*e*. 0.2–1-ton payload AAP or substitute | Open | 1 |
| 2. Electrons (±) and energy spectra: | (*i*) origin of cosmic ray electrons, and (*j*) acceleration mechanisms, (*k*) galactic nonthermal radio emission, (*l*) source distribution in galaxy, (*m*) exclusion of galactic center as possible source, (*n*) 3° K microwave radiation for electron energy loss, and (*e*), (*f*) above | | | | |
| *a*. $(e^+ + e^-)$ 1–$10^3$ MeV | | 2*a*. Available | A2*a*. IMP, Explorer | ---------------- | 2–3 |
| *b*. $(e^+ + e^-)$ to $10^4$ GeV | | 2*b*. Developed for 1970 | A2*b*. 0.5-ton payload AAP or substitute[a] | ---------------- | 2 |
| *c*. $e^+/e^-$ | | 2*c*. In development for 1972 | A2*c*. 200-lb payload | ---------------- | 2 |

| Investigations | Astrophysical importance | Experiment readiness | Spacecraft | Timing | Number of experiments |
|---|---|---|---|---|---|
| 3. Interstellar cosmic-ray energy spectra: | (*o*) Energy density of low energy particles, (*p*) origin of low energy particles<br>(*q*) better information on (*a*) through (*q*) and (*r*) measurement of intersellar cosmic ray pressure and its effect on dynamical behavior of gaseous disk of galaxy | | | | |
| *a*. Expt. A1*a* limited to $Z = 1$ to 6 | | Approved starts 1969, A3*a* in development | Mariner[b]<br>Galactic/Jupiter[b]<br>Pioneer to 5 aU | Solar max<br>Solar min | 2<br>4 |
| *b*. Expt. A2*a* | | Approved start 1969 A2*a* and A3*a* | | | 4 |
| *c*. Expts. A3*a* + A2*a* | ------------------------------ | Requires new start A3*c* | 10–30 aU missions (Outer planet missions or "grand tour" type mission) | 1977–79 | 4 to 8 |
| *B. Solar and Interplanetary Physics* | | | | | |
| 1. Solar wind plasma and magnetic/electric fields:<br>*a*. Gross structure<br>*b*. Small-fluctuations<br>*c*. Interface with interstellar region<br>(Note complement of instruments in each experiment group includes plasma, magnetic, and *e–m* wave detectors.) | To understand (*a*) the behavior of active collisionless magnetic plasmas, (*b*) the origin of the small-scale magnetic fluctuations responsible for cosmic-ray modulation, (*c*) the general extent and internal properties of the solar wind, as an example of stellar winds, (*d*) interaction of the solar wind with the interstellar medium, (*e*) effect of the interstellar medium upon solar wind. | Available with modifications | IMP/Explorer at Earth, simultaneous with galactic/Jupiter Pioneer and planetary orbiters[b] | Solar cycle coverage beginning 1971 from 1 to 1.5 aU and beginning 1973 from 1 to 5 aU | 6–8 groups of expts. (See definition of group.) |
| 2. Interplanetary shocks and solar flares | See B(*a*) above and (*h*) and (*k*) below | Available | IMP/Explorer at Earth simultaneous with galactic/Jupiter Pioneer and planetary orbiters[b] at Earth | Near solar maximum | Same experiments as for B1 above |

TABLE 1.—*Field and Particle Astronomy*—Continued

| Investigations | Astrophysical importance | Experiment readiness | Spacecraft | Timing | Number of experiments |
|---|---|---|---|---|---|
| 3. Solar abundances: | (*f*) General abundances (*g*) temperature of solar corona.<br>(*h*) Particle acceleration mechanisms, (*i*) nature of active regions, (*j*) structure and solar origin of interplanetary magnetic field, (*k*) solar-terrestrial relations. | | | | |
| *a.* Solar-wind composition and charge state | | In development | IMP/Explorer | Open | 4 |
| *b.* 10 KeV–1 MeV | | Available | | | |
| *c.* High energy solar particles, Expt. A1*a*, A1*b* (charge and isotopes). | | | | | |
| 4. Search for solar neutrons | (*l*) Nature of solar flare and particle acceleration | In development for 1971 | Solar probe, Sunblazer Earth Orbiter | Near solar maximum | 2 to 5 |
| 5. Solar electrons: 1 keV to 10 MeV | (*m*) Plasma processes in sun and origin of radio bursts | Available >10 keV; <10 keV under development | IMP/Explorer and planetary orbiters [b] | All stages of solar cycle | 6 to 8 |
| 6. Solar cycle modulation of galactic cosmic rays: | (*n*) Complementary information on B1, B2, (*o*) large scale dynamical conditions in solar "cavity"<br>(*p*) Interstellar cosmic ray pressure | | | | |
| *a.* Expts. A1*a*, A1*b*<br>*b.* Expt. A2*a* | | Available | See referenced experiments and outer planet or "grand tour" type missions. | All stages of solar cycle | See referenced experiments |
| *c.* Cosmic-ray intensity gradient inward to 0.3 aU with expts. A1*a*, A1*b*, A2*a* | ------------------------------------------ | Available | Sunblazer, solar probe and Venus/Mercury flyby | | 3 to 6 |
| 7. Structure of solar wind away from ecliptic plane: | B(*a*), (*b*), (*c*), (*o*) above (*p*) transverse cosmic ray intensity gradient (*q*) solar coronal structure | | | | |
| *a.* Expts. B1*a*, B1*b*, physical structure of plasma<br>*b.* Expts. B3*b*, B5.<br>*c.* Expts. B6*a*, B6*b*, cosmic-ray modulation | | Developed | 2 new missions, galactic/Jupiter Pioneer swingby; >30° solar latitude and AU with simultaneous IMP/Explorer, and planetary orbiter [b] | To initiate study over 2 stages through solar cycle beginning 1973–75 | Group of 6 to 8 instruments on each vehicle |

| Investigations | Astrophysical importance | Experiment readiness | Spacecraft | Timing | Number of experiments |
|---|---|---|---|---|---|
| *C. Collisionless Astrophyscial Plasma* | | | | | |
| 1. Bow shock: | The interaction of the solar wind with the magnetosphere provides a laboratory for observing (*a*) the dynamical behavior of collisionless plasma, leading to field annihilation and merging, collisionless shocks, fast particle acceleration, radio emission, etc. | Most instruments available | IMP/Explorer Earth orbiter for missions under categories A and B above can carry instruments [c] for C1 and C2 plus $S^3$ satellites for special aspects, such as clusters for simultaneous observations; also Jupiter flyby. | | Not decided (See recommendations of National Academy of Sciences Conference "Physics of the Earth in Space," 1968.) See also recommendations of Lunar and Planetary Missions Board. |
| *a.* Plasma and magnetic electric fields | | | | | |
| *b.* Electron, proton acceleration (keV energies, fluxes $<10^2/\text{cm}^2$ sec) | | | | | |
| 2. Geomagnetic tail and neutral sheet: | | | | | |
| *a.* Plasma and magnetic/electric fields | To understand (*b*) interaction of solar wind with magnetosphere, and much more generally (*c*) to study experimentally the processes which make up the activity of such objects as the Crab Nebula, flare stars and solar flares, and perhaps radio galaxies. | | | | |
| *b.* Electron, proton acceleration (key energies and fluxes $<10^2/\text{cm}^2$ sec) | | | | | |
| *c.* Barium expts. for convection in neutral sheet | | | | | |

[a] See app. II for Unmanned Substitute Concept.
[b] See also "Planetary Exploration (1968–1975)," S.S.B., National Academy of Sciences, August 1968 (e.g., pp. 5; 42–45).
[c] Requires extra data-processing funds.

Space Science Board NAS–NRC, Washington, D.C., 1967; Planetary Exploration, 1968–75 of NAS–NRC, 1968.]

The Panel notes that the approved NASA program in what we define as Fields and Particle Astronomy is at this time divided among the NASA administrative units of Space Physics, Astronomy, and Lunar and Planetary Sciences. We have prepared our recommendations for the experiments and missions bearing directly on astronomy and astrophysics without regard to their present or future assignments in these various administrative divisions.

In summary, the purpose of this report is to—

(1) Describe the unique role which particle and field observations play in modern astrophysics;

(2) Identify the field and particle experiments to be carried out during the period 1971–80 and indicate their astrophysical importance and present status and development;

(3) Based on the above problems and requirements, outline a field and particle astronomy program which constitutes a minimum level for U.S. science in this field;

(4) Specify the constraints and conditions surrounding the definition of the minimum program;

(5) Describe the nature of an extended program built upon this minimum program during the years 1971 to 1980; and

(6) Finally, include a summary of correlating and ground-based observations and supporting research and development important for the progress of astrophysics at this time in field and particle astronomy.

This Panel report is intended to form a broad outline of a 10-year program in the field funded at a minimum level. The Panel proposes to extract a set of recommendations for each fiscal year beginning with recommendations for fiscal year 1971 to be prepared and updated in the summer of 1969 for Astronomy Missions Board approval.

## PARTICLES AND FIELDS OBSERVATIONS IN MODERN ASTROPHYSICS

With the rapidly advancing technology of the second half of the 20th century, astronomy and astrophysics has expanded outside the classical confines of ground-based optical astronomy, first into cosmic-ray astronomy, then into radio, infrared, X-ray, and $\gamma$-ray astronomy. Observations are now carried out at ground level, at the top of the atmosphere, and in space. It is evident from the new observations, and from the optical identification of the X-ray and radio objects, that high-energy particles constitute a sizable fraction of the total energy of several objects and regions in the universe. Cosmic rays indicate quite generally the important energy density and pressure of relativistic particles in the galaxy.

Cosmic-ray particle studies are related to radio, infrared, X-ray, and $\gamma$-ray astronomy measurements, across the electromagnetic spectrum from the microwave background radiation to megavolt photons, to form an overall picture which would be completely inaccessible from optical and radio studies alone. Already it has become clear that, through couplings between high- and low-energy processes as the inverse Compton effect, any and all of the data related to a given stellar object, stellar envelope, the interstellar medium, or the intergalactic medium, are ultimately related to each other. X-rays are emitted by the hot stellar atmosphere. Radio emission, when observable, is enhanced, being produced by fast particles in the outer atmosphere. Eventually all inputs are needed for a full understanding of the environment considered. High-energy astrophysics not only adds new windows to the cosmic electromagnetic spectrum by providing X-ray, $\gamma$-ray, and particle and field astronomy, but also represents the first scientific unification of those disciplines and all other experimental studies of cosmic processes.

Some of the broad outstanding problems of modern astronomy are the extremely nonequilibrium phenomena, sometimes explosive, such as the possible "big bang" origin of the universe, quasars, stellar explosions (e.g., nova, supernova), and solar flares, in addition to problems of quasi-equilibrium such as galactic dynamics, the interstellar gas, and star formation. High-energy astrophysics is strongly associated with the nonequilibrium processes and also appears to be relevant to the quasi-equilibrium situations. It now appears that pulsars may be a major source of the cosmic rays. Thus the thermal and dynamical state of the interstellar medium may be intimately linked with the pulsar (in the manner described below).

Cosmic rays above $10^{18}$ eV are likely to be extragalactic, propagating throughout the microwave background radiation of the intergalactic medium; the observed spectral behavior of these ultra-high-energy cosmic rays is a measure of their interactions with this medium (e.g., photo-pion production) and places constraints upon estimates of the universality of the microwave radiation field itself. For our galaxy, much of the dynamics appears to be driven by cosmic rays, being a tenuous extremely hot gas of high pressure and extremely high sound speed, so that its effects on the galactic magnetic field and the interstellar medium are large and unlike those of an ordinary gas. The present state of the interstellar medium depends upon the cosmic rays in interstellar space. The cosmic-ray gas probably plays a critical role in forming the galactic halo, in forming interstellar gas clouds, and hence in star formation. Hence, the stellar population of the galaxy evidently depends upon the galactic field and cosmic rays, and can be understood only when the field and cosmic rays have been sufficiently explored.

How do we learn about the cosmic-ray gas? First, direct particle observations near Earth have already indicated some gross spectral features and chemical composition. The observed cosmic-ray spectrum exhibits a broad maximum at $\sim 5 \times 10^8$ eV/nucleon and a power law tail extending to $10^{20}$ eV/nucleon. If one considers the entire spectral interval, it appears that the number flux of cosmic-ray electrons might be comparable to that of the protons, but the energy density in protons exceeds that in electrons by at least two orders of magnitude. The energy density of cosmic rays is comparable to that of the galactic magnetic field. Supernovae seem to be sufficiently energetic and frequent to supply the input required for maintaining the galactic cosmic-ray gas at a quasi-steady-state, balancing losses. The cosmic-ray chemical abundance is richer in heavier nuclei than that of stellar atmospheres, and is possibly indicative of the supernova. The observed abundance of fragmentation products (e.g., $H^2$, $H^3$, Li, Be, B) relative to parent nuclei (e.g., $He^4$, C, N, O) shows that there is an observed component of the cosmic rays which has traversed $\sim 3$ gm/cm$^2$ of interstellar hydrogen during a time of the order of $10^6$ years. The low-energy cosmic rays, below about 100 MeV per nucleon, appear to be dominated by a different class of cosmic ray, presumably from nearby sources in the galaxy. Energetic particles from the Sun give a reliable sample of the relative solar nuclear abundances, so we expect that both the high- and low-energy cosmic rays may be representative of the nuclear abundances of their sources. *Cosmic rays represent the only known material from other stars that comes into the solar system.*

The dynamical effects of cosmic rays, referred to above, arise from two causes. One is their kinetic pressure, which is comparable to the pressure of the galactic magnetic field and to the turbulent pressure of the interstellar gas. The second is the intense heating of the interstellar gas by the low-energy cosmic rays.

The kinetic pressure of the cosmic rays is produced mainly by the relativistic particles ($>10^9$ eV) and is of the order of $10^{-12}$

dynes/cm$^2$. The cosmic-ray pressure is confined by the galactic magnetic field, which in turn is confined by the weight of the interstellar gas. But the confinement is an unstable one, with the gas tending to clump over dimensions of the order of the scale height of the gas, 10$^2$/parsec. Formally the instability somewhat resembles Jeans' self-gravitational instability. But it is caused instead by the gravitational acceleration perpendicular to the disk of the galaxy, coupled with the cosmic-ray and magnetic-field pressures, so that the net effect is several times stronger. In this way the cosmic rays are a major factor in the dynamical state of the interstellar gas, particularly in the tendency toward clumping and the initiation of star formation. *The dynamical state of the interstellar gas is dominated by the cosmic rays and magnetic field and can be understood only on the basis of quantitative observations of the cosmic-ray energy spectrum.*

Observations of cosmic rays below 10 GeV per nucleon are highly developed using modern instrumentation, and most of the detailed spectral and composition measurements are in this range. Unfortunately the density of such particles measured at the orbit of Earth is strongly reduced by the expanding corona of the Sun, so that direct abundance and density measurements must be made for some distance out through the solar system to obtain a reliable determination of the interstellar values. Observations above 10 GeV, where the effect of the Sun is minimal, have until now been inadequate due to the large size and complexity of the instruments necessary to make detailed spectral and composition measurements of such high-energy cosmic rays above the Earth's atmosphere. A program of such high-altitude observations is now underway and should give a relatively unperturbed direct view of the cosmic rays as regards spectrum, chemical composition, sidereal arrival directions (bulk streaming of the cosmic rays), and temporal variations. The bulk streaming past the Sun is directly connected with the dynamical effects of the cosmic rays on the local interstellar gas. If galactic cosmic rays originate in supernovae, then cosmic-ray observations will provide several direct measures of the supernova phenomenon. For example, ultra-high-Z cosmic-ray nuclei such as uranium (recently detected) might be associated with this process.

Both electrons and positrons are produced in about equal amounts as the result of the decay of pions from proton-proton interactions in the interstellar medium. But it has been found that there are far too few positrons to account for the origin of the electron component by nuclear interactions. Hence, electron acceleration in discrete sources such as supernovae remnants, etc., is believed to be the principal source of galactic electrons. Furthermore, the detailed shapes of the electron-positron spectra are important for establishing the electron lifetimes in the galaxy and to account for galactic radio emissions.

There are a number of observational facts that suggest that most of the modulation of the low-energy cosmic rays ($<100$ GeV) occurs within the first 3–5 AU of the Sun. The problem presented by the solar modulation, to applying cosmic-ray observations to the galaxy is, of course, closely related to the studies of the dynamics of the expanding solar corona (solar wind) which is an important part of field and particle astronomy. The solar wind is the one example of stellar winds that is subject to direct observation. The collisionless plasma phenomena which occur in the solar corona and solar wind, in the bow shock at Earth, and in the geomagnetic tail are novel and were largely unanticipated. They relate directly to the universal astrophysical phenomenon of particle acceleration. The inner solar system is the one accessible "laboratory" where they can be studied in detail. So far as other stars are concerned, there is just enough evidence in the spectra of the more active stars to indicate that activity is similar in many respects to solar activity, suggesting that a stellar wind is occurring. *The main hope for understanding the detailed processes is through particle and field studies in the solar system*

Consider the low-energy cosmic rays. Most of the energy content and ionizing power of cosmic rays resides in the relatively inaccessible lower energy portion of the spectrum ($E<10^{10}$ eV). At the orbit of Earth these particles are heavily attenuated through modulation by the solar wind. Thus to get a complete picture of the cosmic-ray gas, we need to measure the associated photons arriving from outside the solar system. For cosmic-ray electrons, this means galactic radio emission. For low-energy nuclear cosmic rays ($E\lesssim 10^8$ eV/nucleon), this means measuring the inner bremsstrahlung X-rays generated by these particles as they ionize the interstellar hydrogen of HI regions. For medium-energy cosmic rays ($E\sim 10^9$ eV), this involves measuring the gamma rays from the neutral pion products of cosmic-ray nuclear interactions with the interstellar medium.

The ionization losses of low-energy cosmic rays seem to be sufficient to heat the interstellar hydrogen to the observed temperature of $\sim 10^2$ °K. This keeps the cool medium partially ionized to the remarkably high extent of $\sim 1$ percent, consistent with observed Faraday rotation and low-frequency radio absorption; it destroys molecular hydrogen and severely limits its possible interstellar abundance. The heated HI clouds of the interstellar medium are expected to glow brightly in Lyman-$\alpha$ and $H_\alpha$, hydrogen atomic radiation characteristic of recombination processes; the heated hydrogen atoms should undergo steady-state cooling by the collisional excitation of infrared emitting levels of several abundant atoms such as O, $C^+$, $Si^+$, and $Fe^+$. These optical and infrared results of cosmic-ray ionization processes involved in the heating of HI clouds are subject to confirmation in the near future, since the photon fluxes are anticipated to be an appreciable fraction of the light of the night sky. The abundance of the elements required for efficient cooling may be measured by observing the corresponding K absorption edges in the spectra of the many X-ray stars distributed throughout the galaxy as well as in the extragalactic X-ray background radiation viewed from various directions.

The diffuse X-ray background is expected to exhibit at least two types of structure. One of these arises from the cloud formations of the interstellar medium of our galaxy; these HI clouds serve as X-ray "scintillators" for cosmic-ray nuclei via the process of inner bremsstrahlung. If the isotropic component of the diffuse X-ray background is due mainly to the superposed contributions from all normal galaxies, then another distinctly measurable structure would result from the $\sim 10^4$ galaxies of the local supergalaxy. Detectors suitable for observing these structural aspects of the X-ray background are now being developed.

Altogether, field and particle astronomy is an important part of modern high-energy astrophysics, illuminating, and being illuminated by, optical, X-ray, and $\gamma$-ray astronomy, and having a direct bearing on a variety of basic astrophysical problems. The thermal state of the interstellar gas, the dynamical state of the interstellar gas, the formation of stars from the interstellar gas, the thickness of the gaseous disk, the galactic halo, the nonthermal background radio emission, the galactic X-ray emission, the galactic $\gamma$-ray emission, the nuclear abundances in stars, etc., are all problems to which observations of particles and fields are essential. In the following section we point out fields and particle experiments—most of which are already developed—which could carry out the program of studies outlined in this section.

## PANEL RECOMMENDATIONS FOR EXPERIMENTS AND SUGGESTED MISSIONS ESSENTIAL FOR FIELD AND PARTICLE ASTRONOMY IN THE PERIOD 1971–80

The Panel has agreed upon a minimum program based on the experiments outlined in table 1 and carried out through individual space missions described in table 2. In table 1 the Panel has identified individual experiments. It has not attempted to specifically identify individual experimenters; however, the Panel has identified work in progress or status of the experiments. In some cases the NASA and the scientific community may decide that a single experiment defined here would most practically be a group of experiments, or, conversely, what has been defined as two or three experiments may, in fact, be accomplished by the design of a single instrument. To illustrate how we have combined our thinking with the preexisting NASA program, we give several examples to avoid misunderstandings.

> FIRST EXAMPLE: On the Pioneer F and G galactic Jupiter probes to the planet Jupiter, experiments of interest to both the Lunar and Planetary Missions Board and to the Astronomy Missions Board are to be included. It is assumed that plasma, magnetic field and particle experiments will be included in these two deep space missions for operation during the cruise mode and during the Jupiter flyby. The Panel has identified two additional missions of this type to be authorized for flight in 1974 and 1975 (Pioneer H and I) or their replacement by a new type of probe capable of more sophisticated work operating over longer periods of time and greater distances in the solar system, so that it may be used for reaching any combination of the outer planets or out of the ecliptic plane. (These are designated as "Outer Planets Spacecraft," or "grand tour" type missions. Missions A and B could replace the Pioneer H and I missions.) It is obvious that the financial support for missions of this type would come primarily from the Planetary Administrative Division of NASA with a portion of the cost being assigned to Physics or Astronomy for those experiments outlined in table 1.
>
> SECOND EXAMPLE: From table 1, Part B ("Solar and Interplanetary Physics"), it is clear that important solar and interplanetary measurements must be made on a deep space probe moving inward toward the Sun. Thus the Panel recommends the establishment of a "new" solar probe capable of operating to 0.2–0.3 AU, with a mission commencing no later than 1976 carrying a selected group of experiments outlined in table 1.
>
> THIRD EXAMPLE: The Panel has identified the requirement for a large automated payload to carry very heavy experiments into Earth orbit. A low-cost suggestion for meeting this objective is described in appendix II. Since the first launch recommendation exists for 1973, it is essential that

the study be completed in 1970 and that the initial funding begin before calendar year 1970. This recommendation has been made, since there is substantial doubt regarding the S4B-type workshops and the AAP program in general. It is recommended that if the Apollo Applications Program can carry these heavy experiments that they be conducted under the ground rules of "unmanned instrument standards." Automated large experiments (ALE) discussed in appendix II could save heavy space science experiments from being postponed indefinitely because of a deferment or lack of an AAP. Indeed, this may turn out to be a more economical use of space technology.

### Rockets and Balloons

The Panel recommends that approximately six spin-oriented rocket flights be carried out each year for special solar particle studies and for testing new experiments which would be flown later in satellites and deep space probes. Furthermore, it strongly supports the need for a continuation of a balloon-flight program of large balloons to carry experiments to within 1 to 3 $gm/cm^2$ of the top of the atmosphere. These programs are included in table 2.

From the above considerations it is clear that the field and particle astronomy experiments mainly are to share part of missions falling under either the Space Physics Program or the Lunar and Planetary Sciences Program as spelled out within NASA. The Panel has cut across these domains to select what are believed to be an essential and unified program of experiments in field and particle research and the regrouping of these experiments into "multipurpose" missions reflects this fact.

### Sharing Costs

In view of these considerations it is especially important to understand the ground rules for determining the cost level of this program to astronomy. As a rough guideline, the Panel considers the astronomy and astrophysics "share" of the NASA program in this area to be determined by rough guidelines for costing as follows:

(1) Ten percent of the planetary missions as outlined in table 2 for the period 1971 through 1980;

(2) Thirty percent of the physics missions in which they participate; and

(3) One hundred percent for missions of purely astrophysical interest.

TABLE 2.—*"Minimum" Program for Field and Particle Astronomy (Astronomy Portion)*

| | 1971 | 1972 | 1973 | 1974 | 1975 | 1976 | 1977 | 1978 | 1979 | 1980 | 1981 | 1982 | 1983 | 1984 | 1985 | 1986 | 1987 |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Explorers: Interplanetary | $\boxed{\text{H}}$ | $\boxed{\text{J}}$ | K | L, M | N, O | P, Q | R, S | T, U | V, W | X, Y | | | | | | | |
| Scout (Sun Blazer) | $\boxed{\text{B, C}}$ | $\boxed{\text{D, E}}$ | F, G | H, I | | | | | | | | | | | | | |
| Pioneers (Jupiter/galactic probe) | | F | G | H<br>↓<br>or | I<br>↓<br>or | | | | | | | | | | | | |
| Outer planets missions * | | | | | | | | | | | | | | | | | |
| (Jupiter/galactic probe or "multiplanet tours)": | | | | | | | | | | | | | | | | | |
| (a) In ecliptic plane | | | | A | B | | | D | E | F | | | (End A?) | | | | (End D?) |
| (b) Out of ecliptic plane near Sun | | | | | or | | | | | (End B?) | | | | | | | |
| (c) Out of ecliptic plane away from Sun | | | | | B | | C | | or<br>E | | | | | | | | |
| (d) Mariner-Mars (or Viking) | $\boxed{\text{A, B}}$ | | C, D | | | | | | | | | | | | | | |
| Solar probes: | | | | | | | | | | | | | | | | | |
| (a) Venus/Mercury fly-by | | | A | | B | | | | | | | | | | | | |
| (b) German/U.S. solar probe | | | | A | | | | | | | | | | | | | |
| (c) "New" solar probe to 0.2 AU and closer approach | | | | | | A | B | C | D | E | | | | | | | |
| Large automated experiments (or Apollo applications with unmanned instrument standards) | | | A | B, C | D | | E | F | G | H | | | | | | | |
| Planetary Explorers: | | | | | | | | | | | | | | | | | |
| (a) Venus | | A | B | | D | | | | | | | | | | | | |
| (b) Mars | | | C | | E | | | | | | | | | | | | |
| Rockets | 6 | 6 | 6 | 6 | 6 | 6 | 6 | 6 | 6 | 6 | | | | | | | |
| Balloons, large | 20 | 18 | 15 | 15 | 15 | 15 | 15 | 15 | 15 | 15 | | | | | | | |
| SR&T | | | | | | | | | | | | | | | | | |

* This program based on flyby-type missions without capsule landers.

☐ Letters in boxes indicate approved missions as of March 1969.

As a test of what the above constraints on the share of the astronomy and astrophysics program mean, we may use the 1969 NASA program in the area of fields and particles as a test case. Under these conditions we believe that approximately $15 million per year is being spent on topics which are defined in the area of astronomy and astrophysics out of the total funding in the 1969 budget. These figures generally include a share of data analysis S, R, and T and launching expenses.

Although the Panel members are not entirely knowledgeable or competent to evaluate the final costs of the program proposed, the Panel is convinced, on the basis of the experience over the past several years, that this program will be modest. For example, it is the Panel's estimate that for the period 1970–75 the average per annum cost of the Fields and Particles share of this program in astrophysics will be approximately $30 million to $40 million per annum, including the automated large payload experiment partly assigned to Fields and Particles (and partly assigned to X-ray or $\gamma$-ray astronomy or high-energy particle physics). Thus it is seen that the proposed minimum program for the early 1970's is an effort approximately $25 million above the level of the 1969 budget. These figures for the minimum program should be taken as only a rough indication and must be further evaluated by NASA management. In order to make clear what the Panel means by dollar estimates, the Panel defines costs in terms of 1968 value in purchasing technologies, goods, manpower, etc.

### Magnetosphere Physics

The Panel emphasizes its continuing interest in magnetospheric experiments as indicated in table 1, part C. However, these experiments fall primarily in the province of the Lunar and Planetary Missions Board, since they are in the planetary environment. Many of the interplanetary experiments proposed in this Panel report would apply to magnetospheric studies of interest to the Lunar and Planetary Board. The Panel therefore recommends that some contact be established between the two boards to explore common areas of interest.

## RECOMMENDATIONS BEYOND THE MINIMUM PROGRAM

In the preceding section the Panel has outlined what it considers to be a minimum program for field and particle astronomy over the decade of the 1970's. The Panel emphasizes that a withdrawal of experiments and missions below this recommended level certainly means that experiments of substantial value and investigators now contributing heavily to this field must drop out of this area of space science. The Panel further emphasizes that the minimum program allows very little growth for the addition of new, young investigators into these fields of research. Accordingly, the Panel recommends that NASA enhance this minimum program at the earliest possible date. An enhanced program should include:

(1) The addition of planetary orbiters and/or planetary capsule landers for the outer planets carrying field and particle experiments;

(2) An additional interplanetary explorer each year after 1972;

(3) One or two additional deep space probes out of the ecliptic plane;

(4) Additional "new" solar probes, possibly shifting the initiation of the program to an earlier date;

(5) Additional planetary explorers for the inner planets; and

(6) An increase in the number and variety of large automated payloads in the mid- and late-1970's.

An enhanced program should go a long way toward providing for the interests of the next generation of investigators in this field and for meeting the mission requirements of unexpected and totally new directions in field and particle research.

## RECOMMENDATIONS TO THE ASTRONOMY MISSIONS BOARD—NASA

The 1968 report of the Fields and Particles Astronomy Panel resulted in the Astronomy Missions Board accepting and endorsing a series of recommendations which establish a beginning for the minimum program discussed in this report. For example, the recommendation to study a means to develop an automated large experiment payload satellite was approved and endorsed by the Board. The 1968 Panel recommendations are reproduced here, together with some new recommendations which have not yet been acted upon by the AMB.

The Panel on Particle and Fields Astronomy recommended that the AMB approve:

(1) The augmentation of NASA's present field and particle program (as of FY 1969) by the experiments and additional missions outlined in table 1 for the period roughly identified as 1970–1975.

(2) That the emphasis on experiments in Pioneer F and G be of the type outlined in table 1, and that two additional missions of this type be authorized in the 1970 FY budget for launch in 1974–75.

(3) That field and particle experiments be carried on the Mariner 1971 mission, planetary orbiters, and other missions whose primary objective is planetary investigation: specifically, we support the program in planetary orbiters discussed in the report of the SSB of the National Academy of Sciences entitled "Planetary Exploration" (1968).

(4) We recommend that the AMB support the philosophy contained in the 1967 and 1968 SSB reports emphasizing the importance of combining the objectives of field and particle research in interplanetary space with missions having a planetary objective. This will reduce the load on tracking facilities of the deep space network and reduce the cost of preparing missions and data reduction. *Indeed, it is absolutely essential to combine the interplanetary observations in cruise modes to the planets if costs during the next six to eight years are to be kept within manageable proportions.*

(5) That a study be undertaken to find an alternate to the AAP for carrying heavy unmanned payloads in the area of field and particle astronomy, along the lines suggested in Appendix II.

(6) That programs complementary to the field and particle astronomy program be strongly supported. In particular, we identify the small astronomy satellite program (SAS) for X-ray and $\gamma$-ray astronomy which should be under way concurrent with the field and particle program since these investigations are complementary in character.

(7) A series of supporting studies as outlined in table 3 including space experiments, ground-based or balloon programs, and specific laboratory experiments.

(8) That plans be made towards initial funding in FY 1971 for deep space missions to 10 to 20 astronomical units.

In summary, we recognize the over-riding importance of continuing programs for small spacecraft and experiments in the early 1970's. The major objectives can be accomplished by adding a few simple larger missions, ALE, to the complement of small spacecraft. When facilities in a manned work shop or space station become available we can make good use of them.

In addition, we now submit for approval by the Astronomy Missions Board the following recommendations:

(1) The overall program as outlined by experiments in table 1 and by the missions in table 2 is designated as the

minimum program for field and particle astronomy by the Astronomy Missions Board, being a research level below which there will be serious losses of essential new results and the loss of first rate scientific talent.

(2) NASA is urged to enhance this minimum program at the earliest possible date with part or all of the following additional programs carrying field and particle experiments:

(*a*) The addition of planetary orbiters and/or planetary capsule landers for the outer planets carrying field and particle experiments;

(*b*) An additional interplanetary explorer each year after 1972;

(*c*) One or two additional deep space probes out of the ecliptic plane;

(*d*) Additional "new" solar probes, possibly shifting the initiation of the program to an earlier date.

(*e*) Additional planetary explorers for the inner planets;

TABLE 3.—*Supporting Research and Technology*

A. Supporting or Correlating Measurements for 1970–75:

| | |
|---|---|
| In space | 1. X-rays. |
| | 2. $\gamma$-rays. |
| Ground-based or balloon | 3. UV and IR sky surveys. |
| | 4. Solar Observations (e.g. continuous solar magnetographs. |
| | 5. Flare star patrol, optical and radio. |
| | 6. Intensity-time variations from neutron monitors. |
| | 7. Balloon measurements of high-energy cosmic-ray spectra and nuclear species ($Z \sim 40$ to 100 in energy range $10^{10}$–$10^{12}$). |
| | 8. Accelerators for totally stripped nuclei in charge range to $Z \approx 28$ and to energies of $\sim 100$ MeV/nucleon. |
| | 9. Measurements of cross sections for nuclear reactions leading to production of critical nuclear species such as isotopes of H, He, Li, Be, and B, and the nuclei C, N, O, F, etc., over a wide energy range. |
| | 10. Calculations on relative abundances from stellar envelopes, supernovae, etc., under various initial conditions. |

B. Sounding Rockets (see table 2).

C. Balloon Flights (see table 2).

(*f*) An increase in the number and variety of large automated payloads in the mid- and late 1970's.

An enhanced program should go a long way toward providing for the interests of the next generation of investigators in this field and for meeting the mission requirements of unexpected and new directions in fields and particle research.

(3) The supporting research and technology (SR&T) required for continuance at a high level of originality and preparation for future research should be supported. This implies an SR&T level of support of approximately 15 percent added to the cost of the minimum program. (It is further recommended that supporting or correlating measurements for the period 1970 to 1975 be included which will greatly enhance the quality of the research in field and particle astronomy. Supporting research activities are indicated in table 3.)

(4) In view of the interplanetary observations required for missions extending approximately 5 to 10 years and over great distances, it is necessary that deep space network facilities be assigned early so that these observations may be carried out essentially continuous in time and uninterrupted by requirements for other kinds of missions, etc. This policy may require the establishment of additional radio telescopes for the deep space network in the 1970's beyond the three units of 210-foot radio dishes now authorized.

## APPENDIX I: GENERAL REVIEWS OF FIELD AND PARTICLE ASTRONOMY IN SPACE, 1958–68

The following reviews provide bibliographies to the original research papers and additional review articles:

1. Schardt, A. W.; and A. G. Opp: Particles and Fields Significant Achievements. *Revs. of Geophysics*, vol. 5, pp. 411–446 (1967).
2. *Ibid*:. Particles and Fields: Significant Achievements 1966. NASA SP–155 (1967).
3. Fichtel, C. E.; and F. B. McDonald: Energetic Particles From the Sun. *Ann. Rev. Astron. and Astro.*, vol. 5, p. 351, Annual Reviews, Inc., Palo Alto (1967).
4. Parker, E. N.: Galactic Effects of the Cosmic Ray Gas. (1967), Space Science Reviews (to be published).
5. Particles and Fields Research—A Bibliography With Author Index. W. N. Hess and G. D. Mead, (eds.), NASA SP–7026 (1966).
6. Physics of the Earth in Space, Space Science Board, National Academy of Sciences, 1968.
7. Ginzburg, V. L.: The Astrophysics of Cosmic Rays. *Scientific American*, Feb. 1969, p. 51.

## APPENDIX II: AUTOMATED LARGE EXPERIMENT (ALE) CONCEPT

The field and particle experiments which have been flown on various missions are typically of the order of 5 to 15 lb. With this type of experiment it has been possible to carry out definitive studies of the charge and energy spectra of low-energy galactic cosmic rays, study the solar wind, and measure the interplanetary magnetic field. There is much that remains to be done within this class of experiment on the I mission outlined in table 1. Furthermore the evolution of IMP and OSO spacecraft capability is such that experiments of the 75- to 100-lb category could be proposed.

However, as our technology has developed, it has become possible to consider new experiments in the 1000–5000-kg category. There are many vital questions which can be answered only by very large apparatus. Typical examples are the search for gamma-ray point sources, the precise "shape" of our galaxy as defined by gamma rays, the measurement of the very-high-energy cosmic rays and their nuclear interactions, and the study of the cosmic-ray charge spectrum in the lead-uranium region. The technology for doing these experiments now exists, but the corresponding launch opportunities do not become available until the advent of large "workshops" sometime after 1976. Our sophistication in developing fully automated experiments is such that it is now vital to consider the alternatives to the large workshops.

We propose a simple pallet system which would be launched into an ~400-km circular orbit by a Centaur or Titan III–C launch vehicle. Depending on the experiment weight, one could consider one or two experiments per launch. It is probable that each would have its own pallet or platform. Thus the experiments would be separated in space, removing the possibility of experiment interference. The early system would use gravity-gradient stabilization combined with a precise method of determining aspect. This is a passive system, but most of the celestial sphere would be swept out in a 4- to 6-month period. It appears that the $S^3$ satellite data system would handle most experiments if combined with a larger tape recorder and that the $S^3$ command system is adequate.

The power requirements would be on the order of 20 to 30 W for the spacecraft and 40 to 100 W for the experiment.

The concept is to use systems which have already been developed to provide launch opportunities. Except in the mechanical area, very little new development would have to be done to provide a suitable spacecraft.

This would provide opportunities for many of the proposed EMR experiments as well as those previously listed.

# III

# AMB LONG-RANGE PLAN FOR SPACE ASTRONOMY

## PROCEDURE FOR ESTABLISHING PRIORITIES

Space astronomy consists of a number of subdisciplines. These subdisciplines have a common goal: to increase human understanding of the universe through space experiments. The subdisciplines differ greatly, however, in their approach to this goal, both as regards the instruments they employ and to a certain extent as regards the astronomical objects and phenomena they concentrate on. No objective method is known to us to determine in advance the relative effectiveness of these various approaches toward achieving the common goal. The history of science has taught us in this connection just one general lesson: no single approach will succeed by itself. Thus the Board has found it one of its hardest tasks to establish priorities for the programs of the individual subdisciplines of space astronomy. To fulfill this task the following procedure was carefully developed, step by step, with the persistent aim that the result of this work should correspond to the most open-minded point of view of space astronomy as a whole and that it should be as free as humanly possible from the personal preferences of the individual Board members.

The original charge by the Board to its panels, each responsible for one of the subdisciplines, was to study and compile the basic scientific aims in its area as well as the scientific instrumental developments and actual space experiments necessary to fulfill these aims. The consequent work of the panels resulted in separate reports reproduced in the preceding chapter. When most of this work was accomplished, the Board requested from each panel, first, a summary of the scientific highlights spotlighting the central scientific aims of each subdiscipline and, second, a program of a logical sequence of space experiments designed to fulfill these central aims.

Furthermore, these programs were to be presented each on two separate levels of effort. The higher level was defined as representing a program permitting optimum scientific progress; however, without undue technological risks and with sufficient time between experiments for effective exploitation of scientific results from experiment to experiment. The lower level was defined as corresponding to a program just on the limit at which no fundamental scientific opportunity within the subdiscipline had to be abandoned and where, accordingly, the danger of losing top scientists from the space-astronomy program would still be minimal. Finally, the panels were requested to express their minimum and optimum programs in terms of hypothetical flight missions, solely for the purpose of making possible a quantitative assessment of the efforts required by the programs in the separate subdisciplines. Specification of the hypothetical missions and mission objectives enabled the Board members to evaluate the observations, the associated astrophysical problems, and the expected overall scientific contribution of the mission to progress in astronomy. It was only on this scientific basis that the subdiscipline programs could be weighed against one another and priorities assigned.

Thus, although specific mission descriptions are simply exemplary, and are not intended as concrete mission definitions, taken as a whole the subdiscipline flight schedules do provide a good measure of the total effort which will be required to carry out the space-astronomy programs proposed for the decade ahead.

When this material was substantially completed in written form, the Board received presentations of the subdiscipline programs from the panel chairmen and thus had a final opportunity for detailed discussions of these programs with the experts. Immediately thereafter it was agreed that each Board member should take overnight to individually assign relative priority ratings to all the subdiscipline programs. Without getting involved in the actual numbers used to assign quantitative priorities, we can illustrate how a priority assignment affected a given program. Individual members effectively were able to vote to speed up or slow down the rate at which the subdiscipline program will be carried out. The individual votes cast ranged from voting to eliminate a particular subdiscipline effort entirely to speeding up a program by a factor of 3. Nevertheless, when the averages were computed, it was a great surprise to all involved how small the scatter of the individual priority ratings around the averages turned out. Perhaps the long months of detailed discussions, not at all without disagreements, had broadened the view of all members regarding the total entity of space astronomy, thus overcoming to a large degree the natural tendency of each member to overvalue the particular subdiscipline in which he happens to be personally most interested and active.

And this result was not because the votes were simply randomly spread about the requests of the subdisciplines. In one case the program voted by the Board was at twice the rate of the program requested by the subdiscipline.

Still more importantly, the balance struck by the Board differed quite significantly from the current balance between the subdisciplines in the NASA program for fiscal year 1969, and thus indicated some significant new directions into which the space astronomy program should turn as soon as possible.

## MECHANISM FOR ASSEMBLING AN AMB PROGRAM FROM THE SUBDISCIPLINE SCHEDULES

The average priority assignments were simply used to scale each subdiscipline program, speeding it up or slowing it down to fit into the coordinated AMB overall program, with the following exceptions. In those cases where the change in the subdiscipline program was greater than 20 percent up or down, the chairman of the subdiscipline panel was asked to reconsider his program at the rate voted by the AMB, and judge whether a simple scaling of the program was scientifically justified. In a couple of the cases of such relatively large changes, the panel chairmen decided to redistribute the emphasis within their subdiscipline programs at the new level of effort, rather than make a simple stretch out or speeding up of their programs.

## THE TWO AMB LONG-RANGE PLANS

Before presenting the final mission schedules, we should briefly define each of the long-range plans—describing the characteristics that the AMB hoped to impart to these composite programs.

### The Minimum Balanced Program

The definition of a minimum program, especially the concept of a sharp break below which progress becomes substantially more difficult if not impossible, is often an exercise fraught with the possibility of misunderstanding. We therefore emphasize the significance of the AMB minimum balanced program as that level below which one or more of the subdisciplines of astronomy must be dropped to maintain the others above their minimum thresholds of efficient and scientifically profitable operation.

Such a negative step would then seriously undermine a central assumption of the Board's planning; namely, that the agreement between the subdisciplines on the most important astrophysical problems requires an orchestration of the multiwavelength observing programs. This assumption is that for many problems a few relatively unsophisticated, and sometimes less expensive, measurements in different wavelength regions might lead to a deeper understanding of the physics of a process—than a most beautiful detailed picture achieved at great cost at a single wavelength. This is not to say that in some cases the narrow, highly specialized approach might not produce the essential, even indispensable, key. It is simply a judgment of the Board about the requirements for greatest progress in most problems before us at the present time. We should also note in passing that the Board found it was often these problems requiring a multiwavelength, multidisciplinary approach, which are the problems attracting so many physicists and scientists from other disciplines to come to work in modern astrophysics.

A timely example of the lengths to which scientists will go to achieve the completeness of the multiwavelength approach, and an illustration of the uses to which the future capability in space astronomy will be put, is the current standby alert trying to catch the brilliant flash of a flare in the X-ray star, Sco XR–1. Starting in May, astronomers at Cerro Tololo Observatory in Chile have been continuously measuring the visible radiation, watching for the onset of a flare. At the first sign of activity, they will radio other astronomers at Caltech where the 200-inch Mount Palomar telescope will make infrared observations, in Hawaii where a rocket will be launched to record the X-ray spectrum, and at Goddard Space Flight Center where University of Wisconsin experimenters will turn the Orbiting Astronomical Observatory's ultraviolet telescopes toward the X-ray star. Since the Australians recently discovered Sco XR–1 to be a strong variable at radio wavelengths, a large steerable radio telescope may also be used to complete the wavelength coverage. The great hope is to achieve several measurements whose combined value in terms of scientific understanding might greatly exceed the combined cost relative to that of any one of the measurements standing alone.

### The Optimum Program

Although the concept of an optimum program is usually less controversial than a minimum program, the Board wants to take pains to stress that this is not simply a program in which the subdisciplines all are encouraged to do their maximum unconstrained programs. First, it was composed from a set of subdiscipline maximum programs where the principal constraint, or upper limit, was the projected availability of excellent people—scientists and supporting teams of specialists—to carry out the recommended missions. Second, the level of the resulting optimum Board program was about 20 percent below the sum of the maximum subdiscipline programs. Finally, the Board's priority assignments again raised and lowered the levels of the individual programs to accomplish a unity and balance with prospects for greatly multiplied combined benefits.

## FLIGHT SCHEDULES

On the following pages we present the schedules of space astronomy missions which implicitly contain the best judgment of the Astronomy Missions Board concerning the present optimum balance of effort between the various subdiscipline programs, as those programs were described in part II. *These schedules cannot be adequately interpreted without recourse to the subdiscipline reports where the observational programs and mission objectives are developed.* We present here a brief description of the structure of the schedules and a glossary of terms and abbreviations which will assist the reader in referring back to a particular subdiscipline report for further information.

The minimum balanced program flight schedules are shown in table 1 (Astronomy Missions) and table 2 (Space Physics and Interplanetary Missions). The optimum programs are shown in tables 3 and 4. The general plan of the Astronomy Tables 1 and 3 shows increasingly expensive types of missions arranged vertically in successive blocks. Each block contains spacecraft which are approximately a factor of 2 more expensive than the preceding block. Within each block one row is given to each of the subdisciplines, with one exception (X—X and $\gamma$-ray, O—Optical UV and IR, R—Radio, S—Solar, P—Planetary).

## GLOSSARY OF TERMS

**ALE**—Automated Large Experiment. *See* Heavy Explorer below.

**ASTRA**—Intermediate step (1.5-m diffraction limited performance) to LST. *See* Optical Panel Report.

**ATM**—Apollo Telescope Mount. *See* Solar Panel Report.

**Heavy Explorer**—A new spacecraft with general sophistication (telemetry, pointing, etc.) of an Explorer but with a payload capacity in the 1- to 5-ton range. *See* X- and Gamma-Ray and Particles and Fields Reports. Also called High Energy A by X- and Gamma-Ray Panel.

**LST**—Large Space Telescope. *See* Optical, IR, and Planetary Panel Reports

**NASO**—National Astronomical Space Observatory. The collection of space astronomical facilities of the 1980's—LST, 0.1" solar, 5–10-m X-ray, etc.

**OAO**—Orbiting Astronomical Observatory. *See* X- and Gamma-Ray, Optical, IR, and Planetary Panel Reports

**ORO**—Orbiting Radio Observatory. A 10-km electronically filled aperture antenna. *See* Radio Panel Report.

(*Glossary continued on p. 233.*)

## TABLE 1.—*Minimum Balanced Program—Astronomy*

| Fiscal year | 1971 | 72 | 73 | 74 | 75 | 76 | 77 | 78 | 79 | 80 | 81 | 82 | 83 | 84 | 85 |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| IR airplane | Continuous—Fly best available platforms—$3M/yr effort | | | | | | | | | | | | | | |
| Sounding rocket flights: | | | | | | | | | | | | | | | |
| X | 15 | 16 | 17 | 18 | 22 | 24 | 26 | 28 | 29 | 31 | 33 | 35 | 37 | 39 | 40 |
| O | 12 | 12 | 12 | 12 | 12 | 12 | 12 | 12 | 12 | 12 | 12 | 12 | 12 | 12 | 12 |
| R | 1 | ------ | 1 | ------ | 1 | ------ | 1 | ------ | 1 | ------ | 1 | ------ | 1 | ------ | 1 |
| S | 8 | 8 | 8 | 8 | 8 | 9 | 9 | 9 | 9 | 9 | 10 | 10 | 10 | 10 | 10 |
| P | 3 | 3 | 3 | 3 | 3 | 3 | 3 | 3 | 3 | 3 | 3 | 3 | 3 | 3 | 3 |
| Explorers: | | | | | | | | | | | | | | | |
| SAS X | 1 | 1 | 1 | 2 | 2 | 2 | 2 | 2 | 2 | 2 | 2 | 2 | 2 | 2 | 2 |
| SAS O | ------ | 1 | ------ | ------ | 1 | ------ | ------ | 1 | ------ | ------ | 1 | ------ | ------ | 1 | ------ |
| RAE R | ------ | ------ | 1 | 2 | ------ | ------ | ------ | ------ | 1 | 1 | 2 | ------ | ------ | ------ | ------ |
| S | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ |
| SAS P | ------ | ------ | 1 | ------ | 1 | ------ | 1 | ------ | 1 | ------ | 1 | ------ | 1 | ------ | 1 |
| "Heavy explorer": | | | | | | | | | | | | | | | |
| X | ------ | ------ | ------ | A | ------ | ------ | ------ | ------ | B | ------ | ------ | ------ | ------ | C | ------ |
| O | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ |
| R | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ |
| S | ------ | OSO–I | ------ | OSO–J | OSO–K | ------ | OSO–L | OSO–M | ------ | OSO–N | OSO–O | ------ | OSO–P | OSO–Q | ------ |
| P | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ |
| 10-km radio 5-arcsec solar: | | | | | | | | | | | | | | | |
| X | ------ | ------ | ------ | OAO–E[a] | ------ | OXO–A | ------ | ------ | ------ | ------ | ------ | OXO–B | ------ | ------ | ------ |
| O | ------ | OAO–D | ------ | OAO–E | ------ | ------ | OAO–F | ------ | ------ | OAO–G | ------ | ------ | ------ | ------ | ------ |
| R | ------ | ------ | ------ | ------ | ------ | ------ | ORO–A | ------ | ------ | ------ | ------ | ------ | ------ | ORO–B | ------ |
| S | ------ | ATM–A | ------ | ------ | 5″–B | ------ | ------ | 5″–C | ------ | ------ | 5″–D | (b) | ------ | 5″–E | (b) |
| P | ------ | OAO–D[a] | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ |
| ASTRA 1-arcsec solar | | | | | | | | | | | | | | | |
| X | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ |
| O | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ASTRA–A | ------ | ------ | ------ | ------ | ASTRA–B | ------ | ------ |
| R | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ |
| S | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ |
| P | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ |
| 5–10-meter LST 0.1-arcsec solar | | | | | | | | | | | | | | | |
| X | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ |
| O | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | LST→ |
| R | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | |
| S | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | 0.1″→ |
| P | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | ------ | |

[a] Partial use.
[b] Evolves to effect 1″ observations in 1980's.

TABLE 2.—*Minimum Program for Fields and Particle Astronomy (Astronomy Portion)*

| | 1971 | 1972 | 1973 | 1974 | 1975 | 1976 | 1977 | 1978 | 1979 | 1980 | 1981 | 1982 | 1983 | 1984 | 1985 | 1986 | 1987 |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Explorers: Interplanetary | $\boxed{\text{H}}$ | $\boxed{\text{J}}$ | K | L, M | N, O | P, Q | R, S | T, U | V, W | X, Y | | | | | | | |
| Scout (Sun Blazer) | $\boxed{\text{B, C}}$ | $\boxed{\text{D, E}}$ | F, G | H, I | — | — | — | — | — | — | | | | | | | |
| Pioneers (Jupiter/galactic probe) | — | F | G | H | I | | | | | | | | | | | | |
| Outer Planets Explorer * | | | | ↓<br>or<br>A | ↓<br>or<br>B | | | | | | | | | | | | |
| (Jupiter/galactic probe or "multiplanet tours)": | | | | | or<br>B | | C | D | E | F<br>(End B?) | | | (End A?) | | | | (End D?) |
| (*a*) In ecliptic plane | | | | | | | | | | | | | | | | | |
| (*b*) Out of ecliptic plane | | | | | | | | | | | | | | | | | |
| (*c*) Mariner-Mars (or Viking) | $\boxed{\text{A, B}}$ | — | C, D | | | | | | | | | | | | | | |
| Solar Probes: | | | | | | | | | | | | | | | | | |
| (*a*) Venus/Mercury Flyby | — | — | A | — | B | | — | — | | — | | | | | | | |
| (*b*) German/U.S. solar probe | — | — | — | A | — | — | — | — | | — | | | | | | | |
| (*c*) "New" solar probe to 0.2 AU | — | — | — | — | — | A | B | C | D | E | | | | | | | |
| Large automated experiments (or Apollo Applications with unmanned instrument standards) | — | — | A | B, C | D | — | E | F | G | H | | | | | | | |
| Planetary Explorers: | | | | | | | | | | | | | | | | | |
| (*a*) Venus | — | — | A | — | B | — | — | — | — | — | | | | | | | |
| (*b*) Mars | — | | — | | C | — | D | — | — | — | | | | | | | |
| Rockets | 6 | 6 | 6 | 6 | 6 | 6 | 6 | 6 | 6 | 6 | | | | | | | |
| Balloons, large | 20 | 18 | 15 | 15 | 15 | 15 | 15 | 15 | 15 | 15 | | | | | | | |
| SR&T | | | | | | | | | | | | | | | | | |

* This program based on flyby-type missions without capsule landers.

☐ Letters in boxes indicate approved missions as of March 1969.

## TABLE 3.—*Optimum Program—Astronomy*

| Fiscal year | 1971 | 72 | 73 | 74 | 75 | 76 | 77 | 78 | 79 | 80 | 81 | 82 | 83 | 84 | 85 |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| IR airplane | Continuous—Up to 120-in. telescope—$5M/yr effort | | | | | | | | | | | | | | |
| Sounding rocket flights: | | | | | | | | | | | | | | | |
| X | 25 | 27 | 30 | 32 | 35 | 37 | 40 | 42 | 45 | 47 | 50 | 50 | 50 | 50 | 50 |
| O | 15 | 17 | 20 | 22 | 24 | 24 | 24 | 24 | 24 | 24 | 24 | 24 | 24 | 24 | 24 |
| R | 1 | | 1 | | 1 | | 1 | | 1 | | 1 | | 1 | | 1 |
| S | 15 | 15 | 15 | 15 | 15 | 15 | 15 | 15 | 15 | 15 | 15 | 15 | 15 | 15 | 15 |
| P | 5 | 4 | 4 | 3 | 4 | 5 | 5 | 5 | 5 | 5 | 5 | 5 | 5 | 5 | 5 |
| Explorers: | | | | | | | | | | | | | | | |
| SAS X | 2 | 2 | 2 | 2 | 2 | 2 | 2 | 2 | 2 | 2 | 2 | 2 | 2 | 2 | 2 |
| SAS O | | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 1 |
| RAE R | | 1 | 2 | 1 | 1 | | 2 | 1 | | 1 | 2 | 1 | | 1 | 2 |
| S | | | | | | | | | | | | | | | |
| SAS P | 1 | 1 | 1 | | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 1 | 1 |
| "Heavy Explorer": | | | | | | | | | | | | | | | |
| X | | A | | B | | | C | | D | | | E | | F | |
| O | | | | | | | | | | | | | | | |
| R | | | | | | | | | | | | | | | |
| S | | OSO–I | | OSO–J | OSO–K | | OSO–L | OSO–M | | OSO–N | OSO–O | | OSO–P | OSO–Q | |
| P | | | | | | | | | | | | | | | |
| 10 km radio 5-arcsec solar | | | | | | | | | | | | | | | |
| X | | OAO–D[a] | | OXO–A | | | OXO–B | | OXO–C | | | OXO–D | | OXO–E | |
| O | | OAO–D | OAO–E | OAO–F | OAO–G | | OAO–H | OAO–I | OAO–J | | | | | | |
| R | | | | | | ORO–A | | | | | ORO–B | | | | |
| S | | ATM–A | | 5″–B | | 5″–C | 5″–D | | 5″–E | | 5″–F | 5″–G | | 5″–H | |
| P | | OAO–D[a] | | | | | | | | | | | | | |
| ASTRA 1-arcsec solar | | | | | | | | | | | | | | | |
| X | | | | | | ASTRA–A | | | ASTRA–B | | ASTRA–C | | ASTRA–D | | |
| O | | | | | | | | | | | | | | | |
| R | | | | | | | | | | | | | | | |
| S | | | | | | 1″–A | | | | 1″–B | | 1″–C | | 1″–D | |
| P | | | | | | | | | | | | | | | |
| 5–10-meter LST 0.1-arcsec solar | | | | | | | | | | | | | | | |
| X | | | | | | | | | | | | 5–10 m → | | | → |
| O | | | | | | | | | | | LST → | | | | → |
| R | | | | | | | | | | | | | | | |
| S | | | | | | | | | | | 0.1″ → | | | | → |
| P | | | | | | | | | | | LST[a] | | | | |

[a] Partial use.

TABLE 4.—*Optimum Program in Particles and Fields Astronomy*

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A minimum program, based on the experiments described in the panel report, is shown in table 2. The Panel emphasizes that a withdrawal of experiments and missions below this recommended level certainly means that experiments of substantial value and investigators now contributing heavily to this field must drop out of this area of space science. An enhanced program should include:

(1) The addition of planetary orbiters and/or planetary capsule landers for the outer planets carrying fields and particles experiments;

(2) An additional interplanetary explorer each year after 1972;

(3) One or two additional deep space probes out of the ecliptic plane;

(4) Additional "new" solar probes, possibly shifting the initiation of the program to an earlier date;

(5) Additional planetary explorers for the inner planets;

(6) An increase in the number and variety of large automated payloads in the mid- and late-1970's.

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**OSO**—Orbiting Solar Observatory. *See* Solar and X- and Gamma-Ray Panel Reports.

**OXO**—Orbiting X-ray Observatory—an imaging X-ray telescope of about 1-m aperture. *See* X- and Gamma-Ray Panel Report. Also called High Energy B by X- and Gamma-Ray Panel.

**RAE**—Radio Astronomy Explorer. *See* Radio Panel Report.

**SAS**—Small Astronomical Satellite. *See* X- and Gamma-Ray, Optical, IR, and Planetary Panel Reports.

**5″, 1″, 0.1″**—Five-arcsec, 1-arcsec, and 1/10-arcsec spacecraft. A convenient designation of successive generations of solar spacecraft (not to be confused with pointing stability or any other single parameter). *See* Solar Panel Report for definition of the essential parameters.

**5–10 m**—Ultimate X-ray imaging telescope—part of permanent NASO. *See* X- and Gamma-Ray Panel Report.

## BRIEF REVIEW OF THE SCIENTIFIC HIGHLIGHTS AND MISSION OBJECTIVES OF THE AMB SPACE ASTRONOMY PROGRAM

The reader is again referred to the appropriate subdiscipline report for elaboration of particular points in the following highly condensed summary of key observations, instruments, and spacecraft missions.

### X-Ray and $\gamma$-Ray Highlights

*In X-rays* ($E < 15$ keV).— High-sensitivity surveys capable of source detection to a flux limit $\sim 10^{-6}$ Sco X–1; high angular resolution observations to identify sources with (or to observe) objects seen (or to permit searches) in other spectral regions, and to determine structure in extended sources; spectral characteristics (hardness) of sources; polarization of some sources; correlation of variations with optical, radio, and search for transients, supernovae, etc.; and a low angular resolution ($\sim$ 10 square degrees) survey of the diffuse background.

*In nuclear $\gamma$-rays* ($E < 10$ MeV).—Extend energy spectrum of background with enough angular resolution to separate galactic and extragalactic components, and distinguish true background from many weak sources; detect antimatter via 0.5-MeV annihilation radiation?; background peak at 20 MeV?; distinguish production mechanisms; extend energy spectrum of X-ray sources to discover nuclear transition gamma rays from $r$-process elements in supernova remnants—aid in understanding nucleogenesis; time variations of $\gamma$-ray flux-correlated with X-ray, optical, radio, etc.

*In high-energy $\gamma$-rays* ($E > 10$ MeV).—Extend angular resolution of $\gamma$-ray source at galactic center discovered by OSO–III; need sensitivity of $10^{-6}$–$10^{-7}$ cm$^{-2}$ sec$^{-1}$ to study the diffuse background at 1° resolution; spectral resolution to distinguish between the power-law spectrum of electron-produced $\gamma$-rays (bremsstrahlung, inverse Compton), the 70-MeV peak of $\pi^\circ$ decay, and the possible cosmological peak at 20 MeV; measure the stronger radio sources (need $10^{-7}$–$10^{-8}$ cm$^{-2}$ sec$^{-1}$ sensitivity; spectral resolution to distinguish between electron- and nuclear-produced radiation).

## X-Ray and $\gamma$-Ray Missions

*Balloons.*—Continued pointed flights—$\gamma$-ray continuum studies—reach $10^{-3}$ cm$^{-2}$ sec$^{-1}$ level at $10^5$ eV for selected X-ray discrete sources.

*Rockets.*—Broad participation—quick turnaround.

*Explorers.*—SAS–A: sky survey sensitivity $10^{-4}$ Sco X–1; 1–8 keV, 0.5° resolution, broadband spectral resolution; SAS–B: high-energy $\gamma$-ray survey $10^{-6}$ cm$^{-2}$ sec$^{-1}$; SAS–C and beyond: extend energy response 200 eV to 1 MeV, larger spark chamber devices, improve pointing to study sources, time variations.

*OSO.*—Continued use of wheel section for surveys, monitoring.

*OAO or equivalent.*—First stellar X-ray imaging telescope positions to $\sim$1 arcsec, $\Delta\lambda/\lambda$ to $\sim 0.01$ for sources to $10^{-4}$ Sco X–1. Instrumentation state of art.

*OWSE.*—High-sensitivity X-ray and nuclear $\gamma$-ray surveys, nuclear lines and continuum studies; large Cerenkov telescopes, $10^{-8}$ cm$^{-2}$ sec$^{-1}$ above 500 MeV, crude energy resolution.

*Heavy Explorers.*—A: high-sensitivity X-ray survey $10^{-6}$ Sco X–1, 0.1° resolution, nuclear $\gamma$-ray survey chamber $10^{-7}$ cm$^{-2}$ sec$^{-1}$, Cerenkov telescope; B: extend sensitivity, broad energy resolution, increase angular resolution—study continuum $\gamma$-rays from known extragalactic X-ray sources.

*OXO.*—Stellar X-ray imaging telescopes, design goal $\sim$ 1-m aperture, interchangeable instruments at focus to accommodate image detectors, polarimeters, spectrometers.

*5–10-m X-ray telescope.*—A permanent National Space Observatory.

## Optical Highlights

Structure and processes in the outer atmospheres of stars—especially extremes such as very hot, very cold, and very unstable objects—hold clues to the history and fate of stars. Stellar spectrophotometry in the UV can give information on such stellar chromosphere and coronas, adding to our knowledge of similar solar activity which controls solar-interplanetary-terrestrial relationships.

Absorption line measurements in the UV are three orders of magnitude more sensitive than visible lines in detecting the interstellar gas, helping to determine chemical composition, physical state, and energy balance of the interstellar medium.

Continuous spectrophotometric measures of planetary atmospheres, comets, and the interplanetary medium will help us to understand the origin and present nature of our planetary system.

Spectrophotometric UV and IR observations of gas ejection from galactic nuclei, together with high-resolution images of these objects, will help unravel these explosive events whose extreme dynamic conditions play a fascinating role in the evolution of stellar systems and which may lead to new knowledge of fundamental physics.

## Optical Missions

*Airplanes and balloons.*—IR telescopes, from 36 inches in minimum program to a possible 120 inches in maximum; balance flights between large platforms such as Convair 990 and smaller single experiment flights such as Lear Jets. High-resolution visible telescopes (Stratoscope).

*Rockets.*—UV spectrographs, 1-Å resolution, for studies of stellar atmospheres and interstellar absorption lines. Possible standby for a bright comet.

Far IR broad-band scan of sky for emission from interstellar dust grains and sources with peak intensities at wavelengths greater than 20 μ.

*SAS.*—Broadband UV photometer and polarimeter, selective extinction and polarization by grains, variable stars and galaxies, solar-system objects.

*UV sky survey.*—Interstellar gas emission at several wavelengths.

*IR telescope.*—Probably refrigerated—10–100 μ for the study of planets, stars, gas, grains.

*OAO–B* (GSFC).—UV scanning spectrometer 2-Å resolution, stars and nebulae; OAO–C (Princeton): UV scanning spectrometer 0.1- and 0.4-Å widths, interstellar absorption in stars to 6th magnitude, stellar spectra; OAO–D (national facility): UV scanning spectrometer with offset guidance, 0.3–0.5 Å width, spectra to 8th magnitude, 40-Å resolution to 13th magnitude; OAO–E (National): wideband UV spectrophotometer and polarimeter, offset guidance, galaxies, variable stars, interstellar grains; OAO–F (National): UV echelle spectrometer with integrating TV tube, width 0.1 Å to 9th magnitude, 100 Å to 18th magnitude; OAO–G (National): IR interferometric spectrometer plus broadband IR photometer, lightweight cryogenics system, later flights include improved versions of E, F, and G.

*Astra.*—A: UV echelle spectrometer, high-resolution imagery with filters to magnitude 26 in visible; B: more flexible instrumentation; C: include IR capability.

*Large space telescope.*—Aperture 120 inches or more with resolution corresponding to 120 inches, indefinite life. See LST report of the National Academy of Sciences.

## Infrared Highlights

Infrared detector technology and infrared astronomical discovery are together undergoing revolutionary developments in which there are increasing advantages in making observations outside the atmosphere. Infrared space observations will permit—

(1) Observations of extended faint objects.
(2) Observation in the five octaves of spectrum from 25–700 μ.

(3) Broadband observations of extremely faint objects with detectors limited only by celestial radiation.

Fields of study already known to be able to profit from these capabilities are—

(1) The stellar and dust structure of our galaxy.
(2) High-energy processes that occur in some galactic nuclei and quasars,
(3) The role of dust envelopes in the evolution of stars.
(4) The role of dust envelopes in the formation of planetary systems around young stars.
(5) The thermal mission and heat balance of planets and the Moon.
(6) Infrared background radiation. High-temperature phenomena at remote epochs will have their radiation shifted into the infrared. Thus these studies have potential cosmological significance.

## Infrared Missions

Because of the rapid development possible in detector technology, and the high rate of discovery of new classes of astronomical infrared phenomena from the ground, a major effort must be made in these areas to insure that full possible benefit is obtained from the space observations.

Significant developments are expected to occur from the use of small high-altitude aircraft, small balloon-borne equipment, and some rocket flights.

Major installations appropriate for the next few years include a 36-inch telescope to be used in a stratospheric airplane primarily for point source observations, and a Small Astronomy Satellite to be used for studies of extended objects and for surveys for new objects. Eventually, technological advances and astronomical discoveries may slow down, and it will be necessary to use larger platforms such as an OAO or a Large Space Telescope. Should detector improvement be difficult, this phase would come earlier.

## Radio Highlights

Measure the flux densities of 50 to 100 extragalactic and galactic sources at a number of frequencies around 1 MHz.

Map the cosmic background noise level of the full sky at a number of frequencies from 0.5 MHz to 10 MHz.

Measure dynamic radio astronomical phenomena and, in particular, record variations of radio emission from the Sun, Jupiter, and other variable radio sources.

Measure the brightness distribution across a few individual radio sources which are occulted by the Moon.

Obtain data on the statistical parameters of cosmic background noise fluctuation at a few frequencies near 1 MHz.

Study variable interplanetary absorption and interplanetary scintillation effects.

Solar-system observations will concentrate on understanding physical processes in the solar corona and in the magnetospheres of the planets, especially Jupiter and the Earth. The region in the corona from 1 to 50 solar radii is particularly difficult to reach by optical observations or space probes.

## Radio Missions

*Rockets.*—High apogee (1000 km) experiments; e.g., absolute calibration, 1–5, 5–10 MHz, 100–150 lb.

*Explorers.*—RAE–C ionospheric focusing and magnetospheric noise; RAE D&E: two or more element interferometer—supersynthesis test, location experiments; RAE–F: cosmic radio noise background; RAE–G: solar-system radio monitor.

*Orbiting radio observatory.*—Ten-km filled-aperture antenna, circular polar synchronous orbit—2000–4000 lb.

## Solar Highlights

Improved angular resolution XUV spectra and spectroheliograms may lead to understanding the mechanism of nonthermal energy production-plasma and magnetic-field interactions; steep density, temperature gradients; shock and magnetohydrodynamic waves; particle transmission and ejection (perhaps the cosmic accelerator); a flow of energy that controls the state of interplanetary space and planetary ionospheres.

Absolute photometry of XUV resonance lines of atoms and ions will lead to improved abundance determinations and perhaps settle the question of different abundances at different levels in the solar atmosphere.

Absolute photometry of the UV continuum will provide direct observation of the temperature inversion.

White-light coronagraphs may reveal outward-moving disturbances from flares and other active regions.

Visible spectrum observations with very high angular resolution exceeding that possible from below the atmosphere will reveal details of sunspots, flares, prominences, plages, spicules, and the fine network structure.

## Solar Missions

***OSO–I:*** K coronagraph; OSO–J: spectrograph—absolute photometry 300–3000 Å; OSO–L: spectrograph absolute photometry $\lambda < 30$ Å; OSO–M: scanning spectroheliograph $\lambda < 300$ Å; OSO–N: K coronagraph; OSO–O: scanning spectroheliograph 300–1300 Å; OSO–P: spectrograph line profiles 300–1600 Å; OSO–Q: spectrograph line profiles $\lambda < 300$ Å.

*5-Arcsec Spacecraft*—ATM–A:

1. Scanning spectrometer and spectroheliometer (300–1300 Å).
2. Slitless spectroheliograph, photographic (300–650 Å).
3. Spectrograph, photographic, high $\lambda/\Delta\lambda$ (900–4000 Å).
4. Small-field, large-scale X-ray telescope, and slitless spectroheliograph, photographic (2–60 Å).
5. Large-field X-ray telescope with filters, photographic (2–60 Å).
6. White-light coronagraph, photographic.

*5″ Spacecraft No. 2:*

1. High resolution ($\rho \sim 0.2''$) internally pointed telescope, with filters and spectrograph (1100–30 000 Å).
2. Spectrometer for line profiles and spectrum mapping (300–1000 Å). (This is probably a full load.)

*5″ Spacecraft No. 3:*

1, 2. Spectrometers for X-ray spectrum mapping and absolute photometry (2–300 Å). (Probably at least two instruments.)

3, 4. X-ray line profile spectrometers to measure at least the strongest lines (2–300 Å).

5. X-ray imaging instrument (pinhole? Fourier shadowgraph? something else?) ($\lambda < 3$ Å).

### Planetary Highlights

Small pointed satellites will allow spatial scans of the planets in different spectral regions, which will allow us to deduce the vertical structure of hazy atmospheres (e.g., Venus, Jupiter).

Ultraviolet photometry, as well as infrared observations from high-altitude aircraft, will provide critical knowledge of the planetary albedos, necessary to an understanding of the planetary heat budgets.

A cloud model is fundamental to interpretation of spectroscopic measurements, which in turn can yield compositions, temperatures, and pressures.

Measurements of the planetary hydrogen corona in Lyman-$\alpha$ radiation with high angular resolution ($\lesssim 1$ arcsec) can yield the escape temperature, an essential quantity to studies of evolution of the atmosphere and ionospheric structure.

Essential features of a planet's meteorology could be obtained by long-period monitoring of the atmospheric fine structure with large-aperture instruments in Earth orbit.

### Planetary Missions

*Rockets.*—Survey spectrophotometry at several-Å resolution, 1800–3300 Å. Photometry in far-UV resonance lines, especially Lyman-$\alpha$ (1215 Å). Possibly high-resolution scans of narrow spectral regions of special interest.

*Explorers.*—Extension of sounding rocket objectives but to fainter objects and improved spectral resolution, and with the important addition of spatial resolution over disk.

*OAO–A2* (WEP).—Broadband photometry, Lyman-$\alpha$ photometry; OAO–B: spectrophotometry with 10 Å resolution; OAO–C: resolution of 0.1 and 0.4 Å over narrow spectral regions of special interest, possibly with spatial scans.

### Particles and Fields Highlights

Observations within the solar system of magnetic fields and particles with energies from 0.5 keV to many GeV yield information on such diverse astrophysical problems as the 3° blackbody radiation, supernovae, the interstellar medium, the dynamical behavior of the galactic disk, nucleosynthesis and the origin of the elements, and stellar abundances—information not available through any other kind of observations or experiments.

Cosmic-ray particle studies are related to radio, infrared, ultraviolet, X-ray, and $\gamma$-ray astronomy measurements across the spectrum from the microwave background to megavolt photons to form an overall picture which would be inaccessible from optical and radio studies alone. Through couplings between high- and low-energy processes such as the inverse Compton effect, all of the data related to a given object are related to each other and eventually all inputs are needed for a full understanding of the environment.

High-energy astrophysics not only adds new windows to the cosmic electromagnetic spectrum by providing X-ray, $\gamma$-ray, and particle and field astronomy, but also represents the first scientific unification of those disciplines and all other experimental studies of cosmic processes.

## NEW DIRECTIONS FOR THE SPACE ASTRONOMY PROGRAM

Comparisons with the current NASA space astronomy program reveal some of the new directions which will be required to implement the AMB plan. Perhaps the most significant change is an increased effort in X-ray and gamma-ray astronomy. Less than 10 percent of the current NASA effort, X- and $\gamma$-ray astronomy amounts to about a quarter of the AMB program, which assigns approximately equal levels of effort to optical, solar, and high-energy astronomy. The increase needed in the minimum balanced program is a major start in fiscal year 1971 on a new spacecraft with the pointing, telemetry, and general sophistication of an Explorer-class spacecraft, but with a payload size capable of carrying large area X-ray detectors, spark chambers, and Cerenkov telescopes, as well as particle and field experiments in the 1- to 5-ton range. Also included is adaptation of a future OAO spacecraft or an equivalent vehicle to carry a state-of-the-art stellar X-ray imaging instrument comparable to existing solar instrumentation. Later, stellar imaging X-ray telescopes of about 1-m aperture, 10-m focal length will be required.

The optical ultraviolet astronomy program has a mid-1970's goal of a 1- to 1.5-m telescope with diffraction-limited performance, as an essential intermediate scientific and technological step toward the Large Space Telescope of the 1980's. This could be achieved either through a new spacecraft design or by upgrading an evolutionary OAO program.

The infrared astronomy program has a most pressing need for research and development of detectors and small cooling systems which will permit infrared observations with the much greater efficiency that is commonplace at both shorter and longer wavelengths. Such advances could continue the present high rate of discovery of new classes of astrophysical phenomena from the ground and from airplane observatories.

Observations of astrophysical objects in the longwave radio portion of the spectrum with the minimum angular resolution required to distinguish sources may require an antenna made of wires surrounding an area 10 km in diameter. However, a remote possibility of making similar observations by "supersynthesis" interferometric techniques must be studied before this large electronically filled aperture is initiated.

The continuing need for observations of the solar surface with an effective angular resolution of 5 arcsec will require the development of a ground-controlled solar spacecraft with the instrumental sophistication of the ATM–A.

Observations of the planets from Earth orbit will be accomplished with the instruments of the planned OAO's and a Small Astronomical Satellite optimized for planetary observations.

The acquisition of data on cosmic-ray particles and fields in the interplanetary medium requires a careful programing of small fractions of the missions to the planets, and the joint use of the "heavy Explorer" spacecraft for high-energy astronomy.

An important element in the balanced acquisition of essential astrophysical data in the AMB plan is the continuing requirement for the smaller space experiments: the aircraft, balloons, rockets, and small Explorer-class satellites. Though less dramatic and unimposing by their nature, they have a great potential for economic and timely measurements of important data that can complement the other space-based and ground-based multiwavelength observations.

An essential part of the AMB exercise to project the level of space astronomical research as far as possible into the future was an assessment of the availability and enthusiastic interest of excellent people—scientists and supporting specialists, including several engineering and technical groups skilled in the measurement of astronomical radiation. Continuity, breadth, and active competition for flight opportunities must be maintained by a strong NASA program in Supporting Research and Technology (SR&T).

Both SR&T and NASA's Advanced Research and Technology program must press forward to develop essential instrumentation such as lightweight optical mirrors, improved X-ray reflectors and detectors, X-ray photometric standards, electronic imaging systems, improved grating technology, infrared sensors and small cryogenic systems, devices which will be useful in ground based observatories of the future as well as space experiments. Support is also essential for the experimental and theoretical research in related areas of atomic and nuclear physics that will insure progress in analyzing the new observations resulting from these technological advances.

In a properly integrated program of federally supported astronomy, NASA should have a responsibility to support particular ground-based instruments, especially those which are most closely and directly related to NASA's mission. Specific instruments, which are of comparable expense to some spacecraft and might be defended as separate line items in the NASA budget, should include special-purpose monitoring telescopes of intermediate (60- to 100-inch) aperture, large optical telescopes in both hemispheres, and a large steerable paraboloid radio telescope.

# IV

# SUPPORTING RESEARCH AND TECHNOLOGY (SR&T)

## THE COMPONENTS OF SR&T

The specific types of efforts required by a healthy space-astronomy program, in addition to satellite construction and flight and the support of scientific research directly related to the flights, are varied. They include:

*Technological developments* leading to improved instrumentation such as the search for UV transparent materials, the improvement of gratings, lightweight optical mirrors, improved X-ray reflectors and detectors, electronic imaging systems, infrared sensors and small cryogenic systems, the improvement of X-ray photometric standards, and many others.

*Scientific studies* indirectly related to flights, including direct studies of satellite measurements by groups other than the principal investigator, comparisons with other space and ground-based data, laboratory spectroscopic and cross-section studies, complementary ground-based research including intensive optical and radio studies of peculiar objects seen by a satellite experiment, synoptic studies of cosmic rays, solar magnetic fields, and many others.

*Space experiments not requiring satellites,* including rocket experiments, stratospheric aircraft measurements in the far IR, balloon experiments, and the directly related scientific research.

These three somewhat different areas are lumped together within NASA under the name "Supporting Research and Technology." Although perhaps of practical value, this lumping together of activities which precede, parallel, and follow space flights, and which include many individual projects varying greatly in their degree of connection with identifiable flights, makes the SR&T budget item, however essential, hard to describe and perhaps difficult to defend.

The Astronomy Missions Board has studied the various components of the SR&T program in different ways, and the various resulting discussions will be found in several different places in this position paper.

The most detailed discussions will generally be found in the reports of the subdisciplines (part II).

The Board requested each of the panels to identify the most important items in SR&T, especially any long-leadtime items required to implement the next steps in the subdiscipline space astronomy program. Thus most panel reports contain a discussion of their future needs for new scientific instrumentation (technological development) and supporting theoretical and laboratory research (scientific studies), usually in a separate section called SR&T. Note that in most cases, the subdiscipline panels regarded rocket, aircraft, and balloon experiments as part of their recommended flight programs.

However, in some cases, the SR&T section may not discuss the need for complementary ground-based observations. The Board also separately asked the panels to take a look at the ground-based needs of each subdiscipline, and some panels prepared a separate section on this subject. The Board also created a working group on ground-based astronomy, which was charged not only with identifying the need for ground-based observations in direct support of space experiments, but with the problem of how the space-astronomy program and ground-based observatories fit into an integrated national effort in astronomy. Part V of the position paper contains this report on ground-based astronomy.

Finally, and very recently, the Board formed a working group on Supporting Research and Technology, and asked them to look at NASA's Supporting Research and Technology and Advanced Research and Technology programs. They were requested to estimate whether the needs of the astronomy subdisciplines were being met, and perhaps to draw from the lists of items in the subdiscipline reports a list of highest priority needs in technology development and scientific studies for the astronomy program as a whole. They were also asked to estimate the optimum level of effort in SR&T for a healthy space-astronomy program. At the present time, although a tentative list has been prepared by the working group, no master list of SR&T needs has been endorsed by the Astronomy Missions Board. General considerations on a healthy space program and the level of SR&T, as well as some comments on the training of scientific manpower and future NASA/university relationships form the balance of this chapter.

## A HEALTHY FLIGHT PROGRAM

Before a satellite mission can be defined, before its budget can be prepared and defended, two important questions must be answered: What are the scientific questions that block our progress, and what is technically possible in the way of satellite hardware which will permit us to answer these questions? Thus two interacting studies—what is needed and what is possible—must be maintained at a reasonable level in order to design and fly the right equipment at the right time.

Much of the attention and much of the cost in space science concentrates on individual satellite missions, yet a perfect flight, with correct functioning of every component, would be useless without related enterprises on the ground before and after the flight. The analysis and study of the information received from the satellite clearly must take place in order that the engineering effort that created the satellite contribute to space science. Less obvious, but equally necessary, is the effort, both scientific and technological, which must occur before the flight, and which may in fact not be easily identifiable with a single space mission.

The definition of what constitutes a "reasonable level" for these studies conducted before and after space missions is not easy. Results of research are not available ahead of time; surprises are frequently more valuable than straightforward answers; so those efforts which will finally lead to good satellite experiments cannot be planned with precision. The proper level of effort can be fixed only by indirect means based on experience with successful work in the past. This experience indicates that the scientific and technological work needed to support a healthy and well-planned space flight program has several characteristics:

*Continuity.*—Groups pursuing either space astronomy or the technology of space hardware need to be able to build on prior experience and to apply the successes and mistakes of one mission to the planning of later efforts. This need requires that enough support be available annually for most of the experienced groups to continue their work in the space sciences, and to retain key technical staff necessary to pursue further experiments.

*Breadth.*—Just because one cannot predict the outcome of research efforts, it is necessary to support a broader range of work than can be defended on the basis of known relevance to planned space missions.

*Competition for flight opportunities.*—Space flights are extremely expensive, and yet the performance of a space experiment cannot be optimized entirely by careful engineering nor by precise planning of the experiment to seek expected results. The necessary ingredients of imagination and trained speculation can be incorporated into the satellite by competition between good groups for flight opportunities—competition in which overall scientific or technical competence and record of prior successes are considered as well as the precise nature of the proposed experiment. This need requires support of enough groups so that competition for flight opportunities is usually lively.

After a space experiment has flown, the results must be analyzed. Such analysis goes well beyond the interpretations of the telemetry record, employment of correction factors, plotting of the measurements, and drawing of conclusions by the principal investigator—work that is usually regarded as part of the flight program. Once again the characteristics of continuity, breadth, and competition are useful in determining if the level of analysis effort is appropriate to optimize the progress gained from the large expense of the satellite experiment. Breadth is of particular importance, since the fruitful interpretation of the satellite measurement may well require laboratory measurements of atomic parameters, ground-based observation of the same object, or the construction of theoretical models which can only be manipulated in the largest computers.

Our picture of a healthy space-astronomy program—one which sees the most progress in space science for each dollar spent—becomes then one in which many studies and explorations, both scientific and technical, result in a number of ideas and proposals for space experiments, and these in turn lead to a smaller number of actual satellite experiments. After the experiments are flown, the work again broadens, beginning with the interpretations of the principal investigators, but passing to a larger group of scientists who seek in the satellite data new results, new relations with other measurements in other fields, and finally new insight into the structure and change of the universe in which we find ourselves.

The effectiveness of the present system of support for working astronomers and instrumentalists at a considerable number of institutions in promoting the development of improved detectors, automated and programed observing schemes, and innovations in instrumental techniques has been amply demonstrated. The grants or contracts have aided in the formation and training of several groups of engineering and technical personnel particularly skilled in the measurement of astronomical radiation. In the future, support should continue to be given to groups attempting to solve the problems involved in both ground-based and orbiting astronomical instruments. Support is also essential for scientific studies, the experimental and theoretical research in related areas of nuclear and atomic physics that will insure progress in analyzing the new observations made possible by technological advances. It is important to preserve flexibility, in order to be able to take quick advantage of imaginative ideas or technological breakthroughs. A grant system which augments the base support and talent available in existing science departments and observatories can retain this adaptability.

## SCIENTIFIC MANPOWER AND THE CONDUCT OF EXPERIMENTS

In developing the long-range astronomy program discussed in this report, the Astronomy Missions Board was constantly aware of the central role played by the scientific community in pursuing these experiments and programs. The originality and success of an experiment in space depends on the quality of the principal investigator, his colleagues, and the environment and facilities available for the conduct of their work. Furthermore, the growth of the astronomy and astrophysics program depends on the training of future scientific manpower. In both these respects the universities are the ideal environment for conducting both original research and training of scientific engineering talent. Indeed, the role of reproducing the next generation of talent falls entirely on the universities.

To provide opportunities for graduate and postgraduate student participation in the astronomy and astrophysics scientific program, these programs must be based mainly at the same institution of the senior investigator. Over the past decade this has been accomplished successfully in many fields of the space sciences. However, with the introduction of large telescopes and similar large experimental facilities in space within the next 10 years, the financial and management requirements for these programs generally will be beyond the resources of a single university. Hence, the Board is concerned with the problem of continuing effective university participation and training of scientific manpower in these fields, and will be studying these problems.

It is already clear that key elements in the future participation of university scientists include a strong supporting research and technology program with continuity and an increased share in the opportunities for space flight.

## THE LEVEL OF SR&T

Although we agree in principle with attempts to describe the appropriate SR&T level, under normal circumstances, as a percentage of the flight program, this percentage cannot be regarded as fixed during large changes in the overall level of effort. The research and technology contribution to NASA's space science program is exactly at the opposite end of the spectrum from such things as overhead, which is added on at the end when all clearly identifiable expenditures have been tabulated. Indeed, it is at the heart—it is in many ways the ultimate scientific pacing item—of an organic program. Therefore, during a period of declining budgets one cannot simply rely on a fixed percentage, but must reexamine the overall health of the program to ascertain if the flight schedule is efficiently supported with ground efforts. Our reexamination leads us to conclude that the level of SR&T funding needs to be increased at the present time.

The Astronomy Missions Board has identified several important reasons why the level of support of research and technology should not only be an increasing percentage of the declining budget, but actually an increased absolute amount of funding:

(1) Research and Technology Development, at a modest cost compared to that of the flight hardware, can result in an improved technique that transforms a delayed mission from simply the same mission flown at a later time into a significantly more productive mission at the same expense—or possibly achieve the same desired scientific results at a fraction of the original expense. Today it would require only one-tenth the area recommended in 1967 by the President's Science Advisory Committee for a high-energy large-area detector mission to achieve the same signal-to-noise ratio—as a result of 1 year's research with rockets which showed that the background was less than one-tenth the number of counts expected and advances in detector sensitivity.

(2) The principal reason why SR&T can make such dramatic changes in the direction of the space science program is probably to be found in the observation that the expenditure goes primarily to support people—and not hardware. It is this large number of creative people, working at the level of conception of ideas and fashioning the innovations in instrumentation needed to realize those ideas, and not the production of three rather than two of the same spacecraft, that is the most vital of NASA's contributions to the overall technological development of our society.

(3) Many of these people derive the support for their creative efforts from direct expenditures on a flight program in which they are involved. Slowdown or elimination of that program means that their contribution must be reclassified as part of the SR&T effort. Some of the vigorous and competent groups, working on problems where solutions are essential to progress in the currently identified next steps in the space program, are being forced from the field because of a lack of funds, the cost of which is minute

compared with the cost of the related flight missions. The modest addition to SR&T required must be made to keep such groups active, for their work will almost certainly have broad and direct impact on future NASA missions.

(4) An overview of the needs of the several subdisciplines of astronomy and astrophysics, as seen in the reports of the panels of the Astronomy Missions Board on the long-range program in space astronomy, reveals that the present low level of SR&T funds is the single worst bottleneck generated by the current financial situation. Furthermore, the amount of additional SR&T required to ease this pressure on all of the subdisciplines appears to be a very small fraction of the amount of the decline in the total astronomy budget.

Accordingly, the Astronomy Missions Board recommends that a significant increase be made in the expenditure for SR&T, and we are confident that this relatively slight redistribution of expenditures will result in a significant increase in the scientific results within the space science program, as well as multiplied technological benefits to our society as a whole.

# V

# GROUND-BASED ASTRONOMY IN AN INTEGRATED NATIONAL PROGRAM

*Report of the Ground-Based Astronomy Working Group*

*September 1968*
*Revised May 1, 1969*
*Revised June 4, 1969*

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# INTRODUCTION

Space astronomy is a part of today's exciting astronomical research, an important part and a difficult part, but only a part. Clues to the secrets of the universe are not constrained by the boundaries imposed by the terrestrial atmosphere nor by the limitations of any one area of technology. Indeed, the discoveries and unexpected developments that have so broadened the scope and outlook of astronomy in recent years have demonstrated again and again the interdependence of observations and deductions that have come by widely different approaches.

The quasars are a classical example: After radio techniques had segregated a class of sources of small angular extent, optical identification of starlike or quasi-stellar objects followed. Spectrographs on the major ground-based telescopes brought the discovery of very large red shifts, and gave the first clue to the enormous energy output. The pattern of emission and absorption lines, the proof of negligible proper motion, the energy curves over the whole spectrum from ultraviolet through infrared and millimeter wavelengths to the radio range, the variability and polarization of both the optical and radio radiation, and their unexplained narrow absorption lines have combined to show how a wide variety of observational techniques can together build up a body of facts which an acceptable physical model of a quasar must satisfy. Observations from above the atmosphere will undoubtedly be important in reaching an understanding of the nature of this previously unsuspected but important component of the universe.

The discovery of X-ray sources had to await the arrival of space techniques. When rocket-borne detectors acquired sufficient directivity, certain sources were found to coincide with radio and optical objects. Spectroscopic studies of two starlike sources, though handicapped by observing pressure on ground-based telescopes of adequate aperture, have produced a distance and indicated a possible resemblance to old novae, and other objects of very high excitation. Radio radiation from the brighter of these two (Sco XR–1) now appears to have been detected; the better signal-to-noise ratio obtainable with larger collecting area and higher directivity would add strength to the observation. Although thus far at an earlier stage of development compared to quasar research, study of X-ray objects can likewise be expected to proceed via the multiple-approach method, using both space and ground-based instruments. Gamma-ray astronomy is at a still earlier stage, with only one or two certainly detected objects, but it may be expected to proceed along similar lines.

Ultraviolet astronomy from rockets has already produced some surprises and anomalies in the stellar spectra thus far obtained. This new information is certain to have an effect on both theoretical models and ground-based spectroscopic investigations.

Infrared observations of cool bodies and possible protostars hold great promise for investigations of the process of star formation. The suspected connection between infrared hot spots and radio-frequency OH emission H II regions is now established, with a new type of anomaly in the OH intensities. In all these studies, observations at shorter optical wavelengths are needed to separate reddening and temperature effects. Thus far all the information that has gone into a rather fragmentary picture has come from ground-based techniques, but work at the longer infrared wavelengths will profit enormously when routine observation from orbiting vehicles becomes possible. Infrared observations in the atmospheric windows have revealed large excesses in objects in which high energy or nonequilibrium phenomena occur. These observations must be followed up by measurements from space. For example, the recent exciting discovery of the pulsars was expanded when the pulsar in the Crab Nebula was identified as an optical stellar source and now pulsed X-rays have been seen from this pulsar by a rocket experiment. An extreme case is the Crab Nebula itself, a supernova remnant that has shown electromagnetic radiation in every wavelength band that has been tried—a range of 12 decades or 40 octaves.

Ground-based observations have brought new and important information about objects as close as the planets—prime objects for space exploration by soft-landing or flyby vehicles. Refinements in high-resolution molecular spectroscopy forced downward revision of the previous estimate of the surface atmospheric pressure on Mars by nearly an order of magnitude, and ruled out any parachute-assisted landings; later a space flyby confirmed the result. Radio determinations of the surface temperature of Venus showed that landing vehicles must be built to survive the unexpectedly high temperature of 600° K. The intense flux in the Van Allen belts of Jupiter, another unexpected result from radio observations, must likewise be taken into account in the design of spacecraft. And ground-based radar astronomy has given the first accurate rotation periods for Mercury and Venus and has developed a method for plotting the surface topography of the nearer planets.

The advent of the first orbiting astronomical observatories seems likely to place more, rather than less, emphasis on the interdependence of observing techniques. The experience of the recent past has covered a period when space observations of objects outside the solar system have been limited to the 5 minutes available to each rocket-borne instrument package. The transition to larger and more sophisticated devices, operating continuously over many months, promises a veritable flood of data that will not only be extremely valuable in its own right but will also raise a host of related questions. Many of these can be answered by existing ground-based instruments, without waiting for the next generation of satellite-borne instruments. And as has always been the experience with new or more refined techniques, unforeseen discoveries are a virtual certainty. These, too, will demand instant attention from ground-based observing equipment.

### An Integrated Program

NASA's space program promises to be a tremendously rewarding, yet a very demanding, part of the astronomical research of the next two decades. Since the program will depend heavily on the existing resources and talent and on the total strength in this whole area of scientific interest, an integrated viewpoint is essential in assessing the overall requirements.

First of all, the existing ground-based observing centers and university astronomy departments are the principal source of manpower and the training ground for young astronomers needed in astronomical projects involving observations from space. Imaginative and creative minds are the most important resource that the general astronomical community can provide to the space program. The observational research in these departments is the means through which students work together with seasoned faculty members in gaining direct experience with the operations and the instrumental techniques of present-day astronomy. To a very large extent this research is with ground-based instruments. In the departments which successfully combine ground-based and space programs, the stimulation and the more immediate observational results provided by ground-based instrumentation are needed to sustain the faculty-level astronomers and the technical staff during the intervals between flight experiments. If successful flight data are obtained, however, it often feeds directly into ground-based research in progress.

To be effective in this vital educational process, faculty researchers and students must have adequate access to the necessary research instruments. This means not only enough telescopes of appropriate aperture, but the means for constructing new auxiliary equipment and for trying out new and original instrumental concepts. Many of these devices are directly transferable to space research. The development of new detectors, of compact high-resolution spectrometers, and of modern data-retrieval systems now being undertaken in university and institutional observatories are a significant contribution to the procedures of flight systems. Testing of instruments and techniques on ground-based telescopes provides valuable experience before a concept is frozen into flight hardware.

The space program depends on ground-based astronomy in a second way: Selecting the crucial observations that will yield the most useful return from the specialized instruments sent above the atmosphere draws on a vast body of data accumulated from studies of many types of objects by optical and radio telescopes. This reservoir of information is continually being renewed and augmented. Protracted and seemingly routine surveys can yield a bit of information that pinpoints an object worthy of examination in a spectral region accessible only from an orbiting vehicle. The enduring importance of both general and specific ground-based observations in designing effective space experiments should not be overlooked in an integrated plan for all of astronomy.

Finally, ground-based telescopes offer valuable direct support and supplementation to orbiting space instruments. One application is the study of time-dependent phenomena, such as eruptive events on the Sun, or the variations of an X-ray source. Simultaneous monitoring from space and the ground can yield correlations between data observed in quite different spectral ranges.

There will almost certainly be a demand for a quick ground-based followup of discoveries or vital clues sent down from space instruments in orbiting vehicles. The previously cited experience of the great progress in quasar research that arose from the interaction of radio and optical methods suggests that a very profitable feedback could develop from such followup observations. Optical, radio, and rocket observations of X-ray sources may indeed be a case where such interaction may now be in progress on a slow time scale.

Rapid ground-based exploitation of new space results is feasible because of the quick adaptability of large optical telescopes to measurements of many types through a simple change of analyzing instruments and radiation detectors. Single-paraboloid radio telescopes are also quite versatile.

A fully operational ground-based telescope is not only adaptable; it can be made available almost overnight for an unexpected high-priority task. The contrast in time scales relative to preparation of a new flight package is enormous. If there are enough such telescopes of adequate aperture, and if the proper coordination and flexibility can be achieved, these instruments offer the means of multiplying the total impact of an orbiting space vehicle on advancement of astronomy by quite an appreciable factor.

The fundamental importance of ground observations of the Sun is enhanced by the recent space solar observations. The combination of data from space and from the ground compounds our leverage on the major solar problems to the greatest advantage.

In addition to stimulating ground-based research, space observations impose a new burden. The value of most of the space observations can be greatly increased by simultaneous ground observations of the same features. The example of simultaneous solar-flare observations must be mentioned. An instance of such cooperation that has already proved its worth is the combination of ground data on the K-corona from the High Altitude Observatory, and coronal emission lines with the EUV spectroheliograms from the Harvard scanning spectrometer on OSO–IV. When such relatively inexpensive ground observations can enhance the value of an enormously more expensive space observation, cooperative observations are practically mandatory, and should be carefully planned in advance.

### Agency Responsibility

As the agency which will be responsible for the space-astronomy programs, NASA must plan and execute the most ambitious, complicated, and costly astronomical endeavor that has yet been attempted. The stakes are very high, and NASA can hardly avoid being concerned about the health, vigor, and technological advancement of the astronomical community from which it must draw strength. Nor can NASA avoid concern about the adequacy of the ground-based telescopes needed for optimum exploitation of space observations.

The references to an integrated program in the report of a Working Group of the Astronomy Missions Board are a reflection of the broad view which the Group feels is essential to the success of the entire effort. An integrated program does not, however, mean that NASA should assume financial or administrative responsibility for all parts of astronomy receiving Federal support. Few astronomers would advocate abandoning the present system of diversified support from several agencies. Rather, NASA's concern may lead to direct financial support to those particular parts of the ground-based equipment which are most closely and directly related to NASA's mission, and to interagency consultation and negotiation regarding other parts that may be more difficult to accommodate under the NASA budget appropriation. What is most important is that NASA remain cognizant of the need, and take steps to avoid letting the priorities be determined by jurisdictional rigidities.

This report does not attempt to review all the needs of ground-based astronomy. In the next section, four specific ground-based developments are singled out as being relevant to NASA objectives in space astronomy.

## RECOMMENDATIONS

### Large Optical Telescopes

The type of ground-based telescope that will be in greatest demand for followup and exploitation of astronomical discoveries from orbiting vehicles is almost certainly the versatile general-purpose reflector of the largest size. Instruments of this class have provided almost all of the crucial data on the astrophysical nature of quasars, simply because adequate analysis of objects so faint in optical wavelengths was possible only with considerable light-gathering power. A similar experience appears to be developing in investigations of X-ray sources. The strange phenomena that will be uncovered for the first time by space observations seem likely to be connected also with objects that are faint optically. The brighter objects have been typed and cataloged in the all-sky surveys undertaken with smaller instruments; readily detectable peculiarities have already been noted.

Yet the telescopes of largest size are the ones that are in the shortest supply. Observing pressure is high, time assignments are made months in advance, and the objects observed are usually those selected as being worthy from studies with smaller telescopes or in other wavelength bands. The completion of telescopes now under construction will not constitute the answer to special needs of space astronomy. The instruments of intermediate size (2-m class) coming into use in the next year or two (some with NASA backing) will broaden the base of ground-based observational astronomy and will provide a valuable backup to certain phases of studies from orbiting telescopes. But they will not be a substitute for telescopes of the very first rank.

The two 150-inch telescopes being built under Association of Universities for Research in Astronomy, Inc. (AURA) auspices (one in Arizona, one in Chile) have been conceived and funded with quite another objective in mind. These instruments are being built to give astronomers from all parts of the country (and from other countries) access to first-rate observing opportunities. The large fraction of the time reserved for guest investigators will be allotted far in advance to observers who must travel some distance to claim their precious telescope hours. The remaining time available to the Observatory staff will be under correspondingly greater pressure and will likewise be obligated well in advance. Time on these telescopes appears to be fully committed and the programing beset by an even larger measure of the same rigidities that prevail on existing large telescopes. At the present time the national observatory telescopes are oversubscribed by about a factor of 2 in terms of telescope-observing requests. This figure reflects a certain saturation of demands and the currently under construction instruments will only approximately meet the present demand without allowing for growth in needs. At the present time, the number of astronomers is increasing by about 15 percent per year.

The newly constructed instruments of the 2–2.5-m class at McDonald Observatory and Mauna Kea have a 25-percent NASA commitment and represent a positive step. However, the guaranteed available time on these instruments falls short of meeting the needs of planetary astronomy (a subject emphasized in planning these telescopes) as outlined by the Space Science Board's Panel on Planetary Astronomy report, where construction of fully planetary 60-inch and 120-inch telescopes was recommended.

Experiences with the two major, modern telescopes (200 inch and 120 inch) operating since the advent of X-ray astronomy, one of several areas of space astronomy requiring ground-based supplementation, indicates that only about 60 nights per year can be dedicated to studying the optical counterparts. The demand would be at least three times this amount. The number of optical identifications will certainly increase and will demand the largest available optical aperture, since the objects will probably be several magnitudes fainter than the present sources. Observations supplementary and complementary to other space-astronomy studies, such as ultraviolet, gamma rays, infrared, will increase the demand to much more than the one area of X-ray astronomy.

Were there no space program whatever, the pace of current research is such that the ground-based optical facilities now in sight would be inadequate to exploit fully the opportunities that will arise. The large additional demands on ground-based telescopes that will come with the advent of a strong space program cannot be met by the hope that the then-current observing projects can be compressed or crowded out. These requirements must be planned for, and are important enough to justify the maximum power and refinement that ground-based optical technology has to offer. New major telescopes should be provided, designed, and funded with a specific advance commitment to the space-astronomy program.

We can place quantitative estimates on the basis of simplified criteria on the additional needs of ground-based optical telescopes imposed by the current space-astronomy program. Experience has shown that it is impossible to predict exactly the use of a versatile telescope, since the programs adjust to discoveries and needs; however, it is certainly possible to evaluate the needs as they are now understood. We shall restrict our discussion to the four funded satellites—OAO–A2, B, C, and X-ray Explorer—and calculate the needs for the most straightforward program of supplemental observations.

The OAO–A2 University of Wisconsin package can observe about 10 stars each day, with a typical brightness of sixth magnitude. With an adopted lifetime of 4 months, about 1200 stars can be observed. If we adopt the criterion that each of these stars merits investigation with moderately high dispersion (10 Å/mm) throughout the optical window with one spectrogram (this being the slowest step of the many supplemental observations required), the total observing time required is 226 clear nights on a 200-inch telescope. For the Smithsonian Astrophysical Observatory experiment, about five fields/day are obtained. Assuming that there are 10 stars of ninth magnitude requiring study by UBV photometry and 100 Å/mm spectra on each plate, we find that 25 clear nights are needed.

On the second OAO (B), where intermediate resolution scanning will be done, we expect that about 1200 stars will be observed. If each of these stars is of seventh magnitude and also requires 10 Å/mm spectra throughout the optical window, then some 270 clear nights will be needed.

The needs of the high-resolution Princeton University experiment (OAO–C) are rather different. Since the major goal is to study the interstellar absorption lines, we have concluded that each star studied by OAO–C should be the subject of a study of all known optical interstellar lines by the highest resolution that is reasonable. Assuming that 10 optical spectral lines will be studied and that a Fabry-Perot system is used on each of the 240 program stars, a total of 180 clear nights will be needed to provide the optical observations required for adequate ionization and curve of growth analysis.

The X-ray Explorer satellite will be able to determine positions of X-ray sources with a much greater precision than now available except for a very few cases. If we define as the major goal the identification of the optical counterparts of the galactic sources, by means of 100 Å/mm spectra, we find that some 560 clear nights would be required to make identifications of all sources down to $10^{-3}$ the X-ray brightness of Sco XR–1! If we were to seek complete identification to $10^{-2}$ Sco XR–1, the total would be 22 clear nights. Only 13 sources would be involved in the latter case, so the desired level (total of 44 sources) for $10^{-3}$ Sco XR–1 would probably be between the two extremes and we adopt the average of 292 clear nights.

These figures are certainly subject to criticism due to the single-mindedness of the supplementary programs and the lack of allowance for improvement in techniques beyond present capabilities; however, versatility comes with knowing the early results of rather single-minded studies and every experience is that the needs of observers expand to fill the time opened by improved observing speeds.

The grand total of 993 clear nights must be spread over two observatories, since all sky coverage is vital. Taking the optimistic average that usable observations will be obtained on 50 percent of all night hours, we find that 1986 scheduled nights on a 200-inch telescope would be needed. Since two instruments would be needed to provide all-sky coverage, this means that support of these four satellites could require 2.56 years' scheduled time on two observatories comparable in size to the Palomar 200 inch. In view of the current plans to continue and expand the space-astronomy program, to at least one astronomical satellite each year, there will be a continued demand for these supplementary observations. Each observing station would cost about $25 million at the current price level and could be completed in a 5-year timespan if a strong and experienced astronomical group is given the construction responsibility. The leadtime is long enough, however, so that early initiation is required if completion is not to lag behind flight plans. In fact, the estimate given above of 2.56 years would represent the lag even if construction was started immediately.

A built-in orientation toward the space program would be assured by the alternative of direct construction of these observing stations by NASA. But the assembly of a competent group of astronomers and instrument engineers would be difficult and time consuming. Both the cost and the leadtime would go up. A plan that gave the responsibility for each station to a major university or to a small consortium of universities would put to immediate use the considerable body of experience in telescope construction and operation that exists in a number of universities. Some of the candidate universities have scientists already involved in the space program who could contribute both before and after the beginning of observations. In such a group, observationalists who keep up their observing programs on existing telescopes will offer a continuous flow of ideas and comment. In the opinion of the Working Group, the latter plan is the preferred one. Contractual arrangements can provide a sufficiently flexible space-oriented commitment of the ground-based facility, and attachment to a strong university (universities) can probably provide operating funds for at least a part of the local ground-based program.

The Working Group places a high priority on additional optical telescopes of the largest size.

### Ground-Based Radio Astronomy

Radio and radar astronomy is the basis for some of the most unusual discoveries in recent times—quasars, pulsars, the cosmic maser sources, the unexpectedly high temperature and thick atmosphere of Venus, the unusual rotation modes of both Mercury and Venus, the radiation belts of Jupiter, and the low-frequency radio bursts of Jupiter controlled by the satellite Io. From a practical point of view, a number of the radio discoveries have had a direct influence on the course of planning space science and space exploration, just as it has changed the direction of much of optical astronomy.

In our own solar system, effective planning of planetary probes would not have been possible without prior knowledge from radio observations. The high temperature of Venus was discovered by ground-based radio measurements, and theoretical studies suggested the need for a very thick atmosphere. The Soviet probe, Venus 5, entered the atmosphere and ceased transmission before it completed its journey to the surface, thereby giving a false atmospheric depth that was corrected through radar measurements.

The intense radiation belts of Jupiter were discovered entirely through radio measurements that demonstrated the existence of a magnetic field of several gauss or more. The energetic particles trapped in this field emit strong, polarized radio noise that implies radiation levels far higher than one would have expected. It is essential that any probe sent to Jupiter be designed to operate at these very high levels.

Radar astronomy has shown that textbook ideas of the rotation of Venus and Mercury are wrong. Mercury does not keep the same face to the Sun, but rotates 1½ times during each revolution, while Venus rotates in retrograde fashion, with a period that is apparently locked to the Earth. There are, also, the programs of lunar and planetary mapping that can be expanded to include targets such as the outer planets, the moons of Mars and Jupiter, and the larger asteroids if plans for larger antennas and more powerful transmitters can be augmented.

From spectroscopic observations at radiofrequencies, a class of sources has been identified which must be emitting by a maser mechanism. Intense bursts of polarized radio waves are apparently radiated by interstellar clouds composed of certain molecules (notably OH and $H_2O$) which can store large amounts of energy in excited molecular levels. The mechanism is not yet completely understood, but it has enabled us to detect tiny traces of these compounds which would not otherwise be seen.

Recently, more complex molecules such as ammonia and formaldehyde have been discovered through radio observation of the interstellar medium, and there is an expectation that the field of radio spectroscopy will both complement and augment the studies of the interstellar medium that will be pursued with space telescopes in the ultraviolet.

These examples serve to demonstrate that radio and radar astronomy are uncovering hitherto unsuspected sources of energy and new classes of phenomena which have important implications for cosmology, solar system physics, cosmic-ray physics, stellar evolution, and the physics of the interstellar medium. It is, therefore, a vital part of current astronomical research, both in the study of the distant parts of the galaxy and the universe, and in the study of the solar system.

All of these discoveries have been made using large steerable paraboloids. At the present time, the United States is falling behind other nations in the construction of major radio telescopes, and several studies have documented the need for new large radio telescopes. Proposals have been put forward for the construction of a 440-foot fully steerable paraboloid in a radome, for building large antenna arrays using many individual dishes to achieve maximum angular resolution, and for upgrading present systems based on a large spherical dish fixed in the ground. All of these proposals have merit. Our purpose here is to point out aspects of these proposals which are the most relevant to the NASA program, and to recommend ways in which NASA might participate in the support of such facilities.

The optical space-astronomy program will have as one principal target the interstellar clouds seen by the radio astronomers. In the ultraviolet, these clouds will exhibit large numbers of resonance absorption lines which will permit quantitative analysis of their composition and physical properties. The high dispersion of radio techniques, and the ability to detect emission lines from regions that are too faint to be studied optically, will provide complementary information that will be needed to gain understanding of physical processes in the interstellar medium. Some of the ultimate aims, such as understanding how stars are formed from the interstellar gas, and detecting and studying planetary systems in the process of formation, will surely require all the tools that we can bring to bear upon the problem. The radio measurements will require new and larger radio telescopes, particularly a steerable dish with potential for rapid changes of receivers and feeds, covering a wide range of frequency in order to exploit the data from optical space telescopes.

Planets will be a prime target for the optical and infrared programs, as well as for the NASA programs of planetary exploration. As in the past, radio telescopes will continue to gather data on planetary surfaces and atmospheres which will be invaluable in planning future space probes and in interpreting data both from Earth-orbital telescopes and flyby missions. For example, data taken by flybys on limited areas of the planet can be extended by ground-based radar techniques to cover the whole planet. New antennas with large collecting area will be needed for this work. The radar programs will need large steerable dishes for these purposes, although the work will be complemented by the use of arrays of dishes as the receiving terminals, operated in a bistatic mode.

The study of distant radio sources such as quasars will be a goal of the optical, X-ray, and radio space-astronomy programs. The study of such objects from the ground is becoming severely limited by inadequate collecting area and angular resolution. Arrays of antennas are needed for the job. The recent radio detection of X-ray sources provides a clear example of the present equipment limitations. The radio emission is detectable, but so weak ($5 \times 10^{-28}$ $\mathrm{wm}^{-2}$ $\mathrm{Hz}^{-1}$) that only the largest existing instruments can perform the observations. The large amount of integration time required for detection makes the study of time variations difficult indeed, especially since the time required for these observations must be shared with other programs. These observations have been performed with steerable paraboloids, but would have been better performed with large arrays, if the arrays had been available.

A special area of study that is well suited to array work is high-resolution decimetric observations of the Sun. In the wavelength range from 3 to 21 cm, phenomena in the chromosphere and corona can be observed with high angular resolution by ground-based arrays, and it is just this region of the solar atmosphere that is most intensively studied by the solar-space program. Simultaneous observations from the ground in this wavelength region are essential for full interpretation of the complex phenomena occurring in the solar atmosphere. A few such arrays have been built, but the dimensions of these arrays are still relatively small. Much larger arrays could be built, and from 3 to 10 cm an angular resolution could be realized that would be comparable to the angular resolution (5″–10″) that is planned for spacecraft observations.

We therefore recommend that NASA support the construction of new radio facilities in two ways:

(1) By direct financial support of those aspects of new facilities that are deemed to be critical for the success of the NASA program of planetary exploration. We believe the latter would include those facilities which would have as major objectives the study of planetary surfaces and atmospheres by radio and radar techniques. First, larger steerable dishes will be needed, particularly for the radar programs and for spectroscopic studies, and second, arrays of paraboloids can provide high angular resolution for the study of planetary atmospheres and can serve as receiving antennas in bistatic radar experiments.

(2) By cooperating closely with the National Science Foundation, which has the prime responsibility for the support of ground-based radio astronomy, to obtain new facilities that will permit intensive study of objects of direct interest to the space astronomy program.

### Specialized Monitoring Telescopes

Nearly all of the new types of astronomical sources discovered in recent years—quasi-stellar objects, X-ray sources, IR objects, peculiar radio galaxies, for example—are nonequilibrium sources and hence non-steady-state phenomena. Important information as to their physical nature may be learned from variations in the optical and radio flux and from variations in the polarization. Yet experience has shown that it is nearly impossible for an observatory to set aside a multiuser, general-purpose telescope solely for monitoring of possibly variable sources by one or two observers. And uninterrupted monitoring appears to be necessary; random scheduling leaves too many gaps.

NASA has faced this situation before as regards availability of telescopes for astrophysical observations of planets at critical times and for continuous surveillance of planetary surfaces; NASA finally resolved the difficulty by providing support for new telescopes with an advance commitment to NASA-related objectives. The study of time-dependent phenomena presents another example where a specific commitment of special-purpose telescopes is required. Such instruments would play a vital role in simultaneous observations of nonequilibrium phenomena from space and ground-based stations. Since for most special purposes photon fluxes are low, the telescopes recommended here should generally fall in the 2-m class.

## SUMMARY OF GROUND-BASED WORKING GROUP RECOMMENDATIONS

### Large Optical Telescopes

The Working Group recommends the construction of two observing stations, one in the Northern and one in the Southern Hemisphere, each to have as its central instrument an optical reflector of the 200-inch class. The supporting instrumentation and auxiliary equipment should be complete and modern. Each observing station would cost about $25 million at the current price level and could be completed in a 5-year timespan if a strong and experienced astronomical group is given the construction responsibility. The leadtime is long enough, however, so that early initiation is required if completion is not to lag behind flight plans.

In the interest of minimizing construction costs and time, and promoting the widest use within the framework of space astronomy, the Working Group recommends that the construction and operation of these observatories be entrusted to a major university or consortium of universities having the technical experience and competence for such a project. Contractual arrangements can provide a sufficiently flexible space-oriented commitment of the ground-based facility, and attachment to a strong university (or universities) can probably provide operating funds for at least a part of the local ground-based program.

### Large Steerable Paraboloid for Radio Astronomy

The Working Group recommends that NASA back the construction of a large fully steerable paraboloid (400 ft or more) as the most versatile instrument for complementing and extending observations and discoveries from planetary probes and orbiting vehicles with both radio and optical payloads.

In addition to the astrophysical uses of such a paraboloid, it could serve NASA's planetary program in two ways:

(1) The greater receiving area would improve telemetry from unmanned deep space probes going to other planets; reduction of transmitting power on the probe would permit an increase in experiment payload.

(2) Radar astronomy, particularly the mapping of planetary topography, would profit from the increased transmitting and receiving area; such information as may be gained from the Earth, without going to the planet in question, is a great saving in expense.

### Specialized Monitoring Telescopes

Telescopes which are committed to the support of space science are especially required owing to the variability of many of the most interesting astronomical objects. The necessary long blocks of observing time and all-sky coverage require the construction of a series of northern/southern, eastern/western telescopes. Such instruments could be constructed and controlled by either interested present astronomical groups, staff members at one of the NASA centers, or both. The diameter is about 2 m.

# VI

# THE ROLE OF MAN IN SPACE ASTRONOMY

## GENERAL CONSIDERATIONS ON THE AVAILABILITY OF MAN

Throughout the period during which this report was prepared, the Board has paid much attention to the important question: What is the role of man in space for "space astronomy"? (The term "space astronomy" is here used in the NASA definition which excludes in situ investigations of the Moon—as a research subject, not as a possible observing platform—as well as in situ investigations of the planets.) The Board has received a number of specific briefings on this question from NASA representatives, and the reports of the panels for the various subdisciplines of space astronomy (pt. II) contain several references to this subject.

Nevertheless, the Board does not consider itself in a position to make a single set of recommendations regarding this vital subject at the present time. On the one hand, it appears clear that an entirely unmanned astronomy program is possible, a program which could be exciting in the exploration sense and fundamentally important in the direct scientific sense. On the other hand, it seems entirely possible that the range of astronomical research in space could be widened to a decisive degree if manned operations in space were applied to space astronomy; and this becomes increasingly probable the larger the astronomical instruments are that are being considered. However, the answer to the question regarding the effectiveness of the involvement of men in space for astronomical research depends—and will continue to depend—on a wide range of factors of which only a small fraction are directly determined by purely astronomical considerations. Some of these nonastronomical key factors are obviously: Will there exist a substantial manned space activity in Earth orbit—or even on the Moon—for purposes other than astronomical research? If there is such a manned activity, would the operation of astronomical research instruments be a particularly effective tool to assist in the assessment of man's physical and intellectual ability and stamina in space? Can the cost of man-rated transportation to the chosen site of operations be substantially reduced from its present level in the foreseeable future?

In particular, if the cost of manning astronomical experiments in space is not regarded as part of the cost of astronomical research per se, the presence of man could well be an asset. If the cost of manning experiments were to be borne by research budgets for astronomy, the economics of this procedure would be far harder to evaluate. Obviously, these and other similarly relevant questions are related either to engineering developments or to major facets of the overall national space program outside the space-astronomy portion of this overall program. Furthermore, reasonably substantive assessments for these questions appear impracticable at this time. Accordingly, the Board considers itself without sufficient basis at present to take a position on the basic question regarding the role of man in space astronomy, even though the Board realizes that this question may turn out to be of decisive importance to space astronomy.

In consequence of the circumstances just described, this chapter does not address itself to the central question: Can, need, or should man in space be involved in space astronomy? Instead, the following section will address itself only to the question as to the type of role which man in space could play in space astronomy, if the decision should be made that man in space will be involved. Accordingly, the following section should be understood as being written under this specific premise, the validity of which the Board does not try to judge at this time.

## SPECIFIC CONSIDERATIONS ON THE USE OF MAN IF AVAILABLE

It has always been tempting to believe that major discoveries should nearly automatically be gained in astronomy if an imaginative scientist with an adequately large and flexible instrument be positioned in space—free from the encumbrances of the terrestrial atmosphere—so that he could react directly to the new phenomena he is discovering, and could, in the true sense of scientific research, adjust his next experiments or observations immediately to the new insight just gained. This ideal picture does not, however, take proper account of the human limitations of even the best of scientists. Experience in ground-based research has taught us that an astronomer can exploit a major telescope in a truly creative sense only for a quite limited number of days or nights. If he is tied for a substantially longer period to his instrument, his operations will likely turn into routine data taking—certainly not an activity consonant with the value of a major space telescope.

Accordingly, in order to exploit fully a large astronomical telescope in space, a substantial number of astronomers, together with their associates, will have to participate actively in its operation—just as is the case for major ground-based installations. In view of this rather large quantity of scientific manpower required for a major space instrument, we conclude that in general the scientific operation of a large space telescope will most effectively be carried out from the ground. Furthermore, it appears highly likely that these operations will be most fruitfully executed through direct automatic links from the scientists to the instrument rather than indirectly through men in space, since the latter could easily introduce serious psychological barriers between the scientists and the instrument.

Such a general conclusion as discussed in the preceding paragraph is practically certain to require revision under exceptional circumstances. One such exception might well be a solar astronomer investigating the most rapid phenomena of the active Sun, since his scientific judgment could come into play for the best choice of experiments in the case of a rare, fast solar event if he is in space directly with his instrument, but the same would not be the case if he were on the ground as long as continuous wide-band communication links from ground to space telescopes are not practicable.

In contrast with the general conclusions regarding "scientific operations" discussed in the preceding paragraph, quite different conclusions may be derived with regard to "instrument operations," under the specific premise defined at the end of the first section of this part. It appears possible if not even likely that an instrumentalist in space—be he an engineer or a scientist mainly interested and capable in instrumentation—could contribute substantially if not even decisively to space astronomy if he had physical access in space to a major astronomical facility there. The type of instrument operations that have been envisaged include:

(1) Increasing the effective lifetime of an instrument by replacing a subsystem that has failed or by similar maintenance operations.

(2) Keeping a major facility up to date through the replacement of obsolescent subsystems and the addition of entirely new subsystems which embody the latest state of the art in the relevant field of technology.

(3) Taking part in the initial activation of a new major installation either through actual participation in the assembly of the installation or by operations, as needed, to insure failure circumvention during the initiating period.

(4) Film retrieval and resupply for those space experiments which require the very large data-storage capacity of film, as long as no automated methods or instrumentation

competitive with film for these specific research needs have been developed.

One final item should be added with regard to the role of man in space astronomy. If, under the premise defining this entire section, an astronaut is to carry out instrument operations (such as those listed above) for a major astronomical instrument, then it may turn out effective, both scientifically and psychologically, if this major instrument—though basically designed for operation from the ground—could also be operated by an instrumentalist in space for at least one specific time-limited research project for which the presence of the instrumentalist would be of special value.

# VII

# SUMMARY

The Astronomy Missions Board (AMB) was established by the National Aeronautics and Space Administration in the fall of 1967 and charged with the creation of an exciting, significant, and forward-looking long-range program in space astronomy. The Board was asked to formulate the major unsolved problems of astronomy, to define the measurements from space that would assist in their solution, and to specify the types of instruments, spacecraft, and missions needed to perform the required measurements.

## ASTRONOMY AND SPACE RESEARCH

Astronomy has a far greater potential for advancement by the space program than any other branch of science. Telescopes working on the surface of the Earth can only observe those portions of the electromagnetic spectrum that penetrate through the Earth's atmosphere, chiefly those of visible light, and radio waves in the band from a few millimeters to about 20 m in wavelength. Astronomical instruments located in space can now reach the remaining regions of the electromagnetic spectrum. Thus, by coordinated programs of observation, in which the same object is observed over the entire range of the electromagnetic spectrum by telescopes in space and on the ground, the most fundamental problems of astronomy may be brought within range of solution.

The new multiwavelength approach to astronomy requires the combined efforts of scientists working in many fields of the natural sciences, since radically different experimental and theoretical techniques are needed to observe and interpret radiation from different parts of the spectrum. In order of decreasing energy, the principal subdivisions of the spectrum are: gamma rays, X-rays, ultraviolet radiation, visible light, infrared, and radio waves. The measurement of particles and magnetic fields in space has also come to be recognized as a major tool for the exploration of the universe. The acquisition of the data alone involves the application of talent from many different branches of experimental physics and engineering. Moreover, the data are of keen interest not only to astronomers but to research workers in many branches of theoretical physics, chemistry, mathematics, geology, and geophysics, and perhaps also biology. Thus, the multiwavelength approach is also a multidisciplinary approach and space astronomy is an activity that promotes the unification of science.

Because of the specialized nature of the instrumentation employed in different spectral regions and the special requirements of solar and planetary observations, the Board carries on its work with the aid of seven specialized panels, each concerned with a different subdiscipline of astronomy: solar, planetary, particles and fields, X-ray and gamma-ray, ultraviolet, infrared, and radio. In addition, several working groups are engaged in studying the needs of supporting research and technology, complementary ground-based research, education and training of scientific manpower, and the role of man in space astronomy.

## THE MAJOR UNSOLVED PROBLEMS IN ASTRONOMY

Each of the seven panels began its work by formulating the major questions it was seeking to answer by the application of its special techniques and by showing how space astronomy could make unique contributions to their solution in the next 10 years. Full discussion of these scientific questions will be found in the reports of the panels and only two examples will be given here.

The Crab Nebula is a fine example of the usefulness of space observations. This enormous cloud of glowing gas, left over from the explosion of a star in A.D. 1054, radiates in all regions of the spectrum from long radio waves to X-rays. Close to the center of the nebula is a pulsar which may be a neutron star, in which matter is compressed to a density of about 10 billion tons per cubic inch, probably resulting from the collapse of the central core of the exploding star. The pulses have now been observed in radio waves, visible light, and X-rays. Taken together, the combined observations show that the total rate of energy radiated by the pulsar is over 100 times greater than that radiated by the sun, despite the fact that the pulsar is only 6 miles or so in diameter.

A second set of measurements suggests that we may be able to observe the cosmic fireball that occurred at the beginning of the expansion of the universe. Radio-astronomy measurements made on the ground at many wavelengths between 3 mm and 79 cm have shown that space is filled with blackbody radiation with a temperature of about 3° K. Such a background of microwave radiation was predicted by George Gamow to arise naturally from an early hot phase in an evolving universe, and if the radiation is indeed found to have a cosmological origin it would provide strong evidence in favor of an evolving model of the universe and against steady-state models in which matter is being continuously created. The peak intensity of the microwave background occurs at a wavelength of about 1 mm. Since the Earth's atmosphere is opaque at this and shorter wavelengths, it has been impossible with ground-based equipment to verify whether the intensity at shorter wavelengths does indeed decrease as predicted. As a fundamental cosmological phenomenon, the microwave background has a high priority for study from space.

The foregoing are only two examples of the many astronomical mysteries that can be cleared up by the methods of space astronomy. The most pressing of these problems form the basis for the design of a long-range program. A much longer list of problems is given in the subdiscipline reports of part II. They are representative of the many well-defined scientific problems which can now be solved by the multiwavelength approach.

A second major justification for space astronomy consists of the many unexpected discoveries that are sure to be made, as they always are, when a new region of the spectrum is first explored or when a new instrument of unprecedented power is put into operation. The recent history of astronomy is full of examples of such unexpected discoveries. For example, the first radio and X-ray sources were both discovered accidentally, and many of the recent discoveries of strong emitters of infrared radiation could not have been predicted in advance.

## PREPARATION OF THE LONG-RANGE PLAN

Once the scientific problems had been formulated, each panel considered how its special techniques could be applied to acquire knowledge in an orderly, systematic fashion by a series of space missions involving equipment of increasing size and sophistication. Each panel was in fact asked to draw up so-called minimum and maximum programs, the former being defined to proceed at the minimum rate necessary to attract and retain the interest of the leading workers in the field. Conversely, the maximum program was designed to proceed at the fastest possible rate consistent with available scientific and technical manpower. The full Board accepted the judgment of each panel as to the order in which they should be flown. But the rate at which each of the panels' programs was recommended for implementation was decided by the Board, after examining carefully the competing claims of the separate panels.

In effect, the Board decided the percentages of the budget to be allocated to each of the subdisciplines in a given year. In fact, two such programs are presented in this report. The first is a so-called minimum balanced program, which recommends an annual expenditure of $250 million for an average year in the mid-1970's (fiscal years 1974 to 1976 time period). The Board believes that this is the minimum figure at which viable long-range programs in all of the subdisciplines can be supported. The second, or optimum program, calls for an average annual expenditure of $500 million during the same period and is envisaged as the optimum program that can be supported with available manpower. Both the optimum and minimum balanced program cost figures do not include provision for the cost of the largest instruments, among them a 120-inch diffraction-limited telescope for optical stellar astronomy, which are planned for a National Astronomical Space Observatory (NASO) envisaged for the early 1980's.

## SOME NEW DIRECTIONS

Comparisons with the current NASA space-astronomy program reveal some of the new directions which will be required to implement the AMB plan. Perhaps the most significant change is an increased effort in X-ray and gamma-ray astronomy. Less than 10 percent of the current NASA effort, X- and $\gamma$-ray astronomy amounts to about a quarter of the AMB program, which assigns approximately equal levels of effort to optical, solar, and high-energy astronomy. The increase needed in the minimum balanced program is a major start in fiscal year 1971 on a new spacecraft with the pointing, telemetry, and general sophistication of an Explorer-class spacecraft but with a payload size capable of carrying large area X-ray detectors, spark chambers, and Cerenkov telescopes, as well as particles and fields experiments in the 1- to 5-ton range. Also included is the adaptation of a future OAO spacecraft or an equivalent vehicle to carry a state-of-the-art stellar X-ray imaging instrument comparable to existing solar instrumentation. Later, stellar imaging X-ray telescopes of about 1-m aperture, 10-meter focal length will be required.

The optical ultraviolet astronomy program has as a mid-1970's goal observations requiring the equivalent of a 1- to 1.5-m telescope with diffraction-limited performance, as an essential intermediate scientific and technological step toward the 3-meter large space telescope of the 1980's. This could be achieved either through a new spacecraft design or by upgrading an evolutionary OAO program. Also possible would be an early developmental model of the 3-meter telescope, structurally similar but with degraded pointing, mirror quality, etc. providing performance equivalent to a 1.0 to 1.5 meter diffraction-limited telescope.

The infrared astronomy program has a most pressing need for research and development of detectors and small cooling systems which will permit infrared observations with much greater efficiency, as is commonplace at both shorter and longer wavelengths. Such advances could continue the present high rate of discovery of new classes of astrophysical phenomena from the ground and from airplane observatories.

Observations of astrophysical objects in the longwave radio portion of the spectrum with the minimum angular resolution required to distinguish individual sources may require an antenna made of wires surrounding an enormous area 6 miles in diameter. However, a remote possibility of making similar observations by "supersynthesis" interferometric techniques must be studied before this large electronically filled aperture is initiated.

The continuing need for observation of the solar surface with an effective angular resolution of 5 arcsec will require the development of a ground-controlled solar spacecraft with the instrumental sophistication of the ATM–A. This spacecraft may evolve through a series of upgraded missions to achieve effective 1 arcsec performance by the late 1970's, or an entirely new 1 arcsec spacecraft will be needed. This, too, is an essential scientific and technological step needed to acquire solar observations with spatial, spectral, and time resolution intermediate between the ATM–A and the 0.1 arcsec solar telescopes of the National Astronomical Space Observatories of the 1980's.

Observations of the planets from Earth orbit will be accomplished with the instruments of the planned OAO's and a Small Astronomy Satellite optimized for planetary observations.

The acquisition of data on cosmic-ray particles and fields in the interplanetary medium requires a careful programing of small fractions of the missions to the planets, and the joint use of the "heavy Explorer" spacecraft for high-energy astronomy.

An important element in the balanced acquisition of essential astrophysical data in the AMB plan is the continuing requirement for the smaller space experiments—the aircraft, balloons, rockets, and small Explorer-class satellites. Though less dramatic and unimposing by their nature, they have a great potential for economic and timely measurements of important data that can complement the other space-based and ground-based wavelength observations.

An essential part of the AMB endeavor to project the level of space astronomical research as far as possible into the future was an assessment of the availability and enthusiastic interest of excellent people—scientists and supporting specialists, including several engineering and technical groups skilled in the measurement of astronomical radiation. Continuity, breadth, and active competition for flight opportunities among these groups must be maintained by a strong NASA program in Supporting Research and Technology (SR&T).

Both SR&T and NASA's Advanced Research and Technology (AR&T) program must press forward to develop essential instrumentation such as lightweight optical mirrors, improved X-ray reflectors and detectors, X-ray photometric standards, electronic imaging systems, improved grating technology, infrared sensors, and small cryogenic systems, devices which will be useful in ground-based observatories of the future as well as space experiments. Support is also essential for the experimental and theoretical research in related areas of atomic and nuclear physics that will insure progress in analyzing the new observations resulting from these technological advances.

In a properly integrated program of federally supported astronomy, NASA should have a responsibility to support particular ground-based instruments, especially those which are most closely and directly related to NASA's mission. Specific instruments, which are of comparable expense to some spacecraft and might be defended as separate line items in the NASA budget, should include special-purpose monitoring telescopes of intermediate (60- to 100-inch) aperture, large optical telescopes in both hemispheres, and a large steerable paraboloid radio telescope.

The Astronomy Missions Board believes that the long-range program described in this position paper fully complies with NASA's request for the creation of a worthwhile and imaginative long-range program in space astronomy. It includes a careful assignment of priorities and balanced allocation of resources in order to optimize scientific progress on such problems as the origin of the universe, the course of stellar evolution including the ultimate destiny of the Sun and solar system, the existence of other planetary systems, some of which may support other forms of intelligent life, and other problems with deep philosophical significance which are of great interest to everyone and are therefore properly supported by public expenditure. The Board proposes this program to NASA and to the country with its unanimous and enthusiastic endorsement. We believe that the program is one in which scientists from many disciplines will want to participate, and that its implementation will result in a vast accumulation of new and fundamental scientific knowledge.

Finally, we again wish to point out that we regard this report as an ongoing working paper to be reviewed and then revised and updated as necessary, so that it always reflects the best judgment of the scientific community and the march of scientific discovery.

# Appendixes

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## APPENDIX A

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— NATIONAL AERONAUTICS AND SPACE ACT OF 1958

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