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SPACE RESEARCH

DIRECTIONS FOR THE FUTURE

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PART TWO

Optical Astronomy

Solar Astronomy

Radio and Radar Astronomy

X-Ray and Gamma-Ray Astronomy

Physics and Geophysics

SPACE SCIENCE BOARD
NATIONAL ACADEMY OF SCIENCES
NATIONAL RESEARCH COUNCIL

SPACE RESEARCH

DIRECTIONS FOR THE FUTURE

REPORT OF A STUDY
by the
SPACE SCIENCE BOARD

WOODS HOLE, MASSACHUSETTS
1965

PART TWO

SPACE SCIENCE BOARD
NATIONAL ACADEMY OF SCIENCES – NATIONAL RESEARCH COUNCIL
WASHINGTON, D.C. JANUARY 1966

SPACE SCIENCE BOARD

FOREWORD

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

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

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

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

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

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

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

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

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

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

Harry H. Hess, Chairman
Space Science Board

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II

Optical Astronomy

1. SUMMARY AND RECOMMENDATIONS

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

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

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

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

RECOMMENDATIONS

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

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

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

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

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

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

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

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

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

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

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

2. RESEARCH OBJECTIVES

The broad goal of astronomy is the understanding of the vast physical universe in which the Earth is located. To achieve this goal requires answers to many sweeping and challenging questions, such as:

(1) Is the Universe finite or infinite, and if it is finite, what is its size?

(2) Is the Universe in a steady state, and if not, how did it begin and how will it end?

(3) Do the laws of physics as deduced on the Earth apply without change for all times and over all distances? Alternatively, are there fundamental physical laws or phenomena still undiscovered in terrestrial laboratories, that are observable only on an astronomical scale?

(4) Were the chemical elements that form all matter build up out of hydrogen, and if so, how?

(5) How are stellar systems, stars, and planets formed?

Partly in order to answer such fascinating questions and partly because of an interest in understanding all major constituents of the Universe, astronomers have investigated a host of interrelated problems, such as, the internal structure of stars and sources of stellar energy generation; the structure of stellar atmospheres and the complex physical processes that take place in the outer atmospheres of the Sun and the stars; the dynamical equilibrium and evolution of a stellar system, such as a rotating galaxy composed of many billions of stars; and the formation, evolution, and gravitational contraction of a rarefied cloud of interstellar matter, which forms the birthplace of new stars.

Optical space astronomy can make major contributions to these studies in two ways. First, with a telescope in space, observations of radiation from the stars and from nebulae can be made in the ultraviolet, at wavelengths down to the absorption limit of interstellar hydrogen at 912 Å, and in the infrared. Second, a telescope above most of the Earth's atmosphere can, in principle, distinguish fine detail with a spatial resolution limited only by the diffraction of light. Because of the enormously greater simplicity and economy of ground-based observations, it is certain that in the foreseeable future most astronomical research will be carried out from the ground. However, some information cannot be obtained from ground-based observations, and certain key questions in astronomy require space telescopes for their answers.

Astronomical research problems in which space observations could play a decisive role have been discussed in detail in earlier publications ("Science in Space," edited by L.V. Berkner and H. Odishaw, McGraw-Hill, 1961; also "Prospectus, 1965," by the Office of Space Science and Applications, NASA), and no exhaustive analysis of this topic was carried out by the Working Group. Four such problems of astronomical research are discussed here briefly as examples.

(1) The cosmic distance scale. The distance scale for the Universe is essentially determined (on the basis of the principle of the uniformity of nature) from measurements of the brightness of individual objects in other galaxies recognizable as comparable in intrinsic brightness with their counterparts in our own galaxy, such as novae, star clusters, H II regions, associations, variable stars, and supergiants. With ground-based instruments, such measurements cannot be made much beyond the Virgo cluster of galaxies, and, as a result, the farther-distance scale is uncertain. A large diffraction-limited telescope in space, capable of producing sharper stellar images against the fainter background light, could extend these measurements to much more distant galaxies. In addition, determinations of the angular diameter of hydrogen emission regions (H II spheres) around early-type stars, which could be made with a high-resolution space telescope, could give an independent measure of extragalactic distances beyond the nearest great clusters of galaxies.

(2) Structure of nuclei of galaxies. Evidence is accumulating that the galactic nuclei are the sources of the vast energies observed from quasi-stellar galaxies, and possibly from other types of radio galaxies as well. These nuclei are too small to be resolved with existing telescopes. A

diffraction-limited 120-inch telescope in space could resolve detail 0.03 sec of arc across, one-tenth the angular size that can be distinguished from the Earth's surface: observations with enhanced resolution could be completely decisive in our interpretations of the nuclei of galaxies.

(3) Distribution of molecular hydrogen. Hydrogen in molecular form may constitute an appreciable fraction of the mass of our Galaxy, and if present at all, certainly plays a dominant role in the thermal balance of the interstellar gas and thus in the process of star formation. There is apparently no way of detecting interstellar H_2 directly from the ground. Observations with a space telescope in the ultraviolet and infrared can determine with precision the spatial distribution of H_2, and much about its temperature as well.

(4) Detailed analysis of low-temperature objects. Dark interstellar clouds, globules, protoclusters, and protostars are expected to radiate energy in the neighborhood of 100 microns. When these objects are projected against a bright background, they can be detected by the absorption that they produce, but measures of the infrared emission would give more useful data for analyzing the physical nature of these objects, which are thought to play such an important role in the evolution of stars and galaxies.

3. THE SHORT-RANGE PROGRAM

Programs of space astronomy that will lead to launchings within ten years may be regarded as of short range. Supporting research that will affect launchings not later than 1975, or that will be helpful in analysis of the results obtained by 1975, may also be regarded as part of the short-range program. This short-range program of space astronomy is discussed here.

The following three subjects are important for both short-range and long-range programs:

(i) Flight hardware, including sounding rockets and satellites.

(ii) Supporting activities, including development of astronomical instrumentation and engineering techniques for space telescopes as well as researches in physics and astronomy needed for the interpretation of space astronomy data.

(iii) Training of scientific manpower; the success of the rapidly growing space astronomy program during the coming decade will depend in large part on the ability and training of scientists who enter this field.

The Working Group discussed certain aspects of these subjects; recommendations reached are given below, together with brief summaries of some of the background discussion.

SOUNDING ROCKETS*

Aerobee rockets without pointing have been successfully used for broadband ultraviolet spectrophotometric measurements of the brighter early-type stars. More refined measurements require pointing, and difficulties experienced with the coarse-pointing system developed for NASA by Space General Corporation have delayed progress in optical space astronomy. Fortunately, recent flights of this system have been relatively successful, and coarse pointing, with an accuracy of about ±2° and a jitter of ±0.25°, is now available for research programs. More sophisticated guidance systems, capable of guiding on a 3rd magnitude star with a precision of 30 sec of arc, are now being developed by the Goddard Space Flight Center and other groups, both in the U.S. and abroad.

It appeared to the Working Group that the sounding-rocket program should play a central part in optical space astronomy for a number of years, if not indefinitely. For obtaining preliminary data, for testing components in flight, and for gaining experience generally in space astronomy, the relatively low cost and frequency with which small rockets can be launched offer very important advantages. The number of rockets available with the present coarse-pointing system has already become rather limited, however. With the recent successes in this field, and the growing interest in infrared research, additional launchings will be needed by existing groups as well as by other astronomers who should be encouraged to take an active interest in this work. The following recommendation was therefore adopted by the Working Group:

Recommendation 1

We believe that continuing research with sounding rockets is of the greatest importance in obtaining exploratory scientific data and in testing space instrumentation in advance of satellite flights. A coarse rocket-pointing system of adequate reliability now seems to be available, and we recommend that the number of launchings of pointed rockets available to optical astronomy, for research in ultraviolet and infrared, be doubled.

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* A fuller discussion of the use of sounding rockets in optical astronomy has been prepared for the Working Group on Rocket-Satellite Research by N.U. Mayall, A. Boggess, and T.A. Chubb, and appears in Chapter 7, this report.

We also recommend that a fine-pointing system for Aerobee rockets be made generally available as soon as feasible.

ORBITING ASTRONOMICAL OBSERVATORY (OAO) PROGRAM

In the present NASA program, four Orbiting Astronomical Observatories are funded, with a fifth approved technically. In all of these, the primary payload is an optical telescope used at ultraviolet wavelengths; x-ray detectors are also included in the first and fourth OAO spacecraft.

In view of the great effort and expense that went into the development of the OAO spacecraft and related facilities, and the high capability for space astronomy anticipated with the OAO, continuation of this program is clearly desirable and inclusion of other fields of space astronomy would be appropriate. If the Apollo Extension Systems (AES) program is modified to provide a comparable capability, this system, which can be manned and which therefore is potentially more reliable, might well replace the unmanned one. However, the OAO program should be continued until the AES program is sufficiently far advanced that definite launchings can be scheduled and the astronomical capability of the AES can be reasonably well assured. (A definite recommendation to this effect is included in Recommendation 2, below, which is concerned primarily with the AES.)

It will be noted that infrared research is listed as one of the fields that should be included in the future OAO program. This recommendation reverses the stand taken by the Iowa Study in 1962. During the last three years, important progress has taken place in infrared astronomy from the ground and from balloons, and active plans for sounding-rocket flights in this field are under way. It may be anticipated that, in the near future, experience obtained in these researches will indicate definitely how much would be gained scientifically by devoting an OAO to infrared studies.

APOLLO EXTENSION SYSTEMS (AES) PROGRAM

This program can make use of man in operating and maintaining sophisticated scientific equipment. While AES flights should yield useful and scientific data, the Working Group felt that a more important aspect of the AES program was the intermediate step that it provides toward a much larger instrument, which in the opinion of the Group should constitute the objective of the long-range program in optical space astronomy (see Section 4).

In particular, the Working Group felt that, as part of the AES program, it is essential to test at a relatively early stage the ability of man to adjust, maintain, repair, and occasionally operate a large space telescope. To this end, it was proposed that a least two telescopes of intermediate aperture (40 to 80 inches) be included on AES flights (see Proposed Schedule, in Section 4.2 below). Because of their greater aperture and presumably their greater flexibility and repairability, these AES telescopes might, if successful, carry out more effectively some of the tasks that later OAO's might otherwise undertake; if so, the later OAO's might be phased out. Possibly, after an AES telescope had operated for a substantial period with some particular instrumentation, men might substitute other, different instrumentation: for example, they might replace ultraviolet sensors with infrared sensors.

Since the AES flight tests would be critical for obtaining information necessary to the development of the larger telescope that would presumably follow, it is essential that the flight tests not be dropped even if the subsequent AES program were cancelled for some reason. In fact, apart from the overriding priority necessarily given to the astronauts' safety, the primary mission of the AES flights carrying astronomical telescopes should be the operation of these telescopes.

The following recommendation was adopted by the Working Group:

Recommendation 2

We believe that the presently planned Apollo Extension Systems program provides an important intermediate step in the development of the Large Orbiting Telescope, and also provides an opportunity for significant scientific observations. We recommend that two or more telescopes of appreciable aperture (40 inches or larger) be launched as part of this program.

We further recommend that, when major astronomical experiments are included in AES launchings, the experiments be designated as the primary objectives of these flights.

Until developments in the Apollo program make it possible to schedule definitely a number of AES launchings for scientific purposes, we recommend that the OAO program be continued, with experiments in the following fields: high-resolution imaging; infrared photometry and spectrophotometry with cryogenics; planetary spectroscopy in the ultraviolet; polarization and photometry; x rays.

INSTRUMENTATION

Photography. Photography today has an established usefulness in astronomy that no other image recording and storing technique can rival. In addition, there seems to be little immediate prospect that image tubes of any sort will rival a photograph in the number of information elements that can be recorded simultaneously*. We therefore foresee that, for some purposes, photography will be used in space for some time, provided that the photograph can be returned to Earth, or can be processed in space, scanned electronically, and the data telemetered, without degradation, to the Earth. (In Appendix 2, G. Münch discusses the technical properties of photographic emulsions, and describes recent developments in this area.)

Development of new emulsions with higher quantum yield would do much to enhance the value of photography for all astronomy. There does not seem to be any real physical reason for the quantum yield of the photographic process to be less than the 20% obtainable with photoelectric surfaces. For space application, development of an emulsion sensitive only in the ultraviolet (such as a solar-blind phototube) would be desirable, as would also be the development of an emulsion sensitive beyond 1.3 microns.

Image tubes. The primary advantage of an image tube for taking pictures with visual or ultraviolet radiation is its sensitivity, which ideally might be an order of magnitude higher than is obtainable with present emulsions. For space application, a secondary advantage of an image tube, when used with an electron-beam readout, as in a television camera, is that the signal can easily be telemetered to Earth. However, present television cameras have not yet been used successfully for astronomical programs; astronomical research requires precise photometry and long integrating times, which are unimportant for commercial television. Of the tubes now available, it appears that some might be usefully adapted for astronomical work. Further test and development in this area seems highly worthwhile, since a reliable image tube with significantly greater sensitivity than photographic film and with electronic readout would be extremely useful in optical space astronomy.

Present image tubes can record about 10^5 independent picture elements, as compared with at least 100 times this many on large photographs. It is evident from the Appendix that there is no clear agreement as to the number of resolution elements required in the pictures recorded for astronomical research. Quite possibly, even if image tubes are generally used in space astronomy, photography will still be in demand for special programs requiring a wider field.

Infrared detectors. Astronomical research at wavelengths between about 10^{-4} and 10^{-1} cm is limited by the sensitivity of present detectors. A brief survey of available detectors in this range is given in Appendix 2

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* But see p. 30, "Note on Recent Developments in Image Tubes" added in proof.

by F.J. Low. Especially at wavelengths longer than 3.5 microns, it appears that, by use of liquid-helium temperatures (see paragraph on Cryogenics, below) and by a careful selection of materials, substantial improvement in signal-to-noise ratios should be achievable over currently available devices. Intensive research in this field appears highly desirable.

Solid-state imaging devices. Most imaging work has been done either with chemical (film) or photoelectric devices, but, in principle, solid-state techniques might be utilized. These would have the advantage of being usable at infrared wavelengths. Thus far, Westinghouse (under contract with the NASA Marshall Space Flight Center) has made a silicon mosaic with 50 x 50 elements in a 2 cm x 2 cm square, but the picture is very grainy, as one would expect. Extremely high resolution could, in principle, be obtained, equal to the spacing between the trapping sites in the semiconductor; this spacing may be the interatomic distance in the crystal, but it is more likely to be the distance between the impurity sites. Possibly, the image could be read off the crystal by scanning the back side with a spot of light (for semiconductors in which incident light lowers the resistance) or with a shadow-spot (for semiconductors in which light raises the resistance). The size of the scanning spot is likely to set the limit to the resolution.

Cryogenics. Low temperatures are needed to achieve high sensitivity in detectors for wavelengths longer than 1 micron. Although 4 to 10°K may suffice for the interval from 1.0 to 1.8 microns, much lower temperatures are needed at longer wavelengths. (See two paragraphs immediately above, and Low's Appendix.) Solid H_2 is an efficient coolant for temperatures between 6 and 20° K. Ordinary liquid He^4 can be used from 1 to 6° K. Liquid He^3 provides useful cooling between 0.25 and 1 °K. In order to store adequate quantities of these coolants in a small space, they must be kept at rather high pressures; also, fairly complex devices must be developed to regulate the temperature and pressure and to avoid waste. These problems appear to be more tractable in a space environment than those associated with mechanical refrigerators. Studies by the NASA Ames Research Center indicate that 65 pounds of solid H_2 would maintain a detector at 10°K for about one year. Research and development along these lines should be continued.

As a result of its discussion concerning instrumentation, the Working Group adopted the following recommendation:

Recommendation 3

We recommend that NASA be urged to support the development of the detectors required in space telescopes, together with the necessary auxiliary equipment. Particular emphasis should be placed on the following problems:

(i) Development of new photographic emulsions or the equivalent, with (a) higher quantum yields, (b) selective responses in the ultraviolet, and (c) effective responses beyond 1.3 microns.

(ii) Adaptation of television techniques for astronomical use.

(iii) Development of new infrared detectors.

(iv) Development of image-registering devices using solid-state detection.

(v) Development of the cryogenic apparatus needed to maintain temperatures as low as 0.25 °K in a satellite.

Gratings. Astronomical spectrophotometry requires gratings of high efficiency and high optical quality. Much of the advantage of a large telescope in gathering photons can be lost if the grating does not concentrate in a single order most of the light of a given wavelength. Precise photoelectric spectrophotometry requires that the grating response be relatively uniform. High resolution and freedom from ghosts are also very important. Because of their suitability for spectroscopic observations with an image tube, which has a small area of sensitivity, echelles are of particular interest for space astronomy. An imaginative program for developing gratings and echelles of higher quality would be of very great assistance to optical astronomy in space. The following recommendation was adopted by the Working Group:

Recommendation 4

We strongly emphasize the importance of improving the efficiency, uniformity, and freedom from imperfection of gratings and echelles, especially for ultraviolet work. Additional effort in this area would be of very central importance in the space astronomy program.

Interferometry. In principle, a stellar interferometer can make fundamental measurements of stellar diameters. Moreover, just as in radio astronomy, many observations with a two-element interferometer can, in principle, produce a picture the ideal resolution of which would correspond to that of a telescope whose aperture equals the maximum spacing between the elements (aperture synthesis). In practice, the poor seeing produced by the atmosphere has made the use of stellar interferometers on the ground very difficult; Michelson's 20-foot instrument has produced results only under exceptionally good conditions, and the 50-foot beam interferometer has never worked satisfactorily. Above the atmosphere, these particular difficulties would not be present, and interferometric techniques could be an important tool for optical space astronomy.

Recent studies give some hope that the seeing problem may be ameliorated with modern high-speed electronic methods of observation. Experiments using such methods are being discussed at the University of

Chicago, where two telescopes would be used in an interferometer system. Clearly, optical interferometers should not be sent into space until these possibilities for use on the ground have been explored further. Nevertheless, in view of the potential importance of an operable stellar interferometer, the following recommendation was approved:

Recommendation 5

In view of the importance to astrophysics of a knowledge of the diameters of celestial objects of extremely small angular size, we recommend that the development of optical interferometers of various types be pressed, bearing in mind, however, that such instruments may initially prove to be more useful for ground-based observations than for space.

Miscellaneous instrumentation. Among the various instrumental possibilities discussed by the Group, without any action taken, the use of space vehicles for absolute astrometry may be mentioned. Among the possibilities mentioned were: photographing a satellite of known position against a star field to obtain absolute positions; rotating a telescope or other object at a uniform rate, and using this as a standard for absolute positions; development of an optical system that would rotate a light ray through some fixed angle (say 90°) with a precision of a small fraction of a second. Further study is desirable to determine the ways in which precise astrometric observations can best be made from space.

OPTICAL RESEARCH AND DEVELOPMENT

The optical-performance requirements of a large space telescope are extremely exacting and difficult to meet. The mirror figure should be as close as possible to the theoretically desired surface, preferably within about a fiftieth of the wavelength to be used for high-resolution studies, and should not change appreciably with the passage of time or with changes in the thermal environment. To achieve this objective requires methods of measuring the actual figure as well as special techniques to hold the figure constant. Research on these problems has been carried out at a number of locations, partly in connection with the Stratoscope II balloon telescope program and partly in connection with plans for large satellite mirrors.

In addition, the reflecting coating applied to the mirror should have a high reflectivity for all wavelengths of interest. An aluminum coating applied in a vacuum appears to have exactly this property, with high reflectance down to wavelengths less than 1000 Å. To eliminate oxidation of the surface aluminum, which occurs very rapidly in air, it may be necessary to coat satellite mirrors in orbit. Dielectric coatings may still be required, however, and further exploration of the properties of these coatings is needed.

Unquestionably, other problems in the design and construction of space telescopes also require intensive effort. Most of these, however, lie outside the competence of astronomers, and many were not discussed in detail by the Working Group, though some other engineering problem areas are referred to in Section 4 below. The following recommendation, dealing with research and development in optical systems, was approved by the Working Group.

Recommendation 6

We recommend that NASA be urged to support basic research and development leading to the optimum design and construction of space telescope optics. The vital areas are:

(i) Improved mirror materials and construction to provide adequate thermal and structural stability, including particularly methods of measuring and reducing internal strain.

(ii) Mirror surfaces to provide high ultraviolet reflectivity, precision of figure, and freedom from scattering.

(iii) Methods for rapidly evaluating mirror figure and alignment under normal and zero gravity.

(iv) Methods for generating and maintaining diffraction-limited mirror quality in space, by passive or active means.

GROUND-BASED ASTRONOMY

As pointed out at the beginning of this Section, space astronomy depends vitally on ground-based astronomy: first, as a source of scientific data and information and of trained manpower, and, second, as a means to follow up discoveries in space. To cite an example—one of many—of this dependence on ground-based observations for scientific information: the limited amount of spectroscopic information that has thus far been obtained for stars in the southern sky will make it very difficult to carry out much research on the ultraviolet spectra of these stars; thus, to use a satellite telescope for a spectroscopic survey of southern stars would be enormously wasteful unless such ground-based information were also available. Hence, space astronomy cannot sustain a healthy growth unless ground-based astronomy is also expanding at a somewhat comparable rate. To emphasize this point the Working Group adopted the following recommendation:

Recommendation 7

To make efficient use of orbital telescopes requires the most complete knowledge obtainable with ground-based instruments, and requires also the education of many new young astronomers. We wish to emphasize most strongly that increased support of ground-based astronomy, in the spirit of the Whitford Report, is urgently needed for the continuing healthy growth of astronomy in general, and of space astronomy in particular.

4. THE LARGE ORBITAL TELESCOPE

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

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

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

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

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

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

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

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

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

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

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

Following considerable discussion the Working Group adopted the following recommendation:

Recommendation 8

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

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

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

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

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

DESIGN PARAMETERS

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

Aperture

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

Role of Man

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

Location

After reviewing the recommendations of the Report on Lunar Exploration Systems after Apollo (LESA, North American, 1965), the Group discussed the relative advantages of the following three different locations for a large space telescope: low orbit (below the Van Allen belts), at 400 km altitude or less; high orbit (above the Van Allen belts), at 30,000 km altitude or more; and on the Moon. Most of the considerations examined would appear to favor the high orbit. As compared with location on the lunar surface, the advantages of a high orbit include no gravitational flexure, no secondary micrometeorites, and lower cost. A possible major disadvantage of the high orbit is greater risk of exposure of equipment and men to high-energy radiation from solar flares, though evidence presented to the Group suggests that adequate shielding is no problem. Objects close to the Sun, however, might be more difficult to observe from a high orbit than from the lunar surface. As compared with a low orbit, the advantages of the high orbit are: negligible occultation of objects by the Earth (in a low orbit, occultations complicate the programming and are likely to reduce the net observing time by about one half); nearly constant thermal environment, which much simplifies the maintenance of the mirror figure; reduction of external torques due to gravity gradients, magnetic fields, and air drag by at least two orders of magnitude, with resultant simplification of the guidance problem; darker sky than in low orbit, where airglow may contribute light; and virtual absence of oxygen atoms striking the telescope and oxidizing the aluminum. From a high orbit, communication with the ground might be simplified by continuous radio contact, but, as compared with a low orbit, communication would be complicated by the increased distance. The greater exposure to solar flare radiation may be an important disadvantage of the high orbit, especially in view of the longer time (at least 10 hours) required to return a human operator to Earth from the high orbit. A very clear disadvantage of a high orbit is that it requires a Saturn V for launching instead of a Saturn IB; since this additional cost would be required for each visit by men, this could be a conclusive argument for the low orbit. The Working Group unanimously came to the conclusion that, on technical grounds, the high orbit appears at the moment to be the optimum location for the LOT.

Optical Design

A conventional parabola-hyperbola or a Ritchey-Chrétien system seems indicated. The primary should have a relatively low focal ratio to minimize the over-all length of the instrument. Use of the prime or Newtonian focus would not seem to offer any particular advantages, and all of the instrumentation would presumably be at the Cassegrain focus, possibly with tiltable mirrors to direct the light toward the desired instrument or sensor. Careful baffling would be required to keep earthlight as well as sunlight out of the optical path, and the secondary supports should presumably be apodized (with Couder strips). Automatic focusing would presumably be required and, probably, automatic collimation as well.

SCHEDULE

To visualize how long it might be before the LOT could be launched, the Working Group formulated a very tentative schedule (see Table 1). This schedule was regarded as the fastest that could be imagined for such a large instrument, consistent with careful engineering and optical-design studies and with detailed tests of components and concepts both on the ground and in space. By comparison with the history of other large astronomy programs, some extension of this 14-year schedule might be anticipated. While some observers experienced with large space programs regarded the schedule as optimistic, others were of the opinion that a substantial acceleration would be possible.

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

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

Table 1, continued

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

* Approved by NASA, but not funded.

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

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

ADMINISTRATIVE PROBLEMS

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

Preliminary phase

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

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

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

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

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

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

Design and construction phase

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

Operations phase

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

APPENDIX 1: LIST OF PARTICIPANTS

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APPENDIX 2: WORKING PAPERS

DETECTORS FOR DIFFRACTION-LIMITED IMAGING

G. Münch

My task is to analyze, as the basis for discussion, the subject of the dimensions of the diffraction-limited field desirable for a large orbital telescope. The question clearly does not have a general answer, and in order to reach some sort of guiding decision we have to consider the following factors:

(i) Nature of the astronomical problems to be studied,
(ii) Limitations imposed by the effective f-ratio employed and resolving power of detector,
(iii) Guidance requirements.

The number of important astronomical problems requiring the highest resolution is very large. Indeed, every one of the outstanding astronomical problems of our time requires high resolution for further understanding. Among the outstanding problems of current interest we may mention the angular dimensions of the quasi-stellar radio sources, the structure of the nuclei of galaxies, the resolution of galaxies into stars to refine the distance scale, and the study of the faint end of the population of clusters of galaxies and stars. Of paramount importance for the cosmological problem would be the study of the background population of galaxies, say five magnitudes below the limit set at present by the various components of the surface brightness of night sky. A field as small as 20 sec of arc, truly diffraction-limited, say with resolution of about 0.″03, would provide information toward the solution of these important problems. It would appear that such a small diffraction-limited field should not be traded for a larger field with degraded resolution, although it is obvious that a larger field would be highly desirable for reasons of economy in the process of data acquisition. However, the linear dimensions of the diffraction-limited image should correspond to the resolving power of the detectors used, and in this context we have to consider the limitations introduced in (ii), above.

To consider a specific case, we take as a guide the 120-inch, f/4 primary discussed in the Boeing report.[1] The dimensions of the field and of the Airy disk (at $\lambda = 0.55\,\mu$) corresponding to various effective focal ratios are given below:

	Field	Airy Disk (μ)	Matching Detector
f/4	6 sec of arc = 0.4 mm	2.5	Very fine grain
f/8	20 sec of arc = 2.5 mm	5	V-O (>225 line/mm)
f/15	40 sec of arc = 9 mm	10	III-O (96-135 lines/mm)
f/30	120 sec of arc = 55 mm	20	IIa-O (50-70 lines/mm)
f/100	5 min of arc = 450 mm	66	Phosphors*

In the last column we have entered the photographic emulsion with resolving element not larger than the Airy disk. Only for the f/100 beam could we use a phosphor.* But for a 1-inch tube the dimensions of the field would be effectively limited to 16 sec of arc. The emulsions ordinarily used in ground-based astronomical observations and specially treated for reciprocity failure, such as Eastman IIa-O, would require a beam of about f/30. Emulsions of higher resolving power are not generally treated for reciprocity failure, but assuming that we can do something about it (baking, for example, effectively reduces reciprocity failure of fine-grain emulsions), they can be used just as effectively, for it is known that the quantum efficiency of the emulsions remains very nearly constant as the resolving power increases. For the very highest resolving powers (very fine grain) the loss in speed may amount to a factor of 2, but it is difficult to establish test procedures that apply to such cases.

It may be appropriate to mention here that today there exist very remarkable emulsions for special purposes, which might be of great value in an orbiting telescope if we can treat them for reciprocity failure. The Eastman film type SO-132 or 4404, "designed for extremely high-altitude, stable-platform photography, is rated with a resolving power of 475 lines/mm at a test-object contrast of 1000:1 and a root-mean-square granularity of 0.023, both in excess of the V-O ratings. The ultra-fine-grain emulsion, Kodak Type SO-243, has similar characteristics and could be used at small f-ratios.

The restrictions on the dimensions of the diffraction-limited field imposed by the guidance requirements have been briefly analyzed in the Fecker report.[2] It is stated here that with a 120-inch it should be possible to guide on a 16th-magnitude star. Mean star densities from star counts all over the sky lead to the estimate that in a 60 sec of arc field there would be, on the average, three stars to guide on. This estimate appears somewhat optimistic, and most probably, for extragalactic work, estimates of the number of possible guide stars should be based on star densities at the galactic poles. A field of 3 min of arc at an f/15 ratio would certainly satisfy all of the guidance requirements and also be sufficient for most of the high-resolution astronomical programs.

*** But see p. 30, "Note on Recent Developments in Image Tubes" added in proof.**

References

1. Systems Study of a Manned Orbital Telescope: Midterm Report. Boeing Co., Seattle, Wash., May 1965.

2. Final Report, Feasibility Study of a 120-Inch Orbiting Astronomical Telescope, J.W. Fecker Division, American Optical Co., Pittsburgh, Pa., 1963.

Both these reports were prepared for the NASA Langley Research Center.

INFRARED DETECTION

F.J. Low

The following is a brief review of the present status of detection techniques at wavelengths from 1 to 1500 microns.

As a starting point, let us consider the photomultiplier tube with the S-1 surface. The peak quantum efficiency of about 2% occurs at 0.8 micron and, when the tube is cooled to dry-ice temperature, it has a noise equivalent power (N.E.P.) of about 2×10^{-16} watt. At longer wavelengths, the efficiency drops off rapidly.

As we proceed to longer wavelength devices, the relation between detector area and N.E.P. must be taken into account. This subject is somewhat simplified if we stipulate that the smallest focal-plane diaphragm which is useful on large astronomical telescopes is 1 mm in diameter. There are exceptions to this rule, however; in particular, the use of a cooled Fabry lens has been shown to permit detector areas as small as 0.25 mm x 0.25 mm. In future space telescopes, even smaller detector areas should be usable.

At wavelengths from 1.2 to 3.5 microns, the most sensitive detector used thus far for astronomical measurements appears to be a PbS cell cooled with liquid N_2. Its N.E.P., as reported by H.L. Johnson, is 1.0×10^{-14} watt with an area of 0.25 mm x 0.25 mm. Although the peak response and sensitivity of the PbS detector can be varied a little with temperature and construction, it has already reached its practical limit of development. Recent work with doped germanium cooled to 4°K indicates that the gap between the S-1 and PbS peaks can be partially filled in. Experiments with this inverse photoconductive effect covering the range 0.7 to 1.7 microns show that, because of its extremely low noise, this new detector may come close to an N.E.P. of 1×10^{-16} watt.

In the range between the PbS cutoff, at 3.5 microns, and the practical onset of superheterodyne reception, at 3.5 mm, there are two classes of detectors to be considered: (i) the cooled thermal detectors, represented in practice by the low-temperature germanium bolometer and (ii) the cooled photoconductors, represented chiefly by impurity-doped germanium cells such as mercury-germanium.

The latter class includes a wide variety of detectors developed originally for military applications in which the background radiation is high and the response must be very fast. Great attention has been paid to minimizing the cooling requirements for these detectors. Mercury-doped germanium cells cooled with liquid H_2 have been applied to ground-based astronomical observations, providing an N.E.P. of about 1×10^{-12} watt at 12.5 microns. Various other dopants have been used to extend the long-wavelength cutoff. It appears likely that the N.E.P. of very small detectors of this class may be better than 1×10^{-13} watt. Indium antimonide cooled to liquid-helium temperatures provides a unique photoconductive response out to wavelengths of a few millimeters. A large-area (about 1 cm^2) Putley detector has been reported at 1×10^{-11} watt.

Unlike the photoconductive cells, the cooled thermal detector can, in principle, be designed to work efficiently at any wavelength. The germanium bolometer has been used in a radiometer optimized for the 1.0- to 1.5-mm window. When this instrument is cooled to 2.0°K, an N.E.P. of 4×10^{-14} watt has been achieved. A number of 1 mm x 1 mm germanium bolometers have been used at 5, 10, and 20 microns with an N.E.P. about 2×10^{-13} watt. At a minimum size of 0.25 mm x 0.25 mm and with special care in reducing the background radiation, the N.E.P. at all wavelengths should be about 1×10^{-14} watt for bolometers cooled to 2°K. By cooling to 0.9°K, the practical limit for ordinary liquid helium, the N.E.P. should be reduced to 2×10^{-15} watt. If liquid helium-3 is used to reach 0.25°K, even higher sensitivities may be attained. At temperatures below 1°K, a number of possible thermal detector mechanisms should be considered. It is quite clear that materials other than the few that have been investigated may provide substantially higher sensitivites. Indeed, the germanium bolometer is only one approach among many.

The devices discussed above have all been broad-band incoherent detectors. The maser or laser should be included in the discussion since it may provide amplification or detection very close to the photon limit at a specific wavelength. However, the narrow bandwidths attainable restrict the usefulness of such devices in astronomy. If a laser were tunable over a sufficiently large range, certain types of spectroscopy would become possible, particularly the study of narrow emission lines. It should be noted in this respect that even if a maser were available at 1 mm, its sensitivity as a radiometer for continuum measurements would be less than that of the best thermal-detection radiometer utilizing the low-temperature germanium bolometers which we already have.

Thus far, only individual detectors have been considered. Although one or more individual detectors may be used to scan an area of sky to build up an image or to scan a spectrum, the light-gathering power of the telescope is then only partly utilized. The problem is not only to separate the telescopic image into its independent elements but to record all of the information each element contains. This goal is approached by the photographic plate and by photoemissive image tubes. Unfortunately, existing infrared image devices fall short of the goal in both sensitivity and storage capability. This remains the case even though diffraction reduces the number of independent elements in the image at longer wavelengths. The bright sky background in the infrared would reduce the effectiveness of such devices for ground-based observations; however, their significance for space astronomy cannot be overemphasized.

From the foregoing discussion the following conclusions may be drawn: despite sizable advances in detector technology, the limiting factor in a well-designed infrared spaceborne telescope is detector sensitivity. Unless new breakthroughs occur in photoconductive techniques, the best hope for extending sensitivity beyond 2 microns is to develop thermal detectors cooled below 1°K. Although great effort is required to improve image devices in the infrared, their potential applications in space astronomy are so fundamental that their development must be continued.

NOTE ON RECENT DEVELOPMENTS IN IMAGE TUBES

L.W. Fredrick
(Added November 1965)

Dramatic developments in the field of high-resolution phosphors for image tubes have been reported by several research groups at the Symposium for Photoelectric Imaging Devices held in London, 20-24 September 1965. Improved techniques now make it possible to deposit phosphor grains with diameters in the range of 5 microns down to less than 1 micron, to form screen coatings that yield image resolutions of 100 line pairs/mm or better. This resolution is degraded somewhat in the multiplication type intensifiers. The efficiency of the new coatings is claimed to be better than the older coatings by one group, but not quite as good by another group. Nevertheless, the prospect that image tubes that compare favorably with photographic emulsions for high-resolution astronomical work will become available within a few years, is greatly enhanced as compared with the prospects in June 1965 when the Summer Study met.

At the same symposium, developments in image orthicon tube technology were announced that should improve the application of this type of tube to astronomical problems. In particular, the Westinghouse group discussed the new secondary electron conduction target and the long storage times available. Images were stored and retained for periods of 24 hours with the tube turned off.

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III

Solar Astronomy

1. INTRODUCTION

AIMS OF THE WORKING GROUP

As part of a summer study of space science by the National Academy of Sciences, the Solar Astronomy Working Group met at Woods Hole, Massachusetts, from June 21 to July 3, 1965. In many ways this study can be regarded as a follow-up to a similar conference held at the State University of Iowa in 1962, the proceedings of which have been published as "A Review of Space Research" (Publication No. 1079 of the National Academy of Sciences—National Research Council).

The direct objective of the Solar Astronomy Working Group was stated as follows: "To examine future needs of, and opportunities for research in solar astronomy, using all space techniques ranging from orbiting observatories more advanced than the Advanced Orbiting Solar Observatory (AOSO) and possible manned facilities, to small satellites and sounding rockets."

Scientists participating in the work of the Working Group on Solar Astronomy are listed in the Appendix. The recommendations of the Working Group are listed at the end of this Section; the discussion leading to these recommendations is found in Section 2.

SOLAR ASTRONOMY AND SOLAR SPACE ASTRONOMY

At the beginning of a report on solar space astronomy, it is appropriate to emphasize the unity of solar research, and to point out that observations from space and observations from the ground are simply two aspects of a single venture. Clearly, if one wants to obtain all possible information about the Sun, one uses every means at his disposal; one must never forget that space and ground-based astronomy are partners—not competitors—and the knowledge to be gained from using the two together is very likely greater than the sum of the two used separately. If, for example, the time development of flares could be followed in the light of He I λ584 from a spacecraft and in the light of He I λ10830 from the ground, it would be far more valuable to observe a particular flare simultaneously in both lines than simply to observe any flare in either line separately.

There are three major reasons why the Sun is important: first, because of its intrinsic interest as the only nearby star; second, because of its effect on the planets and, in particular, the Earth and the human race; third, because it is a valuable astrophysical laboratory.

As the only star on which detailed observations can be made, the Sun holds a unique position in stellar astronomy. Because the disk of the Sun can be resolved, single features—sunspots, prominences, granules, and flares—can be observed, and spectral scans across the disk from center to limb can be made, which in turn give directly the temperature and density in the photosphere as they vary with depth. The solar atmosphere can be used as a standard against which theories of stellar atmospheres and spectral-line formation can be tested. Stellar spots and cycles of activity can at present be studied in detail only on the Sun. Theories of convection can be tested on the Sun, where granulation and chromospheric microstructure can be seen. Chromospheres and coronas are seen nowhere else in as great detail as on the Sun. Finally, the Sun is a stable, main sequence star that serves as a photometric and spectroscopic standard for stellar work. Every new piece of information about the Sun contributes to our comprehension of the stars.

Knowledge of the Sun is indispensable to an understanding of the physics of the Earth and other planets. The Sun is the source of virtually all the heat and light a planet receives. Photon flux from the Sun causes dissociation, excitation, and ionization of atoms and molecules in planetary atmospheres, produces ionospheres, maintains planetary heat budgets, causes escape of atmospheres from the planets, is responsible for weather, and controls any life that exists on the planets. Particle flux from the Sun affects planetary ionospheres and magnetic fields, produces auroras, fills the interplanetary region, and changes the chemical abundances by producing nuclear reactions in planetary atmospheres. In addition, solar radiation (photon and particle) affects the Moon, comets, meteoroids, and asteroids, as well as grains in interplanetary space. In fact, the whole solar system can be viewed as being imbedded in the outer solar corona.

The importance of the Sun to life can scarcely be overemphasized: solar energy ultimately sustains practically every living organism—both plant and animal. Radiation from the Sun has also controlled the atmosphere and environment in which life has developed, and thus has been the principal determinant in shaping the course of evolution. Biology is therefore interested not only in the present radiation (photon and particle) from the Sun but also in any changes that may have occurred during the Earth's history. With the dawn of the space age, an upsurge of interest in exobiology—life outside the Earth—has taken place. This has increased the demand for an intimate knowledge of all solar radiation, especially in the ultraviolet and x-ray regions of the spectrum.

A subject deserving special consideration under the general topic of the effects of the Sun on its environment is the effect of the Sun on man. Living as he always has, on Earth, where the atmosphere shields him from the harmful radiations of the Sun, man has tended to look upon the Sun as completely benign. With the coming of the space age and man's emergence from within the protective atmospheric envelope, however, the problem of shielding astronauts from solar radiation must be faced. Solar protons (and possibly x rays) emitted at the time of solar flares pose the most serious danger. If man is to work in space, outside of a heavily armored module, the Sun must be constantly monitored so that the astronaut will have immediate warning of any solar event from which radiation might be sufficiently intense to force him to re-enter his spacecraft. Further knowledge about the nature of flare events, which would enable scientists to better predict their occurrence or better shield the astronaut, would be of substantial benefit.

Ever since Janssen's discovery of helium on the Sun, it has been apparent that the Sun can be used to complement the terrestrial scientific laboratory, for there are available in the Sun combinations of temperature, density, and path length quite beyond terrestrial capabilities. Although its potential value in this respect has not been fully utilized (and is perhaps not clearly appreciated by all scientists), the Sun has been exploited as a laboratory by workers in several branches of physics. The measurement of precise wavelengths of spectral lines from highly ionized elements in the corona was used by Edlén to determine a number of atomic parameters otherwise inaccessible. Recently, both in this country and abroad, scientists have used the Sun as a source for spectra of wavelengths that are not otherwise producible in the laboratory. In the same way, observation of many strong lines near 170 Å in the solar spectrum stimulated efforts in several laboratories to obtain spectra from highly ionized atoms in an attempt to identify these solar lines. As the ultraviolet spectrum of the Sun is examined more carefully, one may expect to gain important new knowledge of atomic physics and spectroscopy. The Sun has also been, and still is, important for studying nuclear reactions and testing theories of nucleogenesis. The solar atmosphere provides—in such features as spicules, the corona, and the convection zone—a large-scale laboratory in which phenomena of aerodynamics and hydrodynamics can be observed. In fact, observations of the outward particle flux from the Sun have stimulated a great deal of important work on the solar wind and interplanetary plasmas. Finally, the Sun provides unique opportunities for the study of plasma physics and magnetohydrodynamics. In addition to the solar atmosphere itself, which can be regarded as a giant plasma, such features as sunspots, prominences, flares, and the corona are examples of the interaction between plasmas and magnetic fields. The Sun is a readily available source in which many waves and oscillatory phenomena of interest in magnetohydrodynamics—acoustic waves, magneto-acoustic waves, electromagnetic waves, and Alfvén waves—occur naturally. As solar research progresses, discoveries of interest to several disciplines will undoubtedly be made, and many new uses will be devised for our convenient astrophysical laboratory.

In summary, we emphasize once again the central position of solar astronomy and the contributions that it makes to such fields as stellar astronomy, radio astronomy, planetary atmospheres, interstellar matter, biology, atomic physics, and meteorology, as well as its influence upon such very practical matters as radio communication on the Earth and the safety of an astronaut.

Although the reasons for observing the Sun from above the Earth's atmosphere have already been listed in many places, we mention them once again in order to remove any existing doubt concerning the benefits to be gained by putting telescopes into space. The most obvious reason is the possibility of observing the ultraviolet and x-ray solar spectrum. Gases in the Earth's atmosphere completely block from our view all radiation with wavelengths shorter than about 2900 Å. Yet this obscured spectral band is vitally important in understanding the Sun for several reasons: the resonance lines of most elements lie within it; it is emitted by the interesting region of temperature inversion just above the photosphere; it contains many of the strongest coronal lines; and the most violent variations of radiation with solar activity are observed within it. The preliminary work that has already been done in the ultraviolet spectral band by means of sounding rockets and satellites has added greatly to our knowledge of the Sun. More complete observations (including x-ray and gamma-ray detection), offer the most fruitful means of understanding the processes taking place in the upper solar atmosphere. It is especially important for observations to be made during the years leading up to and through the coming solar maximum of 1967-1970.

A related advantage, and one that has not often been discussed, is the opportunity to observe the Sun in the far infrared—from 20 microns up to 1 millimeter. Our ignorance of this region is almost complete; we do not even know the energy distribution, much less any details of molecular bands or lines. Although the infrared is probably of less importance than the ultraviolet, and for this reason has often been neglected in space-science planning, there are several interesting observations to be made; for example, the detection of the radiation from the photosphere-chromosphere interface (somewhere between 20 and 200 microns) and observation of the molecular spectra of sunspots. Questions about the temperature structure of cool regions (such as above sunspots and near the temperature minimum) can perhaps be answered by observations at those wavelengths. Again, since the atmosphere of the Earth entirely prevents us from detecting this radiation, such observations can only be obtained from space (perhaps, in this case, airplanes or balloons would suffice).

A crucial advantage that could be obtained from space observation is the increased resolution (clarity) of small features on the Sun. The developments of the past 20 years in the theoretical and observational study of the dynamical and magnetic properties of the quiet solar atmosphere and the many features of solar activity focus increasingly on the smallest observable structures. Every improvement we achieve in spatial resolution reveals new detail of the greatest significance. The size distribution of granulation, the fine structure of magnetic fields, the stranded structure of loop prominences, the local variations in line profiles, the turbulent velocity fields, and the minute structure in flares are examples. Ground-based observation has carried us to the verge of solutions, but in most instances falls short of the theoretically decisive resolution. While it is obvious that there will always be interesting details too small to be seen with any resolution we may attain, many features of the solar atmosphere should have a size scale in the neighborhood of the scale height. Except for the very thin layer at the base of the photosphere, the scale height is of the order of 100 km or more. This corresponds to 0.14 sec of arc in angle. The practical resolution limits of ground-based solar observation are about 0.5 sec of arc for direct photographs and 0.8 sec of arc in the best spectra. Resolution of this grade is the kind one achieves only on very rare occasions in one lucky photograph or spectrogram. The limit is set by poor seeing in the Earth's atmosphere, and could be overcome by space observation.

Another limitation imposed by the Earth's atmosphere is the brightness of the sky. The bright sky near the Sun, at the best observing sites, is rarely less than 10 times the brightness of the inner corona. Nevertheless, ground-based observation has successfully detected the polarized component of the brightest streamers of the white corona out to a height of one solar radius above the limb; however, direct measurement of the electron-scatter brightness and detection of the corona beyond one radius are impossible except during a total solar eclipse. Any continuous watch from the ground for outward-moving plasma clouds is also out of the question. Since the sky is completely dark in space, we can expect to monitor the Sun's corona routinely from space vehicles, which will lead to major advances in our knowledge of coronal processes.

A final important benefit to be gained from observation in space is the continuity in time that can be obtained. Often, critical observations need to be extended for several hours, days, or even months, in order to study the time development of certain features, such as prominences and centers of activity. Such observations must ultimately be made from satellites where they will be independent of terrestrial meteorological and diurnal effects. Real-time monitoring of solar events will be accomplished by such satellites, making communication delays short or nonexistent as telemetry reception becomes possible at the user's site. As space observations become routine, close cooperation between ground and space observations must be maintained for maximum benefit of such solar monitoring.

CURRENT SOLAR PROBLEMS

Solar physics has as an ultimate task of describing completely the structure, understanding thoroughly the dynamics, comprehending fully the origin and development, and predicting exactly the future evolution of the Sun. These ultimate goals, which can never be fully attained, are perhaps best expressed in the form of more specific questions that might be asked about the Sun. The following are some of the major questions with which solar physics is now grappling. What are the details of the processes by which energy is transferred outward from the center of the Sun? What is the source of the sunspot cycle and solar activity? Why, and how, do solar flares occur? How are energetic particles and photons produced? What is the detailed structure of the chromosphere, and what is its connection with the corona and with magnetic fields on the Sun? What is the nature and cause of spicules and prominences? Why is there a solar corona, and how does it produce the solar wind and interplanetary medium? What is the origin and early history of the solar system? What produces the equatorial acceleration of solar rotation? How is the solar magnetic field produced and what is its effect on the solar activity cycle?

Such questions, which have been formulated and listed many times before, are the grand questions toward whose solution all solar physics is directed. But these are too comprehensive to be answered fully or even attacked intelligently. Instead, most solar physics is directed toward the solution of much more specific problems in the hope that the accumulated answers to many smaller problems may eventually provide answers to these larger questions.

The following are some of the more important specific questions with which present solar astronomy is engaged:

Photosphere. What are the size and velocity distribution of the solar granules? What are the source and structure of the weak magnetic fields produced at the surface of the Sun? What is the simplest nonhomogeneous model of the photosphere? What is the relationship between the granules and the super-granulation? Why is there a temperature inversion? What is the detailed variation of temperature with height through the temperature minimum? What is the vertical structure of features observed in this height range? Do these structures vary with the solar cycle?

Chromosphere. What is the morphological specification of the vertical structure (including the size distribution)? What is the velocity distribution in this region and how does it vary both horizontally and vertically? What is the detailed structure of the cells of the chromospheric network? How does the magnetic field vary across the cell boundary? Which features inside the cells are periodic in time? Are any observed features rotating? Is the chromospheric oscillation vertical or horizontal? What determines its period? How does the temperature vary horizontally across the chromospheric cells? How are spicules produced? What is their microstructure? What are the magnetic fields in spicules? Why do spicules seem to favor the edges of the cells? What other aerodynamic phenomena take place in the chromosphere? What is the energy budget of the chromosphere? What types of waves are propagated in the chromosphere and what are their results? What is the temperature gradient as one passes from chromosphere to corona? What causes this temperature gradient? What is the nature of the transition from spicules to the corona? How should we interpret spectral lines formed in the absence of local thermodynamic equilibrium?

Corona. How is the corona heated? Is the corona localized over the edges of the chromospheric cells? Is the corona in equilibrium? What is an M region? What is a coronal streamer? What is the relation of streamers to the solar wind? What coronal phenomena give rise to radio bursts and gusts in the solar wind? What is the magnetic field of the corona and what is its effect on corpuscular streams? How do radio bursts escape from the corona? Is there a difference in chemical composition between corona and photosphere?

Flares. What is the primary flare phenomenon? How do flares occur? What is the source of energetic particles in a flare? Is the energy released primarily over a small hot kernel? What is the relation of the flares to the surrounding magnetic field? How are flares related to the coronal condensations? Are x-ray flares observable at extreme ultraviolet wavelengths? How do active regions with intense magnetic fields arise? What is the physical structure of active regions?

Sunspots. How is the observed brightness distribution across a sunspot maintained? What is the physical structure of sunspots? What is the relation of the magnetic field to the fine structure? How does the granulation behave inside the spots? What is the fine structure of the umbra below the resolving power of Stratoscope I? What system of gas motions exists around the sunspots?

Plages and Faculae. What is the microstructure of plages and of faculae? What is their relation to each other? What oscillation takes place in the faculae? How are the faculae related to the network cells? Is there a difference in coronal heating above faculae and above active regions?

Prominences. Is there a basic lower limit to the size of filamentary structures in prominences? What is the magnetic field in prominences? What is the mass and energy balance between the corona, prominences, and the lower atmosphere? What causes eruptive prominences? Why are loop prominences so extremely hot? What are their special relation to solar flares?

ORGANIZATION OF THE PANEL

To meet its objective, the Working Group divided its assignment into three parts:

1) A critical review of the past and present NASA programs in solar physics and of the Prospectus 1965-1980;
2) An examination of major unsolved problems in solar physics and a specification of the instrumental requirements for their solution;
3) The recommendation of specific experiments that might be initiated in the three time periods 1965-1970, 1970-1975, and beyond 1975.

Certain other matters, such as the role of man in carrying out scientific observations in space and the relation of laboratory work to the NASA mission, were discussed as part of the solution of the problems of solar physics.

There are several alternative ways in which the Working Group could approach its assignments. The physically most meaningful is the problem-solving approach, in which the unsolved questions of solar astronomy (based on the list given above under Solar Astronomy and Solar Space Astronomy) are considered and then observations are sought that would aid in answering them. Experimenters generally prefer an instrumental approach, in which knowledge of the characteristics of an instrument is used to determine what observational data the instrument can obtain. A third possible approach might be termed vehicular, in that the capabilities (size and weight of payload, pointing accuracy, lifetime, orbit, power available, and data-storage and transmission capabilities) of the planned vehicles are first examined and then it is decided which observations could be made from them.

The Working Group favored the first approach, in general, as it felt that this approach would ultimately be most fruitful from a scientific standpoint. Although other viewpoints were adopted at times, an attempt was continually made to relate all discussions to the basic questions of solar physics.

To facilitate the work of the Working Group and ensure complete coverage of the field of solar astronomy, the Working Group was divided into four subcommittees. Since the detection and analysis of solar electromagnetic radiation forms the principal source of information about the

Sun, and since different regions of the spectrum come from different parts of the Sun, a division according to spectral region is to a large extent a division according to region or height on the Sun and hence according to basic questions about the Sun. This division of labor to some extent combines the problem-solving and instrumental points of view. The entire spectrum was therefore divided into sections and a subcommittee was assigned to study each section. Included in the charge given to each subcommittee was the request that it look ahead at least to 1975 and discuss problems, instruments (including the role of man), and vehicles. The assignments were as follows:

a) $\lambda < 500$ Å: Lindsay, Teske, Zirin;
b) $500 < \lambda < 1500$ Å: Athay, Firor, Orrall;
c) $1500 < \lambda < 3000$ Å: Johnson, Smith, Tousey;
d) $\lambda > 3000$ Å: Evans, Howard, Ney.

The reports of these groups were discussed by the Working Group as a whole and are contained in Section 3.

In addition, and partly as a result of the previous work, several informal subcommittees were designated to study and make recommendations about such other topics as the use of rockets, the role of man, the relation of the astronaut-observer to the scientist directing the experiment, the role of the ground-based laboratory in the space effort, and the question of a Moon-based observatory. Discussion and recommendations concerning the findings of all subcommittees are found in Section 2.

RECOMMENDATIONS

The full texts of the recommendations of the Working Group on Solar Astronomy are presented below.

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

(b) That other improvements (such as increased payloads and peak altitudes, increased reliability, and more dependable recovery techniques) be made in existing rocket systems;

(c) That the number of rockets available per year for research in solar astronomy be at least doubled;

(d) That funds for payload development be increased to an adequate level, especially when the triaxial pointing controls become available.

Recommendation 2. (a) That the presently approved Orbiting Solar Observatory (OSO) program be augmented by at least four additional launchings during the period 1970-1972 inclusive;

(b) That no decision be made to terminate the OSO program after 1972 without further review at an appropriate time;

(c) That NASA make every effort to implement such desirable improvements in the OSO spacecraft as increased power, offset pointing, localized raster scans, provision for slightly longer instruments, greater data capacity and more flexible data format, and improved pointing accuracy (15-30 sec of arc);

(d) That consideration be given to injection of one or more OSO spacecraft into a polar retrograde orbit in order to provide continuous surveillance of the Sun.

Recommendation 3. (a) That a satellite with Advanced Orbiting Solar Observatory (AOSO) specifications is an indispensable next step in NASA's solar program, and must be flown close to the coming solar maximum;

(b) That the AOSO program be accorded all the priority necessary to maintain the launch schedule shown in the Prospectus.

Recommendation 4. (a) That manned missions in the 1968-1972 time period, such as the Astronomical Telescope Orientation Mount (ATOM) in the Apollo Extension Systems, are desirable to supplement AOSO, but cannot replace it;

(b) That because it offers the prospect of providing answers to critical questions relating to the technology of manned space telescopes and data recovery, the ATOM concept merits vigorous support.

Recommendation 5. That solar space observation be included in the manned space science program of the Apollo Extension Systems in order to develop the technology of manned space astronomical operations. Such observations, which could attain resolving power of 1 sec of arc in the wavelength region 500-3000 Å, mark the next logical step beyond both AOSO and ATOM.

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

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

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

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

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

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

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

2. DISCUSSION OF RECOMMENDATIONS

GENERAL REMARKS

Scientific progress is usually achieved by a steady, step-by-step process—each step being built on the knowledge obtained in the previous step. Since this process has been of almost universal validity and utility, we would do well to remember it in connection with space science. Because of the rapid progress of astronomy in the past few years, and because of the truly marvelous opportunities for research and discovery arising from man's conquest of space, both scientists and nonscientists are likely to be rather speculative in their thinking about the opportunities of space. We therefore emphasize that space research should be done, insofar as possible, in the traditional, tested manner of science. Thus, for example, such large and expensive projects as the Apollo Extension Systems (AES) and the Manned Orbiting Telescope (MOT) should be firmly based on instruments and methods that have previously been thoroughly tested and proven. It also seems wise to connect planned space research with specific problems, such as those discussed previously, and with the new questions that will undoubtedly be raised in the course of further exploration.

We find good reason to be optimistic about the progress in space research that has already been made. Even though the United States' space effort is still in its infancy, many solid scientific results have already been produced. NASA has shown a great deal of foresight and imagination in developing the large and sophisticated spacecraft now being flown or built, and in providing the scientific community with opportunities for space observation. We commend NASA for the success thus far obtained.

In agreement with our decision that space science should be done in an orderly, step-by-step fashion, we envision a continuing series of instruments of increasingly greater complexity, range, power, and cost—extending from sounding rockets to OSO to AOSO to AES and finally to MOT. This tidy progression does not, however, tell the full story, and several important factors must be considered in order to understand the comprehensive plan that solar physics must follow in accomplishing its mission.

The Sun is a quasi-periodic variable star, exhibiting an approximate 22-year period of magnetic activity and an 11-year period during which the average number of visible sunspots and the scale of energetic activity follow striking cyclic patterns. A great variety of transient phenomena on the Sun, some of which disturb the atmospheres of planets, also vary in intensity and complexity with this cycle. In fact, the study of solar behavior at different phases of the solar cycle represents a major branch of solar research, its aim being to arrive at an understanding of the physical nature of solar phenomena and a theoretical explanation of the cycle itself.

The Sun's variability is a major reason why a given solar observing technique or instrument does not become obsolete after it has been employed on one or two flights; its minimum useful life is 11 years at least and may be as long as 22 years.

It is certainly true, as pointed out in Section 1, that only by means of such systems as AOSO, AES, and MOT can solar physics achieve the high spatial and spectral resolution required for the solution of many fundamental problems. At the same time, there are and will continue to be many problems that can be attacked with instruments of less power and lower cost. Thus, although the goal of the solar physics flight program must be to develop observing facilities with progressively greater range and power, the precious observing time of such large and costly facilities should not be used to make observations that can as easily be obtained with smaller and cheaper equipment.

This same philosophy has long been followed productively in ground-based astronomy, where small telescopes continue to be usefully employed despite the existence of many telescopes in the 100-200 inch class. It does not, however, seem to be followed in present NASA program planning, as set forth in the 1965 Prospectus. For example, it is apparently assumed that once the AOSO spacecraft is available there will no longer be any need for the OSO; and, indeed, in the 1965-1980 Prospectus there is an overlap of but one year, 1969, in which the last OSO and the first AOSO are to be launched. Similarly, the AOSO is to be phased out in about 1975 with the advent of the MOT. Finally, although the sounding rocket program is planned as a continuing program, its support level is expected to remain essentially constant for the indefinite future. On the other hand, there is every reason to expect a substantial increase in the number of sounding-rocket launches that will be required during the next few years, both to test experiments to be flown later in satellites and to attack solar problems that do not require satellites for their solution.

One of the features of space science which has most discouraged scientists from embarking upon space research programs is the very long lead time between the conception of an experiment and the acquisition of results. Unfortunately, the program outlined in the Prospectus would seem to increase rather than decrease this time interval. The rather minor character of the rocket program outlined in the Prospectus, and the termination of OSO launches by 1970, leaves solar physics only with projects requiring lead times of 3-5 years. This will make it exceedingly difficult to attract university experimenters to the program, except as they may advise or consult with industrial companies or government laboratories. The effects of this trend on the training of graduate students could be disastrous. Thus, the present Prospectus in solar physics, if followed, could well be self-defeating.

The NASA space flight program in solar physics must be viewed as one element in a broader national program of research into the physical nature of the Sun. Other major research activities, in which most of the country's solar physicists are now engaged, are ground-based observations, laboratory experiments, and theoretical investigations. If more solar physicists are to be attracted into the NASA program as experimenters, they must be given an opportunity to remain solar physicists and not be converted into instrument developers. If they are expected to give up a major fraction of their current research activities in favor of space solar physics, they must be able to do space solar physics in a reasonably short time scale. It is true that the design and construction of a large solar observatory on the ground may also require about five more years—but with guaranteed results, whereas space experiments are still fraught with the possibility of failure. Eventually, when a large permanent solar observatory has been erected in space, the problems associated with long lead time will be solved, because observational data may then be called forth by radio command. But, in the interim, the solar physics program must continue to provide opportunities for experiments with rockets and small satellites.

In connection with the difficulty of attracting more investigators into the space science program, it might be well to point out one other important consideration. As larger and more complicated experiments become possible, it is apparent that experimenters must work more and more in groups. The time has, in fact, already passed when a single experimenter could conceive of an experiment, design an instrument, and analyze the results. In order to build a stronger base of space scientists in the university community, it is important that NASA make a firm effort to encourage the development of new groups in plasma physics and atomic physics, but with an interest in space science.

SPECIFIC VEHICLES AND INSTRUMENTS

Balloons and Airplanes

The Working Group did not study the use of these vehicles in detail, for there is little new to be added to that already written in several places. We endorse the statements of the Iowa study, noting that since that time, balloons have carried several valuable experiments and the first observations from the X-15 have just been made. In addition, such other aircraft as the current generation of jet transports (some of which have already been used to study solar eclipses) and the proposed supersonic transport offer both the prospect of longer observing time and the certainty of visibility for future eclipse study. Infrared observations of the Sun (in the wavelength region between 20 microns and 1 mm) constitute an important new area of research that can be done effectively and cheaply from balloons or airplanes, and we encourage the endeavor. These vehicles will continue to be of use in solar-space-science work for an indefinite time in the future.

Rockets

The Office of Space Science and Applications Prospectus 1965, listing program opportunities for 1966-1985 (in a Draft dated June 10, 1965), is based on two possible situations: a constant budget (called the Minimal Growth Program) and a 5% per year increase above this. The proposed program in Physics and Astronomy, however, contains a tremendous increase in manned projects, principally the MOT, and an early termination of OSO, phasing out of AOSO and OAO, and a constant level for sounding rockets and space research and technology studies.

Although such a proposed program represents vigorous progress in the development of spacecraft and vehicles, it also contains a danger to which those planning the program must be alert. A large, complicated effort such as an MOT or even the AOSO requires extensive preliminary study and testing, which must include checking of actual components on rocket flights, in order to insure effectiveness of the final flight equipment. Rocket experiments, ground observations, laboratory studies, and OSO experiments will all be necessary to plan adequately and intelligently a spectroheliograph, for example, which can take advantage of AOSO capabilities. Furthermore, even in an era with AOSO or MOT capabilities, scientific questions will continually arise that can be answered with a single rocket flight. Therefore, if we allow the space program to increase in complexity at the expense of eliminating or making ineffective the more straightforward techniques, we may find ourselves unable to plan adequately the complex ventures.

We can see then that a program of increasing complexity and cost can be conducted well either with a steadily increasing budget, or by allowing sufficient time for the complex projects so that they can build upon, and do not cripple, the simpler techniques.

A particular example of a situation in which a serious imbalance of this sort has developed—that of sounding rockets—was discussed by the Working Group. Improvements are greatly needed in the performance and reliability of sounding-rocket vehicles. Not only will rockets continue to be used for direct studies in solar physics; they will also be required for prototype work on OSO, AOSO, AES, and MOT. Rockets will also be needed for the periodic checking and calibration correction of many long-lived orbiting experiments, and also for carrying out particular researches suggested by satellite results.

The Prospectus simply does not include a sufficient budget for rockets, their improvement, new rocket systems, and instrumentation. It is urged that Aerobee and other rockets, suitably improved, be provided in quantity adequate for all needs in solar research that can be met by their use. It is estimated that 25 to 40 Aerobee-150 rockets, many containing a triaxial stabilization system, will be needed each year from now until 1975, although this estimate might be changed by a significant improvement in rocket performance or reliability.

This need for continued development of rockets was clearly recognized at the 1962 Space Science Summer Study and a recommendation was formulated as follows:

"Recommendation. We recommend that the sounding-rocket program continue to receive full support; and that both the inertially guided Aerobee with fine pointing at selected stars, and the inertially guided Aerobee with fine pointing at the Sun controlled by an optical sensor be made available at the earliest possible time."

Thus, the need for triaxially stabilized Aerobees was clearly defined in the Iowa report, and this recommendation has been restated many times since by the NASA Solar Physics Subcommittee. Unfortunately, progress has been slow or nonexistent. Little or no improvement has been made in rocket performance, reliability, or recovery. Failures in the Aerobee-150 and parachute recovery continue to be numerous, and the opportunity to achieve greater altitude with a small redesign has not been seized. Even now, the "solar-pointed" Aerobee, as envisioned in the 1962 report, is not in sight.

In the meantime, the U.K. has constructed and flown successfully three solar-pointed Skylark rockets. This has already placed them ahead of the United States in one branch of solar research. Stop-gap measures have been resorted to in the USA; for example, the combination of an Attitude Control System (ACS) and Biaxial Pointing Control (BPC), flown by the Goddard Space Flight Center, the small triaxial version of the BPC being constructed by the University of Colorado, and the cross-spin stabilization system of Kitt Peak. This shows how great is the need for the triaxial-solar-pointed Aerobee. It is already too late to use it for testing the design of instruments to be flown in the first AOSO, but perhaps not for the second.

In view of these considerations, the Working Group recommended:

Recommendation 1. (a) That the recommendations of the Iowa meeting (1962) concerning fine pointing be given immediate attention and that highest priority be given to the development of a triaxially stabilized rocket attitude controls, leading, as soon as possible, to a fine-pointing system capable of an accuracy of 5 sec of arc and optimally designed for solar use;

(b) That other improvements (such as increased payloads and peak altitudes, increased reliability, and more dependable recovery techniques) be made in existing rocket systems;

(c) That the number of rockets available per year for research in solar astronomy be at least doubled;

(d) That funds for payload development be increased to an adequate level, especially when the triaxial pointing controls become available.

There is also the question of the continuation of the OSO series. The spacecrafts OSO-I and OSO-II performed almost perfectly. Preparation of the major instrumentation was commenced for OSO-II four years before flight, and was done with the greatest possible care. Nevertheless, one major experiment failed completely, another was only a partial success, and two others failed sooner than they should have. The conclusion is clear—much more work must be done in rockets and in the laboratory, prior to freezing designs of experiments for satellites, and the previously described improvements in rockets are greatly needed. A second conclusion is equally clear: OSO should be continued far beyond OSO-H; it is an excellent spacecraft and it bears a relation to AOSO that is similar to that of the Aerobee-150 rocket to OSO. As with rockets, OSO can be improved at a cost that is relatively small, to become a still more useful spacecraft, by increasing its pointing accuracy; by providing ground control of pointing, offset pointing, and fine raster scanning; by placing it in a Sun-synchronous polar retrograde orbit; by providing space for experiments at least five feet long; and by increasing the power, telemetry capability, and flexibility in data format.

Still other possibilities exist for this versatile spacecraft. It might, for example, be placed in an Earth-synchronous orbit so as to provide continuous real-time telemetry, thus making possible enormous data handling capability, as required to make use of television-type image-forming techniques. A stripped-down version of OSO launchable from a Scout, with far less expense and lead time but providing ample room and pointing accuracy for experiments intended mainly for solar monitoring, would be extremely useful for this purpose.

Additional considerations have already been given above under General Remarks. There is a range of experiments, beyond the capabilities of rockets, which OSO can do more effectively and cheaply than more advanced equipment. Monitoring of various solar features through the coming maximum and into the minimum beyond is such a task.

The Working Group therefore recommended:

Recommendation 2. (a) That the presently approved OSO program be augmented by at least four additional launchings during the period 1970-1972 inclusive;

(b) That no decision be made to terminate the OSO program after 1972 without further review at an appropriate time;

(c) That NASA make every effort to implement such desirable improvements in the OSO spacecraft as increased power, offset pointing, localized raster scans, provision for slightly longer instruments, greater data capacity and more flexible data format, and improved pointing accuracy (15-30 sec of arc);

(d) That consideration be given to injection of one or more OSO spacecraft into a polar retrograde orbit in order to provide continuous surveillance of the Sun.

Advanced Orbiting Solar Observatory (AOSO)

None of the recommended improvements in the OSO program, neither the improvements recommended in the spacecraft itself nor the additional flights recommended, in any way diminish the need for AOSO. The spacecraft specifications for AOSO and the significant improvements of these specifications over those for OSO were discussed at Iowa, and reference is made to that report. The crucial improvements in the AOSO spacecraft over the OSO are two: 1) much greater pointing accuracy and stability (5 sec of arc, about an order of magnitude greater than that obtained in OSO) and 2) much larger volume for optical instruments. In addition, AOSO will provide much greater data storage and telemetry capacity.

It is well to point out that AOSO is an advanced, highly specialized spacecraft designed for a particular mission, and it cannot be replaced during this time period (1970-1975) by any other spacecraft yet foreseen. In particular, it does not compete with the proposed ATOM of the Apollo Extension Systems (discussed below) for several reasons. AES is essentially a temporary flight of a duration of approximately one or two months, with only short intermittent solar observing periods, while AOSO provides continuous observations over many months or years. AOSO also possesses the crucial advantage of a pointing capability of 5 sec of arc, which is clearly beyond that planned for the early manned missions.

Since AOSO is the next logical step beyond OSO and is the only spacecraft available to do the jobs for which it was designed, the Working Group made the following recommendations concerning AOSO:

Recommendation 3. (a) That a satellite with AOSO specifications is an indispensable next step in NASA's solar program, and must be flown close to the coming solar maximum;

(b) That the AOSO program be accorded all the priority necessary to maintain the launch schedule shown in the Prospectus.

Apollo and the Astronomical Telescope Orientation Mount (ATOM)

Some of the early Apollo flights (in the 1965-1970 period) include flights in Earth orbit of a few weeks' duration. These flights provide excellent opportunities both for training astronauts in the techniques of astronomy and for making useful astronomical observations. Since the spacecraft is recovered, photographic film, with its great storage capacity, can be used. Photography is particularly valuable for obtaining high temporal resolution of transient events and for obtaining high spatial resolution in wavelength regions where there is adequate flux. In addition, longer exposures than obtainable by rockets can be made.

The simplest experiments can be performed by one of the astronauts, if an air lock is provided as planned. For example, a small extreme ultraviolet (XUV) photographic spectrograph can be introduced into the air lock, pointed at the Sun by the astronauts, and exposed for an hour or more, instead of two minutes, as in a rocket.

The Astronomical Telescope Orientation Mount (ATOM), as proposed by Ball Brothers Research Corporation in an engineering requirement study, is even more promising. This concept would utilize these extended periods in orbit for astronomical observations from a spar which could be erected from the service module of the Apollo spacecraft in flight. An astronaut would then act as an observer to point and guide the equipment.

The Working Group believes that in addition to the valuable solar data that can be obtained, an important aspect of the ATOM project would be the determination in a very practical way of the usefulness of man as a space solar astronomer. This should be accomplished before the completion of the design and construction of the large observing facilities proposed in the Committee's report.

A number of AOSO experiments, suitably modified, are already under study for inclusion in ATOM. An important additional experiment for ATOM would be a 50-cm-aperture photographic telescope for high-resolution cinematography in white light and Hα. Because the exposure times required are very short (about 10^{-3} sec), guiding would not limit the angular resolution, which could exceed that of Stratoscope by a factor of 2. Furthermore, the observing period would be much longer.

Other suggestions of experiments suitable for ATOM are contained in Section 3.

The Working Group made the following recommendations:

Recommendation 4. (a) That manned missions in the 1968-1972 time period, such as ATOM in the Apollo Extension Systems, are desirable to supplement AOSO, but cannot replace it;

(b) That because it offers the prospect of providing answers to critical questions relating to the technology of manned space telescopes and data recovery, the ATOM concept merits vigorous support.

Apollo Extension Systems (AES)

Flights of the Apollo spacecraft beyond the first lunar landing constitute the AES program. The Working Group considered in detail the AES concept, for it is apparent that such flights could provide great opportunities for solar space science in the period 1970-1975 and beyond.

Certain possible solar experiments for AES are contained in the reports of Section 3 and especially in the discussion of the Manned Orbiting Telescope, below.

The Working Group made the following recommendation:

Recommendation 5. That solar space observation be included in the manned space science program of the Apollo Extension Systems in order to develop the technology of manned space astronomical operations. Such observations, which could attain resolving power of 1 sec of arc in the wavelength region 500-3000 Å, mark the next logical step beyond both AOSO and ATOM.

Manned Orbiting Telescope (MOT)

(a) The problem considered first concerns the solar telescopes that should follow the AOSO program. The scientific needs in the 1970's will probably demand high angular resolution in all types of observation. Here we confine ourselves to the achievement of better resolution at wavelengths for which normal-incidence optics are effective, i.e., $\lambda > 500$ Å. The reasonable next step after AOSO should aim at an improvement in resolution at far ultraviolet wavelengths by a factor of about five, which represents an increase by a factor of 25 in the information detail in terms of elements of area examined. A similar relative gain in resolution at visible wavelengths should also be sought. Therefore, we shall discuss the possibility of obtaining resolution of 0.1 sec of arc at $\lambda > 3000$ Å and 1 sec of arc at $\lambda < 3000$ Å, or better. A further goal is the achievement of photometric accuracy of about 1%. The kinds of observations one can expect to make are summarized in Table 1, in Section 3.

We consider first the feasibility of one universal telescope for the whole spectral range $\lambda > 500$ Å. The aperture must be about 1 meter to attain the 0.1 sec of arc resolution in the visible region. Its focal ratio cannot be less than f/12, the maximum focal ratio now available or planned in concave gratings. The requirements dictate a telescope 1 meter in aperture and 12 meters long. The whole package would have to be about 15 meters long, since the XUV spectrographic equipment must be in line to avoid prohibitive extra reflections.

A more promising approach may be the use of two telescopes in a single package. The first would be a 20-cm f/12 XUV reflector of 2.4 meter focal length, with a mirror figured to the highest attainable accuracy. In the 500-1500 Å region, it would have a theoretical resolution of about 0.1-0.3 sec of arc, which exceeds our requirement of 1 sec of arc. The exposure required in a video system of 0.2 quantum efficiency would be in the 1-10 second range. Interchangeable mirrors for the various spectral regions could be provided. The second telescope would be a compound reflector of 1- or 1.5-meter aperture with an effective focal length of about

50-75 meters. A total folded length of about 5 meters would allow room for the in-line XUV spectrographic equipment. Conceivably, however, the whole instrument could be squeezed down to the 4-meter length which would fit into two compartments of the Apollo service module that may be available by 1970.

Manned operation of this telescope has many obvious advantages, including the recovery of photographic records and the kind of maintenance that leads to a long useful life (provided rendezvous can be contemplated), not to mention a considerable simplification of the original design and installation. Comparison between manned and unmanned operation in terms of cost of observational data can be determined only by a careful study.

(b) A white-light coronagraph is another space instrument of great importance. Such a coronagraph, which is conceived of here as externally occulted, would have a camera lens of approximately 10-cm aperture and a length of at least 10 meters. Appropriate auxiliary equipment, such as polarimetry instruments, must also be provided. Details of such a system are contained in the discussion of the wavelength range above 3000 Å, in Section 3.

(c) The telescopes proposed in (a) and (b), above, would fill an important interim need and would provide further valuable experience in the design, installation, and operation of manned solar instrumentation, probably during the AES period. The next step would be the relatively permanent space observatory discussed in Section 3 (under Wavelength Range above 3000 Å). As pointed out there, a 1- to 1.5-meter telescope could clearly surpass the performance of the best ground-based instruments in the visual region of the spectrum, and, if properly adapted, could be used over the entire electromagnetic spectrum above 1500 Å. Unlike the interim version, the system would not be folded and the telescope would be 50-75 meters in length. A 1-1.5 meter aperture is adequate for solar work because of the great intensity of light from the Sun compared to that from stars. We are here proposing not simply an orbiting telescope, but a fully equipped, and manned, orbiting solar laboratory. Such an orbiting solar space station would be at least semipermanent and would be periodically serviced from the ground.

(d) As a companion instrument to the telescope proposed above for wavelengths $\lambda > 1500$ Å, we require a second telescope of similar aperture, specifically designed to operate in the spectral range 500-1500 Å, as an integral part of the manned orbiting solar laboratory. The required spatial resolution is 0.5 sec of arc, but the pointing accuracy need not be better than a few seconds of arc.

Considerations of the above information and of the information contained below under "Man in Space" led the Working Group to make the following recommendations:

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

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

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

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

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

MAN IN SPACE

AOSO will point telescopes up to 3 meters in length at the Sun with a precision of about 5 sec of arc. It is apparent from the discussion of specific vehicles and instruments, above, that solar astronomers need to achieve a resolution of 1 sec of arc, and eventually 0.1 sec of arc, in nearly every mode of observation planned for OSO and AOSO. Not only must telescopes be pointed with that precision, but more significantly, considerably longer telescopes are required, as well as corresponding refinement of component specifications, alignment, etc. Two consequences of this desired high-resolution performance of space telescopes are of special importance to the question of man's potential role. First, the data collection rate of large telescopes is vastly greater than that of small telescopes. Second, those telescopes and their accessories must be specialized as to functions, wavelength of operation, etc., so that versatility is achieved only by major subsystem interchange or adjustment. All of these factors must be considered as part of the question of man's usefulness in performing astronomical observations from a satellite.

So far as pointing and tracking a 1 or 0.1 sec of arc telescope is concerned, the physical presence of man is both a great advantage and a formidable handicap. Motion of an astronaut must be severely limited if he is tightly coupled to the telescope (i.e., not dynamically isolated, as, for example, by an umbilical cord or tether). At 0.1 sec of arc, even his breathing and involuntary muscular activity may be a major problem to the engineer developing an automatic stabilization system. Indeed, this one question has to be studied further before any commitment can be made to manual operation of a large space telescope. On the other side of the balance, an astronaut can make a notable contribution by performing manual guiding control, by monitoring a solar region for unpredictable activity, or by providing real-time assessment of instrument performance and quality of data acquired. To serve this useful purpose, the astronaut can still be essentially decoupled from the telescope, utilizing only a television monitor or some simple remote indicator to permit physical separation. In theory, this communication link could as well be relayed to the ground, so that an astronomer at his telescope could be included in the control loop. In practice, such wide-bandwidth, real-time, continuous channel operation requires a prohibitively costly and perilously unreliable ground communications network or relay satellite. With an astronaut present, such a control communications link can be provided through an umbilical cord, or in some instances by direct viewing.

As to the size of a telescope that can be carried into orbit, apertures are limited by diameters of launch vehicles, and over-all length by the accommodations of rocket nose cones. Resolving powers of a million for spectrographs and 1.0 or 0.1 sec of arc for telescopes require apertures of a few meters, which can be accommodated by modest vehicles like the Thor or Atlas. However, solar telescopes must operate at small aperture ratios of the order f/25 to f/100, for such reasons as thermal loading, diffraction-grating aperture, and the constraints of linear resolution of photographic films and image tubes. Therefore, solar telescopes for 1 to 0.1 sec of arc resolution would range up to several tens of meters in length. Such extended structures very likely will require assembly in orbit, after being launched in a stowed condition. For example, an enclosing tube, constructed in sections, might be literally telescoped and later extended in orbit. A mirror or grating probably should be packed in a special support to protect it from the shock and vibration of launch, with provision to install and adjust it as part of the assembly procedure. In conjecturing on such operations, the possibility of either fully automatic or remotely controlled operation must be considered. However, the astronaut himself is obviously well suited to perform such complex one-time operations in which very high reliability is mandatory, particularly in view of his ability to cope with unforeseen problems like fouling lines, sticking components, etc. Doubtlessly, his greatest asset is his ability to monitor and control, as in focusing and squaring on an objective, choosing a wavelength in a spectrum, etc.

Thus, it is likely that a man must be counted on to erect large solar telescopes and spectrographs in orbit. Beyond this initial operation of installing and commissioning a large instrument, a man must be relied upon to maintain and repair it. Large telescopes will be exceedingly costly to create, so that a useful lifetime of many years must be planned to fully amortize the investment. The finite lifetime of components (storage batteries and transistors, motor bearings, optical surfaces in the meteoroid environment, etc.) as well as the exhaustion of expendables (reaction motor fuel, for example) also demand periodic servicing of major orbiting observatories. For this task the versatility and flexibility of a man compared to a robot or automatic equipment will be critical in defining the feasibility of large space telescopes.

In the operation of large telescopes, man has several potential functions. First, he can perform major configuration changes—for example, converting a spectrograph into a spectroheliograph, altering the wavelength setting of a heliograph monochromator, interchanging gratings of different rulings, etc. During individual observational projects, man provides the ability to perform instantaneous analysis of the output data in order to modify the subsequent observations. An example would be to monitor an active region, and to start a series of high-rate spectral or cine observations at the inception of a flare. Automatic equipment to do the job accurately and reliably probably cannot compete with human judgment, since the complex activities of time correlation, field search and event localization, and very sensitive threshold judgments must be made simultaneously, and both accurately and quickly, under very poor conditions of low signal-to-noise ratio.

In connection with data collection, the 20-million-bit-per-orbit capacity of AOSO is recognized as a severe limitation on the performance of certain classes of observation in that spacecraft. Monochromatic imaging, coronal mapping, and spectral scanning in the normal-incidence region all permit data collection rates, even at 5 sec of arc angular resolution, in excess of the tape recorder capacity of AOSO. As a release from this constraint, photographic data recording appears highly desirable. Film permits parallel-channel recording over the equivalent of 10^4 to 10^6 channels simultaneously, and total data capacities of roughly 5 x 10^9 bits per 100-ft roll of 35-mm film. (For example, AOSO's tape recorders store 2 x 10^7 bits per orbit, with recording of the equivalent of only 10^2 - 103 parallel channels.) Recovery of photographic data by return of the astronaut thus appears attractive as one way of breaking through the data barrier. Alternative methods must be considered, however. One is on-board data processing—for example, transmission of a scene and then, for several frames, only time changes in the scene. Another suggestion is the return of a film package by a re-entry module from an unmanned satellite. For AOSO, it appears that a week's data could be recovered in this way from each mission, with about a 50% chance of recovery. This is clearly a very limited return payload. Other alternatives to film recovery with an astronaut are real-time video-bandwidth telemetry by relay to a high altitude communications relay satellite and use of an Earth-synchronous orbit.

It is clear that men can perform many useful functions in connection with the assembly and operation of large solar instruments in space. Perhaps other advantages of manned operation will appear as man gains experience in space work.

GROUND-BASED SOLAR ASTRONOMY

The study of the Sun is carried on in a variety of ways, and this variety will be increased and not diminished by the advent of sophisticated space techniques. The great range of wavelengths emitted by the solar atmosphere, the changes with time, the structural detail that can be observed, and the tremendous range of brightness encountered all force the solar astronomer to consider many techniques when attempting to solve a problem. Even in studying a particular problem at a particular time several complementary methods are frequently employed. For example, at the present moment solar flares are under observation in visible and radio wavelengths from the ground, in x-ray, ultraviolet and very long radio wavelengths from space, and occasionally in x rays from balloonborne equipment. As a different example, the electron corona of the Sun has recently been observed in the same wavelength but at a variety of distances from the edge of the Sun by photoelectric techniques from the ground, by a balloonborne coronagraph, and by a coronagraph aboard a satellite. These measurements overlap, but more importantly they complement each other to allow more rapid progress in understanding the Sun. And, to repeat, this situation will certainly continue.

Measurement of emissions from the Sun by ground-based equipment will continue for years to come to provide exciting progress in understanding the Sun. In addition, these measurements will be essential in planning space experiments and in providing vital information for the interpretation of the space measurements. The complementary roles of ground-based and space solar astronomy have already been emphasized earlier in the report. Therefore, it is the strong opinion of this Working Group that ground-based solar studies must be vigorously pursued, and that more extensive and elaborate solar ground-based astronomical facilities are required than those described in the recent study "Ground-Based Astronomy—A Ten Year Program," (National Academy of Sciences—National Research Council, 1964) commonly known as the Whitford Report.

Solar telescopes differ in an important way from the nighttime instruments. Most solar work has been done not on large universal instruments, but on more specialized telescopes designed for narrow ranges of problems. This point is illustrated by the fact that although the cost of the primary collecting surface (lens or mirror) for a nighttime instrument may represent up to one-third of the total investment, for solar instruments this optical element is a minor fraction of the total cost. Much more of the solar instrument consists of secondary optics, photoelectric guiders, filters, and other auxiliary equipment.

Because of this difference, a report on ground-based solar astronomy over the next ten years would not exactly parallel the Academy report, which lists the large universal instruments to be built. However, it seems very important that the genuine requirements of ground-based solar studies be described and the kinds and types of specialized facilities needed be outlined so that users of the existing Academy study may have a balanced picture of the needs of astronomy. This panel urges the Academy to arrange for an early formal report on the needs of solar astronomy over the next ten years.

The Working Group also discussed sources of support for solar observations from the ground. Like research in other fields, basic research in solar astronomy enjoys a variety of sources of support in this country today. This diversity is healthy and fully in keeping with the attitudes of the society in which we live. Now NASA has a special interest in ground-based work since the planning, support, and interpretation of the flight experiments in various spacecraft are tied closely to observations made on the ground. It is therefore reasonable to expect NASA to be one of the sources of support of a growing program of solar astronomy from the ground. The Working Group therefore recommends:

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

LABORATORY RESEARCH

Active research in many fields of physics is essential to the program of solar physics research from space vehicles, to astronomical research in general, and to the entire space program. In spite of the fact that this is extremely obvious, it is sometimes overlooked. Actually a breakthrough in the laboratory may open new possibilities for solar physics that could not be realized, even with the most sophisticated and costly vehicles. An example is the 80% reflecting coating, $Al + MgF_2$, which now makes possible the use of complicated optical systems at $\lambda > 1100$ Å. This may be contrasted with the region $\lambda < 500$ Å, in which normal incidence reflectances are still very low, and only simple instruments can be used. Looked at another way, an increase in efficiency of an instrument by a factor of 10 may mean obtaining the same information in one-tenth the time in space or obtaining ten times more information during a particular flight. Indeed, at shorter wavelengths it is not now possible to study with sufficient time resolution the distinct development stages of a flare, either with spectrographs or with monochromatic cameras. Until more efficient optics are developed, the long exposure times required with such instruments preclude these critical observations.

In the following paragraphs the present status and needs for laboratory research directly connected with observational aspects of the solar physics program are summarized. Generally, the money that can be spent here is at least an order of magnitude less than that spent in vehicle development.

Diffraction Gratings for the Extreme Ultraviolet (XUV)

Research to improve diffraction gratings is being conducted by Bausch & Lomb, with NASA funds, and directed by NRL. Progress has been made in the use of the electron microscope technique to observe the form of grooves and the surface roughness within the groove, both too fine to see optically. It is expected that gratings with higher efficiency will become routine, rather than the exception. It is also believed that gratings ruled with as many as 4800 lines per mm will become possible; this will expand the potential of the normal-incidence XUV spectroheliograph. At the University of Michigan, a radically new type of grating ruling engine is under development, with good expectations of making possible the ruling of gratings of greater precision and up to 30-inch size. Such large gratings will be required to achieve resolutions of the order of 10^6 in future large satellite spectrographs.

Effects Produced by the Space Environment

The degradation of optical and photoelectric components in the high vacuum of space, produced by evaporation effects, by corpuscular and hard electromagnetic radiations, and by the impact of the residual atmosphere, merits much study on a systematic basis. For example, it has not yet been shown that replica diffraction gratings are satisfactory for use in orbiting spacecraft; therefore, use of original gratings is mandatory. Because the production of original gratings is severely limited in quantity, it is important to develop replicas that are safe for use in orbiting space vehicles. The production of several originals for OAO, for example, delayed grating production for many other purposes for months. Replicas offer also the great advantage of providing several gratings of exactly the same characteristics. Replicas should be perfected such that they can be used in all cases instead of originals. Long-delayed testing of replica gratings has recently been undertaken at Bausch & Lomb, and at the University of Colorado, to determine their stability in a simulated space environment.

Thin-Film Metallic Filters for the XUV

The thin A ℓ filter has made possible the simple XUV spectroheliograph for the range 170-800 Å, and has also made grazing-incidence spectrographs useful for recording the XUV solar spectrum by photography. Other such filters, passing different wavelength regions, are greatly needed, but difficult techniques are required for producing them. Beryllium, for example, transmitting at $\lambda > 110$ Å, would be extremely useful. But much research is needed to produce this film unsupported by a backing material. Elimination of pinholes is another problem, often of extreme difficulty. At present, some research is being conducted at NRL, at the University of Colorado, at the University of Southern California, and at the Northrop Space Laboratories.

Photographic Emulsions

To obtain photographic emulsions of high sensitivity in the extreme ultraviolet it is necessary to make use of the delicate Schumann-type emulsions. There is no other demand for this material; hence their production and improvement is entirely a prestige endeavor by the manufacturers. The value of Schumann emulsions for solar physics, on the other hand, is too great to measure; experiments and vehicles depending on this single component have already cost millions of dollars. Eastman Kodak first made available its SWR emulsion for rocket work some 20 years ago; their efforts in the next 10 years succeeded in largely freeing it from blemishes. Meanwhile, Roger Audran, of Kodak-Pathé, in France, made a real breakthrough, producing a Schumann-type emulsion 10 times more sensitive than SWR. This has made possible most of the advances in photographic solar XUV spectroscopy since about 1959. Gradually, Kodak-Pathé has improved the material by devoting a team of two or three very talented men to both research and production. This type of research should be supported and encouraged on an increased scale. There are many problems waiting to be worked on, in order to make this emulsion of even greater usefulness. It is essential to learn how to process it so as to make it as useful for intensity measurements as the more usual photographic emulsions. The proposed photographic instrumentation for major manned projects will very soon require specialized developments relating to photographic technology. Examples are cassettes to handle Schumann emulsions, protection of sensitive materials from space radiation, etc.

Photomultipliers

Photomultipliers constitute one of the critical elements in all orbiting solar physics experiments. It would be impossible to devote too much effort toward their improvement. There are many directions for research: improved quantum efficiency as a function of wavelength; fatigue effects; use in the photon-counting mode; polarization properties; noise and dark current suppression.

For XUV use, open photomultipliers of various types are required. For example, the Channel Photomultiplier of Bendix was used in the OSO-II spectroheliograph, but its performance in space has shown that much more needs to be learned about its characteristics. Arrays of these tiny photomultipliers offer great promise for image forming.

Video Systems

In lieu of photographic techniques, certain observations require video detection and wideband data systems. Improved video cameras sensitive to the UV, XUV, and x-ray regions of the spectrum and with long-time storage of images without deterioration should be developed. Improved image intensifiers, with better resolution and stable high gains, are needed. Along with these improvements, data compressive techniques should be investigated.

Research in Laboratory Spectroscopy

The XUV spectrum of the Sun has shown the existence of a great gap in our knowledge of the spectra of highly ionized atoms. For example, most of the observed solar lines from 171 to 500 Å have defied identification. A promising start on identification has been made in the United Kingdom, using the high-temperature plasmas (zeta- and theta-pinch) at Harwell and Culham, by introducing materials to study their spectra under conditions similar to those in the Sun's corona. Limited work is also going on at Los Alamos, NRL, and the High Altitude Observatory, and, with hot spark sources, in Sweden, Israel, and the USSR. It is important to assure the vigorous continuation of this work, persuading more spectroscopists to enter the field and using computer methods of analysis, until the energy level systems of the common solar elements are completely known in all stages of ionization encountered in the corona.

Another example of the important part played by laboratory spectroscopy is the study of molecular spectra under solar conditions, as conducted in the shock-tube laboratory of the Harvard College Observatory. Band heads in the XUV absorption spectrum of CO heated in shock tubes were found to match features in the solar spectrum from 2000 to 1500 Å, as recorded with rockets. This result established that CO is a strong contributor to the absorption in the critical region of the solar atmosphere between the photosphere and the chromosphere, where shock-wave mechanisms set in and transfer energy to produce the high temperatures in the corona.

There are many other topics in spectroscopy that deserve more attention; among them are: the rare-earth spectra, which appear in certain stars, and also with strangely great intensity just above the Sun's limb; transition probabilities; transitions of the autoionization type under conditions prevailing in the Sun; and live emission resulting from dielectronic recombination.

Technical Problems Associated With Space

Some major problems here are: lubrication of moving parts, deterioration of optical surfaces by evaporation or micrometeorite impact, radiation damage to optical and electronic components, fogging of photographic film, and electrical problems peculiar to the space environment, as encountered in OSO-II.

The solution of difficulties known to exist in certain space experiments is essential before the design of similar experiments for orbiting vehicles is frozen.

Miscellaneous

Ground equipment used in support of space experiments must not be neglected. For example, improvements are possible and are greatly needed in filters that permit monochromatic solar monitoring. Lyot birefringent filters are costly and fragile, but have proved to be powerful tools in the visible spectrum. Much progress has been made in Fabry-Perot type filters, so that a 2 Å bandpass filter is now available. Even narrower filters of this type are desirable, and merit aggressive development.

The infrared is a portion of the solar spectrum in which much remains to be learned. Rockets and satellites are not always required, since balloon altitudes are sufficient for many purposes. Detection problems on the other hand are very difficult. It is urged that breakthroughs in infrared technology be made available for use in solar-physics research at the earliest possible time.

Recommendations

In the light of the foregoing discussion, the Working Group makes the following recommendation:

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

3. COMPLETE SUBCOMMITTEE REPORTS

WAVELENGTH REGION BELOW 500 Å

The extreme ultraviolet and x-ray spectra ($\lambda < 500$ Å) are a particularly important part of solar radiation for observing and studying:

(1) Solar activity such as flares and other transient events.
(2) Active regions of the chromosphere and corona.
(3) The quiet corona against the disk.

In the extreme ultraviolet and x-ray spectral region, additional observations are required to learn more about the spectral lines present, and their identifications and intensities. Along with spectral data, spectroheliograms with low spatial resolution are needed not only to learn more about active regions and transient solar events but to gain information that will make it possible to optimize the design of future experiments. These requirements are natural tasks for rocket experiments and future OSO's.

As in other spectral regions, future requirements in wavelengths < 500 Å are for high spatial resolution. Two important steps appear in future resolution improvements. With the Advanced Orbiting Solar Observatory (AOSO), a resolution of 5 sec of arc will be attainable, which will permit resolution of the chromospheric network structure, general features of active regions, and flares. For the post-AOSO period, there is a need for higher resolution (1 sec of arc or better) to permit observations and study of individual spicules, granules, elements of the network structure, prominences, flare nuclei and microstructure of the corona. These short-lived features require high time resolution, which can be met only with TV systems having large data storage and wideband communication links or else by the use of photography with recovery of film from orbit.

Although the spatial resolution would be less than that of AOSO, for some observations the ATOM on Apollo could be used with photography to obtain high time resolution. In particular, broadband x-ray images of solar activity and coronagraph observations could be obtained with the ATOM.

The requirement for high spatial and time resolution goes beyond the capacity of either AOSO or ATOM and indicates the need for more advanced systems associated either with AES or with large observatories using man for servicing and film recovery. Telescope requirements also go beyond any now planned for AOSO, and consideration of possible designs point out the need for research and development in several areas. The instruments proposed for post-AOSO observations in the XUV and x-ray region of the spectrum consist of either normal or grazing-incidence primary optics coupled with normal or grazing-incidence grating spectrometers or crystal spectrometers. In the spectral region $500 > \lambda > 170$ Å, either a normal-incidence primary or a grazing-incidence primary with a grazing-incidence grating spectrometer have approximately the same over-all efficiency, the lower effective area of the grazing-incidence primary being balanced by the higher reflectivity. Neither of these systems produces stigmatic monochromatic images so that photoelectric detection in conjunction with a raster scan is required to build up images. This mode of operation limits the time resolution that can be obtained. To illustrate, the reflection efficiency for a normal-incidence primary is approximately 0.01 in this wavelength region, the grating efficiency in grazing incidence is about 0.10, and the detector efficiency using SrF as a cathode is 0.20, resulting in an over-all efficiency of 2×10^{-4} counts/sec per photon/cm^2/sec incident on the primary optics. For the Fe 170 Å line, the flux measured by OSO-I for the quiet Sun was approximately 2×10^8 photons/cm^2/sec. With a 1-meter primary, 65 counts/sec would be recorded on the average from 1 square sec on the Sun's disk. When about 10% of the Sun's disk is occupied by active regions, one can expect, according to OSO-I data, a 50% increase in the 170 Å flux. The 1-meter instrument in this case would record approximately 375 counts/sec for 1 square sec of the active region. These counting rates are somewhat typical of what will be encountered in the XUV and x-ray regions with state of the art systems. It is important that instrumentation be improved, and, in particular, that optical systems for forming stigmatic, monochromatic images be developed in order that high time resolution may be realized by photography.

The possibility of recording high-resolution spectroheliograms on film with the use of normal-incidence concave gratings should be explored. The NRL results from rocket measurements might be improved by large concave gratings with greater dispersion (> 4000 lines/mm) and better pointing. Broadband imaging for AOSO, however, is already available. As an example, with a 25-cm-diameter normal-incidence primary mirror and a filter transmitting the band 170-240 Å, one can possibly photograph the Sun in 1 sec with a flux of approximately 100 photons per sec of arc squared. At 44 to 60 Å, the situation with grazing-incidence optics is similar. Important observations of flare activity would also be possible with photographic techniques, larger primary optics and improved narrow-band filters.

Suggested observations in the XUV may be outlined as follows:

Flare Activity. Observations with the aim of providing a N_e, T_e model of flares, surges, prominences, etc.

a. Spectral lines. Lines of high ionization potential important with lower ionization stages needed to complete the structure. X-ray lines and continua should be emphasized. Time-history of spectrum at wavelengths shorter than 20 Å during active events is important.

b. Resolution. High spatial resolution with medium time resolution ($\leq$ 1 min) for studies of the history of chromosphere and corona at time of the flare (with spectral resolution $\lambda/\Delta\lambda$ 10^2). Lower spatial resolution with high time resolution ($\Delta t \leq 10$ sec) for detection and analysis of rapid events at flare time.

The subcommittee suggested that for bursts of hard x rays observations with high time resolution (< 1 sec) at $\lambda < 0.5$ Å might be combined with observations having high space resolution at $\lambda \sim 10$ Å, $\Delta\lambda \sim 5$ Å. The real need, of course, is for observations in x radiation (1-40 Å) with both high spatial and temporal resolution. It was also suggested that the possibility be studied of observing the polarization of x radiation, by use of grazing-incidence polarization.

Active Centers. Observations are needed upon which to base a model of the distribution of N_e and T_e in the active chromosphere and corona and to study variations of the model with time. This includes identification and analyses of small, high-temperature regions ("hot spots"). Time resolution should be adequate to study the development of active centers.

a. The most important spectral lines for this study are the Fe coronal lines in the interval 170-360 Å, which permit study of the variation in the degree of ionization from Fe IX to XVI. The chromospheric lines of O I-O V, which show the variation in chromospheric excitation, and the coronal lines of O VI, O VII, and O VIII are also extremely important. Because of its high ionization potential, the C VI line at 33 Å is also useful for identifying very hot regions. Spectral resolution of 10^3 and spatial resolution of 10 sec of arc are needed. Time resolution of minutes to hours is sufficient for the slow variations.

b. It is possible that the highest possible resolution will reveal slowly varying hot spots of scale < 1 sec of arc. Once that resolution is available, only stable pointing and longer integration times will be needed. Very high spectral resolution is desirable to resolve the profiles of a few lines, such as C VI at 33 Å.

c. For studies of active regions, only relative intensities are necessary, since our chief concern is with the changes in relative ionization.

d. It is especially important to study active regions in the poorly known interval $1 < \lambda < 20$ Å with $\Delta\lambda = 0.01$ Å, angular resolution of 5 sec of arc or better, and $\Delta t \sim 1$-150 sec. The energetic radiation in this region is particularly sensitive to very high temperatures. In ground-based studies, the maximum observable ionization potential is that of Ca XV (814 volts). Hence, we cannot say whether regions with temperature greater than 4,000,000°K exist.

Quiet Corona. Spectral and spectroheliographic studies of the same lines mentioned above under Active Centers will help to reveal the nature of the faint corona outside the active regions. It is of particular interest to connect the quiet corona structure with the chromospheric network and the spicule bushes, to determine the nature of the chromosphere-corona interface.

a. Important spectral lines for this study are the Fe series mentioned above, also Ne VII (465 Å) and Ne VIII (780 Å); Mg IX (368 Å) and Mg X (610 Å); and Si X (254 Å, 272 Å); Si XI (303 Å); Si XII (499, 521 Å). Comparisons of the intensities of the resonance lines with those of subordinate lines in the 100 Å region provide a good measure of the electron temperature. Departure of the resonance doublets of the Ne VIII-Mg X-Si XII sequence from the 2:1 ratio is a sensitive measure of optical depth.

b. Spectral resolution of about 10^3 is all that is needed, except for strongly blended lines. Resolution of 5 sec of arc will determine the gross correlation of the network structure with that of the corona, but 1-2 sec of arc is necessary to reveal the details of the interface.

Quiet Chromosphere. Most of the important lines are at longer wavelengths, but attention should be called to the lines He II 304 Å and 256 Å. We need spectral observations of the over-all and detailed distribution of these lines to determine the height variation of temperature and density of the chromosphere as well as the general structure. Low-noise observations are necessary to search for a possible uniform hot chromosphere over the center of the network cells in addition to the hot elements at the edges. Reasonable time resolution may be obtained by restricting observations to selected parts of the disk.

a. Spectral lines of interest include the He II 304 Å and 256 Å lines, the He II continuum, the C and O chromospheric ions, and isoelectronic sequences such as C II-Ne III-O IV. Spectral resolution of 10^3 or better is needed.

b. Spatial resolution of 5 sec of arc near the limb, with less near the center of the disk, will resolve the network; 1 sec of arc over an area of 30 square sec will permit study of the detailed dynamic structure.

c. Time resolution should be less than the chromospheric oscillation period (300 sec for 10 sec of arc resolution), but high spatial resolution (1 sec of arc) will also require higher time resolution (10 sec) because smaller chromospheric structures vary more rapidly than large ones.

The survey of the solar spectrum in the extreme ultraviolet is still far from complete. Only a small fraction of the lines has been identified, mostly because of the low resolving power of the spectrum. It is extremely important that a resolution of $(\lambda/\Delta\lambda) = 10^4$ be obtained in this region so that line identification may proceed. Large rocketborne spectrographs appear ideal for this purpose. At the same time, laboratory investigations must be encouraged. The value of this is shown by the recent success of the group at the Culham Laboratory of the UK, in identifying some of the lines in the 170-200 Å range.

There also is a large gap in the calibration of intensities in the extreme ultraviolet. Present intensity calibrations are reliable only as far as 304 Å, and both identifications and determinations of relative intensities of different lines are dependent on some sort of calibration; the same can be said of more-accurate wavelength standards in this region.

The need for laboratory research in instrument development should be reiterated. In particular, as is mentioned above, instruments with large aperture that produce stigmatic-monochromatic images of the Sun are not yet within the state of the art. Such an instrument is needed to make observations with high spatial resolution and high time resolution. For both the AOSO and post-AOSO eras, better reflecting surfaces and detectors are required. Improvements in these areas translate directly imto improved time resolution and in certain cases into improved spatial resolution. Improved narrow-band filters will increase the usefulness of instruments like the AOSO x-ray telescope.

WAVELENGTH RANGE 500-1500 Å

No Fraunhofer lines have been observed below 1525 Å; radiation observed at shorter wavelengths originates in the Sun's atmosphere above the temperature minimum. Thus, in this section, we are concerned with the chromosphere, the chromosphere-corona interface, and the corona, as well as with the structures that occur within these regions, such as plages, faculae, flares, coronal condensations, and prominences. We have set 500 Å as the lower wavelength limit only because below this, normal-incidence spectrographs become less useful than grazing incidence instruments—but otherwise the limit is rather arbitrary.

In answering the questions of solar physics, as listed in Section 1 under Current Solar Problems, there are several reasons why this wavelength range is of fundamental importance:

a. It contains the resonance lines of the most abundant elements and of the coronal ions. Much of our knowledge of physical conditions in the Sun's outer atmosphere must come from the interpretation of observed emissions that arise in regions that depart severely from thermodynamic equilibrium. This interpretation must then be made in the light of a sound theory of ionization, excitation, and line formation. The present vigorous attempt to develop such a theory will stagnate unless comparison can be made with observation. The most sensitive and clearcut tests of such a theory can be made in the XUV resonance lines. For this purpose, we require spectra with high resolution over the entire range.

b. We have at present no satisfactory understanding of energy balance in coronal and chromospheric structures nor of the mechanisms that heat the corona and drive the solar wind. Quantitative theories of energy supply to the corona, chromosphere, and flares must be compared with observed energy losses. Since most of the energy loss is by radiation in the resonance lines of the abundant ions, we need accurate absolute spectrophotometric observations in the XUV.

c. The fine structure of the photosphere and chromosphere can be photographed in the visible part of the spectrum with high resolution against the Sun's disk, but no such picture of coronal structure can be gotten from the Earth's surface. Under the very best observing conditions, some coronal structure can be observed at the limb with low contrast in the light of the forbidden coronal lines, but at the limb, simultaneous observation of the underlying chromosphere is not possible. Radio observations in the decimetric range show coronal condensations against the disk, but with very low resolution. However, permitted XUV lines of the coronal ions are bright against the disk and it should be possible to obtain high-resolution spectroheliograms of the inner corona in the light of these lines. Such observations could reveal the form and development of coronal condensations over centers of activity and would show how the corona differed above chromospheric structures such as spicules and the network.

d. We have at present no quantitative measures of magnetic fields in the corona, but, given sufficient spectral resolution and sensitivity, it should be possible to measure the Zeeman splitting of the permitted coronal lines. These observations require four basic types of instruments. These are: spectrographs (or spectrometers), which record the spectrum from some given region on the Sun; spectroheliographs (or monochromatic filters) to produce a picture of the Sun in the light of some specific radiation; monitors, which record a specific radiation from some given place on the Sun as a function of time; and magnetometers, which measure the Zeeman splitting of spectral lines. We consider these below, in order.

Spectrographic Observations

Spectra have been obtained over this entire wavelength range from rockets with spectral resolution of about 10,000. This resolution is sufficient for spectral surveys and also for line profile measurements of the strongest chromospheric lines such as Lyman-α. The next task is to obtain such surveys of the spectra of specific regions such as plages, coronal condensations, areas of the undisturbed disk, prominences, and flares. The photometric accuracy should be no less than 10% and the guiding accuracy 0.5 to 1 min of arc. Such observations could be made from rockets with a three-axis pointing control, or from an OSO-type spacecraft with command offset pointing.

With the higher pointing and guiding accuracy of the AOSO spacecraft (about 5 sec of arc) the same surveys should be made in smaller structures. Less averaging over various structures would occur and thus make higher photometric accuracy (say 3%) desirable. Line-profile measurements of emission lines from both chromospheric and coronal structures are of great importance. For coronal and prominence lines and for the weaker chromospheric lines, resolving powers of at least 100,000 are required. Profiles of all observable lines of an isoelectronic sequence (such as the lithium sequence) or of all ions of a given element (such as oxygen, for example) would be invaluable. With 5 sec of arc guiding accuracy, observations of the hydrogen and helium lines made from center to limb would become feasible. To make these observations in a reasonable time, a telescope of about 40-cm aperture would be required.

The interpretation of spectra is always made difficult by the fact that we observe the integrated emission over the line of sight rather than the intrinsic emission from a uniform element of the atmosphere. This is unavoidable, but if we could observe the variation of brightness across the smallest features in prominences, flares, or the corona, it would be possible to infer the intrinsic emission as a function of position and so deduce the actual conditions in the emitting regions. The resolution required for such measurements would be 0.3 to 0.5 sec of arc. This angular resolution would require a telescope of about 1-meter aperture if photometric observations of 1% accuracy are to be obtained in a reasonable time. Furthermore, the guiding accuracy required is beyond the capability of any presently planned vehicle.

Spectroheliographic Observations

The spectroheliograph enables one to look at the Sun (or a portion of the Sun) in a very narrow band of wavelengths. When proper filters are available, the effect of a spectroheliograph can be obtained simply by imaging the Sun through a narrow-band filter and observing the structure and time changes of the Sun as seen in the light of the chosen wavelengths. More versatile instruments can be constructed, however, around a spectrograph in which the desired wavelength band can be adjusted for the particular problem at hand.

Preliminary spectroheliograms have been made from rockets using a slitless spectrograph and depending upon the brightest emission lines to produce separate solar images in the focal plane. These photographs have shown that there is large-scale structure on the Sun in these wavelengths and have indicated the approximate light levels one has to deal with. Much better images are within the capabilities of the OSO spacecraft having a raster scan mode, and pictures with about 1 min of arc resolution can be obtained in a few minutes of time. Such pictures, if made in several selected lines (in particular, Lyman-α and the coronal lines of Mg X and

Si XII) should reveal the chromospheric and coronal response to the slow changes in active regions and should, in addition, give an impression of the center-to-limb variation in each of the selected lines.

Precise planning of an AOSO spectroheliograph should await the OSO results, but some general statements can be made now. From observations made in visible wavelengths, we know that chromospheric and coronal structures have detail smaller than 1 min of arc in size, so that the increased angular resolution available with the AOSO will show new aspects of the structure and behavior of the solar atmosphere. The AOSO spectroheliograms should show detail down to approximately 5 sec of arc and thus be similar to many of the flare-patrol pictures made in Hα. The great value of the AOSO picture over the ground-based observations, however, will be in the fact that they can be made in light arising high in the atmosphere, so that it should be possible to follow chromospheric structure into the corona.

Beyond the AOSO, plans are necessarily more tentative. We expect to find additional important fine structure in the solar atmosphere down to a fraction of 1 sec of arc, and knowledge of this structure is fundamental to understanding the processes governing the chromosphere and corona. Present thinking places the position of the initial energy release of a solar flare high in the corona, and our opportunity to observe whether this is indeed so will come with a spacecraft of the AOSO class or beyond. The instrument to observe the early stage of a flare must combine high time rate, rapid data handling, and sizable aperture. This can be seen to be a difficult assignment. At 700 Å, a useful emission line may yield a flux near the Earth of 250 photons/cm^2/sec from an area on the Sun of 0.25 sec of arc squared. If we must scan in only one dimension with a spectroheliograph and must cover an area 2 min of arc wide on the Sun, we will have 350 elements, or slit positions. Changes in active regions occur in less than a minute, so, setting our repetition rate at one picture every 10 seconds, we find that to reach an accuracy of a few percent we must have a telescope aperture of something more than a meter. Line profile work with a spectral resolution of 10^5 and 0.5 sec of arc spatial resolution places similar requirements on the space telescope.

Our goal in the 500-1500 A region, then, is a spacecraft with a pointing stability of 0.5 sec of arc or better, a pointing accuracy of a few sec of arc, and capable of carrying a telescope with an aperture of at least 1 meter. We will need both spectral and spectroheliographic observations, so that some versatility must be built into the instrument.

Radiation Monitors

In addition to spectrographic and spectroheliographic observations, detectors monitoring specific radiations are of great value in this spectral range. These detectors are needed, first of all, to monitor the radiation from the entire disk, especially for aeronomy. This should be done in Lyman-α and in broad wavelength regions throughout the entire wavelength range with an accuracy of 1%. But, more importantly, for solar physics one should monitor carefully chosen emissions from specific regions on the Sun as a function of time. Such observations would yield the rate and rate-of-change of energy loss from transient phenomena such as flares and sporadic condensations. These observations can of course be made with the same scanning spectrometer used for the spectrum survey by simply arranging to stop the scan at any wavelength or at certain selected wavelengths.

Magnetometers

The development of a magnetometer for use in this region of the spectrum will probably be difficult, but it would provide a direct way of measuring magnetic fields in the corona. The difficulties are low photon counts, small Zeeman splitting compared to the visible, and lack of suitable polarization analyzers. Such an instrument will probably have to await technical advances and larger spacecraft.

WAVELENGTH RANGE 1500-3000 Å

The wavelength region 3000-1500 Å divides itself naturally into two parts: 3000-2085 Å, which is the extension of the near ultraviolet and visible and is mainly photospheric; 2085-1500 Å (or 1200 Å), where the spectrum is completely different in character and comes, for the most part, from the transition layer and low chromosphere.

Spectral Mapping

One important task is spectral mapping with intensity precision and line detectability exceeding that obtained by the NRL echelle spectrograph, from 3000-2085 Å. These latter spectra become faint at the short wavelength end, and stray light, for which no satisfactory correction can be made, fills the cores of all the Fraunhofer lines. Lines fainter than -1 on the Rowland scale are scarce, and -3 lines are nearly completely absent. This task requires a conventional spectrograph using a concave grating at normal incidence, with about the resolving power provided by a 21-ft grating. Predispersion is required to produce a spectrum that is free from stray light. The goal is $\lambda/\Delta\lambda \sim 10^5$ or 2×10^5 and $\Delta I/I \sim 1\%$ over a spectral span of 10 to 100 Å, and scattered light $<1\%$ of the continuum intensity. Spectral mapping would be carried out for a region restricted to a diameter of less than 10 min of arc near the center of the Sun. This mapping must be done when the Sun is relatively quiet in order to obtain a definitive atlas for reasonably standard solar conditions. The same instrument would be used to continue the solar map to shorter wavelengths, at least to 1500 Å, and probably to about 1000 Å, so as to provide the increased resolution over the present value, 10^4, necessary to separate blends.

A satisfactory instrument of this type should be possible within a 5-ft length but this may require a sophisticated design. One min of arc pointing accuracy in yaw and pitch would suffice. Roll control is not needed, but is desirable when many active regions are present. Because of the size of this instrument, existing biaxial pointing control units are inadequate. A stabilized Aerobee-150 would be satisfactory, but the ACS system will not provide sufficiently accurate guidance. The United Kingdom (Elliott Brothers) control adapted for an Aerobee appears to be the quickest way to produce this vehicle.

Some of the results to be expected from this spectral mapping would be: separation of Fraunhofer from emission lines; detection of chemical elements not yet discovered on the Sun and the determination of their abundances; observation of molecular lines and thus the study of dissociation-ionization equilibrium, and measurement of molecular abundance; improved values for the abundance of poorly known elements from a study of the profiles of Fraunhofer lines more favorable than those hitherto used; profiles of chromospheric lines and, hence, data on the chromospheric model; and knowledge of the atmospheric structure above sunspots.

Spectrum of Selected Regions

Another problem is spectral mapping of specially selected regions on the Sun. For example, the change in the character of the spectrum from the center to a point beyond the limb could be studied. This requires a solar image a few centimeters in diameter, with predispersion. The basic instrument described in the previous Section with certain modifications would be satisfactory. For center-to-limb mapping, an area of the Sun about 5 sec of arc wide radially would be covered by the slit, which might be curved. The essential difference between this and the previous use of the instrument is that it would be flown in a rocket that provides 5-10 sec of arc pointing accuracy and comparable stabilization in roll.

The basic instrument could be modified to accept light from a small area of the Sun and thus to study spot spectra or prominence spectra. By placing the slit beyond the limb, chromospheric and coronal emission lines could be detected. This might require, however, the use of an external occulting disk to reduce the stray light from the disk as a whole.

An Aerobee-150 would probably provide enough space for this instrument using folded optics. It is certain, however, that a rocket with greater payload and space potential is highly desirable. Physical recovery by parachute of the instrumentation and photographic film is necessary and this rules in favor of a rocket rather than a satellite. For use in rockets, the full capability of this instrument requires programmable offset pointing to 5 sec of arc accuracy.

This instrument should also be considered for flight in an early AES using a stabilizing system of the type proposed by Ball Brothers (ATOM). Observations with this instrument would be an excellent task for a scientist-astronaut. He might, for example, secure a time sequence of high-resolution flare spectra by pointing the instrument at the flare as soon as it was detected by other means. Spot spectra and prominence spectra could also be obtained by manned pointing of the instrument.

A high-resolution solar spectrum recorded from disk center to beyond the limb would be of great value in perfecting the photospheric model and extending the model across the transition region. It would also allow the study of possible abundance anomalies. The spectrum of active regions could be followed from beyond the limb in the low chromosphere across the transition region and into the photosphere to determine how the structure of normal chromospheric features and active regions is modified with increasing depth in the atmosphere. Certain lines in this region are also formed in the chromosphere and even up to the chromosphere-corona interface, and information about these lines would help in a better understanding of these regions.

Line Profiles

The third and long-range phase is the study of certain selected lines with still greater spectral resolving power ($\lambda/\Delta\lambda \sim 10^6$), greater spatial resolving power (0.1 sec of arc), greater sensitivity to small variations in intensity ($\Delta I/I \sim 0.1\%$), and greater time-resolution (so as to provide cinematographic presentation). Not all of these requirements would be met in a single experiment; in fact, many combinations are envisioned. Various types of instruments would have to be developed as no single instrument could accomplish all these objectives.

Some of the most obvious lines for early study are Si III (1892 Å); Si II (1817, 1808 Å); C I (1657 Å); He II (1640 Å); C IV (1548, 1551 Å); and Si IV (1340, 1403 Å).

Preliminary work could be carried out in a very large stabilized rocket providing recovery of the photographic film (perhaps a triaxially stabilized Aerobee-350 would be sufficient).

As larger and larger orbiting telescopes become available, the resolution in wavelength, intensity, angle, and time can be improved, but the completion of the full task will be one of the goals of a completely equipped manned orbiting laboratory with a telescope of at least 1-meter aperture.

The results of such observations would add greatly to our understanding of how granulations disappear, turbulent effects arise, spicules enter, and active regions are formed as one looks higher and higher in the solar atmosphere. Another aspect of the investigation would be the time variation of certain features of these line profiles, which are formed in the chromosphere and low corona. These lines should be observed and analyzed in a manner analogous to that employed with the wiggly line spectra from the visible part of the spectrum. An additional purpose for this observation would be to study the structure of the chromosphere and chromosphere-corona interface.

Monitoring the Total Solar Flux at Low Spectral Resolution

The flux from the entire Sun should be monitored at low spectral resolution over the wavelength range 3000-1200 Å, with an accuracy in intensity measurement such as to detect variations of less than 1% over long periods of time. The absolute accuracy need be no better than 5%. The spectrum would be monitored in sections of 10 to 100 Å in length, and these measurements should be carried on over an entire solar cycle.

These data are of interest both to aeronomy and to solar physics. For the benefit of aeronomy, for example, a band centered at 2550 Å, the peak of the ozone absorption, should be monitored with approximately a 10 Å bandwidth. For solar physics, the continuum just short of 2085 Å should be monitored, since here the radiation comes principally from the transition region between photosphere and chromosphere. Another region of the continuum from 1520 Å to about 1475 Å should also be monitored as a large fraction of the continuum in this range is produced by the recapture spectrum of Si II and originates in the chromosphere.

Such monitoring experiments must be carried on from an instrument that is kept pointed at the Sun in pitch and yaw but need not be roll-stabilized. A suitable vehicle is OSO. However, it would be highly advantageous to place OSO in a polar orbit so as to obtain continuous monitoring of the Sun. Alternatives are a solar Explorer or an Orbiting Geophysical Observatory (OGO). The high pointing accuracy of AOSO is not required, though this measurement should be considered as a suitable complement to first-priority AOSO experiments.

Spectroheliogram Studies

A huge number of useful spectroheliographic observations—ranging from the very simple to the most sophisticated, from the one-shot type to the monitoring kind with cinematographic display—can easily be conceived.

The vehicles required, likewise, range from the present biaxial-pointing-control Aerobee rocket to the largest imaginable Orbiting Solar Observatory. A few of the most obvious types that are of interest are noted below:

a. Lyman-α spectroheliograms with a spatial resolution of 1 sec of arc recorded in the light of the whole line every 10 sec; also, spectroheliograms at 5 sec of arc spatial resolution monitored every 10 to 30 sec, but with velocity discrimination produced by selecting different parts of the line. Experiments of this type are being constructed for AOSO by the Harvard College Observatory and by NRL. If these experiments show new features, they should be carefully monitored over long periods of time. Finally, it may prove desirable to obtain 0.1 sec of arc spatial resolution, which could only be done with a large orbiting telescope.

b. Various emission lines—for example, He II (1640 Å); C I (1657 Å); C IV (1548, 1551 Å); Si II (1817, 1808 Å); Si III (1892 Å); and Si IV (1340, 1403 Å).

c. The autoionization lines of aluminum at 1935 Å.

d. Selected spectral regions that are largely continuous radiation, but in which the radiation comes from various heights in the solar atmosphere. Some examples are: radiation at about 2700 Å, which comes from the visible photosphere; radiation at 1950-2050 Å, which comes from the very outer layer of the photosphere; radiation from the capture continuum of Si II in the range 1490-1525 Å, which originates in the low chromosphere; and radiation in the range 1275-1375 Å with the emission lines excluded by means of a masking arrangement that would permit detection of only the continuum and the very faint emission lines (there is reason to believe that this radiation may originate in the photosphere or in the transition region).

e. 1715 ±3 Å to map the Sun in the molecule CO. This would perhaps greatly advance our understanding of sunspots and the surrounding chromosphere.

It is obvious that experiments of these types can be carried out in the solar-pointed triaxially stabilized Aerobee-150, especially after its pointing accuracy is perfected to 5 sec of arc. Experiments can also be designed to advantage for AES, for in this program, the astronaut could activate the equipment during a solar flare so as to record changes taking place in the particular wavelength range being studied and of the type observed by Moreton in H-α. Equipment of this sort would certainly be incorporated in any large manned orbiting observatory.

Magnetometry

The Zeeman splittings of the spectral lines originating in the chromosphere and lower corona should be investigated to ascertain whether use of these lines to study the magnetic fields from these regions is feasible.

WAVELENGTH RANGE ABOVE 3000 Å

Since the region of the spectrum with $\lambda > 3000$ Å is the domain of ground observations by well-equipped observatories all over the world, the only purpose of space observation here is to surpass the atmosphere-limited ground capabilities quite emphatically. This means at the outset that the observing equipment in space must generally have larger apertures than the largest useful apertures of ground-based instruments.

We list below some of the features of the Sun for which the spatial resolution of a space telescope (0.1 sec of arc, say) would mean a significant advance in knowledge crucial to the basic current problems, discussed in Section 1.

1. Broadband Features

a. Granulation in white light or broad bands. Determine contrast, evolution, shapes, size distribution, possible ordered arrangements, systematic changes in and near active centers.

b. Sunspot structure. Determine photometric characteristics and evolution of penumbral filaments, umbral granulation, and the foreshortening effects near the limb.

c. Faculae. Structure and evolution.

2. Spectroheliograms and sharp-band-filter observation

a. Structural details and their evolution in the plages, active center whorls, spicules at the limb and on the disk, prominences, and active coronal regions.

b. The still unresolved pebbly looking background seen in the best H and K line spectroheliograms and large-scale filtergrams.

c. "Moustaches" or "points" (which are comparable in size to granules).

d. Photospheric structures at different levels shown in spectroheliograms in medium and weak Fraunhofer lines.

e. Velocity and magnetic spectroheliograms.

3, Spectrographic observations at $\lambda < 1$ micron

a. Line profiles of very small features (granules vs. intergranular spaces, spicules on the disk and at the limb, prominence strands, flare details, etc.).

b. Distribution of velocities in the line of sight (shown as wiggly lines) to the highest resolvable spatial frequencies.

c. Point-to-point variation of flare and prominence spectra over the whole observable wavelength region (with a universal spectrograph of moderate dispersion).

The problem in observing the corona at large R is not one of resolution, but of freedom from scattered light originating in the Sun. Coronal brightness starts near the limb at about 10^{-6} times that of the photosphere and decreases steadily outward to the level of the zodiacal light, about 10^{-10} times the photospheric brightness. Externally occulted coronagraphs in space could work effectively at these levels. On a continuous basis, observations of the white corona are perhaps our only hope for seeing directly the outward motions of the plasma clouds detected near the Earth and relating them to the various types of radio bursts. Such observations are very important.

In the infrared, the present need is for any information whatever at wavelengths between 3 and 300 microns. The receivers and graded filters for the determination of the energy distribution with a spectral resolution of about 10 are available. The observations could best be made from balloon or airplane altitudes and do not require the continuity of satellite vehicles. The satellite should be used, however, to study the infrared characteristics of large sunspots and plages. (The resolution of the 1- to 1.5-meter telescope proposed below would be just about sufficient at these wavelengths.) Until we have this kind of start, it is difficult to predict the next logical steps.

Because the variety of possible significant observations is very large, we believe that a solar space observatory should be a versatile multipurpose instrument, basically similar to a well-equipped ground observatory, prepared to perform as many as possible of the different functions we can foresee, as required. A most important feature of this concept is that once a really versatile observatory is in orbit, it is a ready facility capable of quick changeover to deal with different observational problems. The lead-time for a given experiment could be reduced from years to weeks. If an experiment fails it is not a catastrophe—little more than some observing time is lost. Such a capability would virtually eliminate the most serious deterrents to potential space experimenters in solar research, and we could expect a very large increase in the participation in the solar space program.

We feel that spatial resolution in the visible spectrum better than 0.1 sec of arc, guiding on any point of the Sun accurate to 0.05 sec of arc, and acceptable light efficiency from 3000 Å to about 1 mm, are the minimum requirements to achieve the significant gains over ground-based performance that would justify a space solar observatory. These requirements automatically specify a reflecting telescope of 1- to 1.5-meter aperture. An acceptable image scale, with the $\lesssim 0.1$ sec of arc resolution element exceeding 25 microns in the solar image, requires an equivalent focal ratio $\gtrsim f/50$. If the reflecting optics were coated with aluminum, the range of the telescope would actually extend down to about 1500 Å, which increases its utility very considerably. The telescope would have to be a very long structure (50 to 75 meters), which might conceivably be assembled in space or, less desirably, a shorter compound system with its more severe thermal problems. As proposed in Section 3, under Wavelength Range 500–1500 Å, Radiation Monitors, this telescope would be supplemented by one of similar aperture designed to operate in the spectral range λ 500-1500 Å, but with lesser spatial resolution of 0.5 sec of arc.

The telescopes would be designed to feed any of a number of attached accessories. They include:

a. The most powerful stigmatic spectrograph possible, with facilities for spectrum scanning in a double-pass arrangement and for photography.

b. A large spectroheliograph, adjustable to any desired wavelength, with adjustable band pass down to about 0.02 Å.

c. A stigmatic universal spectrograph for recording a long section of the spectrum in a single exposure.

d. A stigmatic infrared spectrograph (the characteristics of which will be less obscure when balloon experiments give us more information).

e. A camera for direct white-light or broad-band-filter photography.

f. A similar camera with appropriate scanning equipment for the infrared.

g. Other fundamental accessories that solar astronomers will doubtless propose.

A second important, but far simpler, instrument would be an externally occulted white-light coronagraph. (An internally occulted coronagraph is of no use in space; since no one has succeeded in reducing the scattered light to less than about 5×10^{-6} $I_{\odot}$ (intensity of the disk) or 5 times the brightness of the inner corona, it works nearly as well from the ground.) The bulky part of this instrument is a simple $\geq$ 10-meter boom that positions the occulting disk with respect to the relatively compact camera. The geometry is such that a zone of the lower corona is vignetted, since the occulting disk and corona cannot simultaneously be in focus. The vignetted zone is reduced by reducing the camera aperture (which increases the depth of focus) or by lengthening the boom (which moves the occulting disk toward optical infinity). Since the corona varies in intensity from 10^{-6} $I_{\odot}$ at $R = 1R_{\odot}$ (solar radius) to about 10^{-10} $I_{\odot}$ at R about 20 $R_{\odot}$, two or three coronagraphs on a common boom would be required to achieve the necessary dynamic range. The bright inner corona would be observed with a long camera of 0.5- to 1.0-cm aperture with a very narrow vignetted zone, while a short 10- or 15-cm aperture camera with a broad viginetted zone could reach the outer corona.

If the space telescope is to be 50 or 75 meters long, it would serve as a most admirable coronagraph boom, although some provision would be necessary to resolve occasional conflicts in pointing between the telescope (which may be offset or scanning) and the coronagraph (always pointed at the center of the Sun). The corona would be accessible from about $R = 1.1$ $R_{\odot}$ on out. The shorter compound telescope would be less favorable, but could carry an attached 10-meter boom, with which the corona would be accessible from $R = 1.3$ $R_{\odot}$ out with longer exposures.

The often suggested possibility of an independent orbiting occulting disk at a great distance from the telescope is very attractive optically, but may present difficult problems of control; however, such a system may be possible within a finite number of years. If so, the problem of attaining a sufficiently large ℓ is replaced by that of using a sufficiently large occulting disk. If we consider a diameter of 5 meters reasonable, $\ell \approx$ 500 meters. With a guiding accuracy of $\Delta\theta = 10^{-3}$rad, we find that the corona is observable at $R > 1.25\ R_\odot$ with a 15-cm aperture capable of resolving 1 sec of arc and fast enough to detect the outer corona.

The telescope, its accessories, and the coronagraph constitute a fairly complete observatory, capable of dealing with most of the problems that are beyond the reach of ground-based observation. As described above, this is at best a very complicated array of instruments, the effectiveness of which will depend on the precision of mutual adjustment of individual components and of the telescope to its accessories. Furthermore, a long useful life is an important consideration for a project of this magnitude. It is clear that men in space, who could assemble things in place and perform periodic maintenance and servicing would enormously simplify the initial design of the system and prolong its operating lifetime. We then have the possibility of minor modifications (like replacement of gratings with better new ones, as the ruling art improves), attachment of new small accessories (a video tube of better discrimination, or an image dissector, for instance), the replenishment of expendables, alterations in setup for the next series of observations, and recovery of data films and tapes. Although continuous observations throughout the life of the system require ground-controlled unmanned operation as the normal, a man might be able to make some special observations during routine visits.

While this is an exceedingly attractive picture, we should consider the alternatives that are open if manned maintenance and servicing turn out to be impractical. A most important function of the man would then be lost, namely, the recovery of photographic film. Various photographic emulsions are capable of recording and storing from 10^6 to 10^9 bits per cm^2 simultaneously, and the area that can be used is practically unlimited. For unmanned observations we must either plan on recovery of film canisters (or the whole observatory) or find a substitute for photographic film that is within the communication capacity of the vehicle. Single-point scanning is clearly impossible. For example, a coronagraph of 1 min of arc resolution working to 20° elongation can record a picture photographically in about 1 min, while the single-point recording of the same data would require a day. A spectroheliogram that takes a minute to record with 0.1 sec of arc resolution would require 9 hours to record point by point over a 10-square min of arc area. However, image storage and wide-band video recording are a likely substitute for photography, and the recovery of film canisters might be substituted for manned operation in space. A third possibility is on-board development of film followed by broad-band video transmission, a system used in the lunar orbiter. The independent development of video methods seems very likely, and the recovery of canisters would appear to be less difficult than safe manned operations in space. We should regard these alternatives as serious

Table 1. Characteristics of Various Observing Techniques

	System	Potential Lifetime	Spatial Resolution*	Continuity of Observation	Wavelength Region Accessible	Recording† Method
I	Ground-based telescope	∞	Limited to 1 sec of arc by seeing, regardless of aperture	Determined by weather, day and night station network density and distribution	3500 Å- 1 micron plus windows	All
II	Balloons or high-flying aircraft	1 day per flight	Limited to possibly 0.15-0.2 sec of arc by seeing	Costly for continuous observation	3000 Å- 1 cm	All
III	Nonrecovered satellite	About 1 year	Diffraction-limited: < 0.1" for 50" aperture	Determined by telemetering capability**	All < 1 km	Video transmission of photographs or real time in cryogenic bolometers
IV	Recoverable satellite	Determined by equipment life and film supply or data system	Diffraction-limited: < 0.1" for 50" aperture	Almost continuous, depending on orbit***	All < 1 km	All except bolometer (films and tape recovered)
V	40" to 50" telescope in orbit and serviced by men	1 to 10 years	Diffraction-limited: < 0.1" for 50" aperture	Almost continuous depending on orbit***	All < 1 km	All

*Resolution is at λ5000 Å (see Figure 1 for resolution limits).

**Includes the problem of station network density, bandwidth of the telemetering, and possibly of wideband transmission to one station real time from a synchronous orbit or by the use of a relay satellite.

***For an OSO type orbit two-thirds of the time is on sunlight.

†All means + video + photomultiplier + He.

competitors with manned operations, and see what losses to the program could be expected. Table 1 summarizes the situation as it looks now.

It is evident that unmanned balloons and aircraft at 70,000 ft and recoverable satellites could perform very well, but with a more limited repertoire than the manned station. All these methods of observation, however, must have the same telescope aperture as the manned operation for use in the visible spectrum. The complexity of the unmanned satellites would be much greater than that of the manned station, their life expectancy considerably less, and their reliability very much less. Balloons or aircraft platforms appear to be the equal of the manned station for programs that can be accomplished by one-day flights if sufficient guiding accuracy can be had, and would probably be preferable for most of the infrared work.

Obviously, such a man-maintained space observatory would be very attractive to a solar astronomer. A great deal can be done with any of the systems, however, provided the limited life for such a large piece of equipment is acceptable.

MANNED ORBITING TELESCOPE (MOT)

In this section are collected certain technical considerations relating to the collection, storage, and transmission of data by a telescope of very

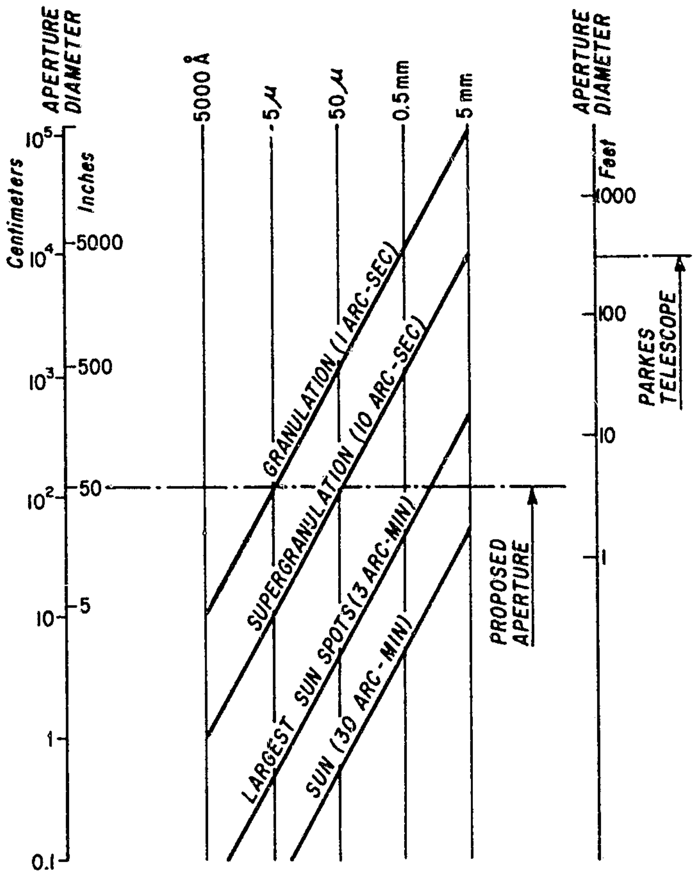

Figure 1. Resolution limits of instruments of various apertures, operating at wavelengths between 5000 Å and 5 mm.

high resolving power. Figure 1 shows the resolution limits of instruments of various apertures operating at wavelengths between 5000 Å and 5 mm as these limits relate to solar features of special interest.

In light of Table 2, we examine the feasibility of current scanning methods, in which the elements of the solar surface or the spectrum are examined individually in succession, or with point-by-point scanning. The photons from a single element enter the telescope aperture and are converted into counts of some sort. The required photometric accuracy of 1% leads to a required signal-to-noise ratio of 100, which is achieved by a count of 10^4. The total point-by-point scanning time, T, required to cover

Table 2. Information Obtained by a Large Solar Telescope

Mode of observation	n	Counts ϵn	Δt(sec)	N	T (=N Δt)
XUV spectroheliograms	$\sim 10^7$	$4\text{x}10^4$	0.25	$3\text{x}10^4$	10^4
XUV spatially resolved spectra	$\sim 10^7$	$4\text{x}10^4\delta\lambda$	$0.25/\delta\lambda$	180k	$50k/\delta\lambda\ (5\text{x}10^5)$
$\lambda > 1500$: broad-band photographs	10^{11}	10^{10}	10^{-6}	$3\text{x}10^6$	3.0
$\lambda > 1500$: spectroheliograms	$10^8\,\Delta\lambda$	$10^7\,\Delta\lambda$	$10^{-3}/\Delta\lambda$	$3\text{x}10^6$	$3\text{x}10^3/\Delta\lambda(3\text{x}10^4)$
$\lambda > 1500$: spatially resolved spectra	$10^8\delta\lambda$	$10^7\delta\lambda$	$10^{-3}/\delta\lambda$	1800k	$1.8k/\delta\lambda(2\text{x}10^4)$

n = number of photons incident on 1-meter aperture per second from one element of area on the solar surface with a resolution of 1 square sec of arc in the XUV and 0.01 square sec of arc in the visible.

ϵn = number of counts per second resulting from n photons.

Δt = time required to accumulate 10^4 counts at a rate of ϵn/sec.

N = number of resolved elements in scanned area of 10 square min of arc.

T = time required for a point-by-point scan of N elements.

$\delta\lambda$ = wavelength resolution (bandwidth) of a spectrum scan in angstroms. A value of $\delta\lambda = 0.01$ Å would be about right.

$\Delta\lambda$ = bandwidth in the visible spectroheliograms, perhaps 0.1 Å.

k = $(\lambda_1 - \lambda_2)/\delta\lambda$, where λ_1 and λ_2 are the limits of a spectrum scan, which might be 10^3.

10 square min of arc of the solar surface is calculated very roughly and listed in the last column of Table 2. Although the calculated T's could easily be wrong by a factor of 10, it is quite evident that the point-by-point scanning times are far too long to permit reasonable time resolution (1 min of arc, say), and it will be necessary to do our recording either by photography or by a video receiver. The video receiver can theoretically record all elements of the observed area simultaneously, and reduce the time required by a factor of about 10^4 in the XUV and 10^6 in the visible. Photography should be about 20 times slower. Either method of recording largely removes the quantum limitations of the system.

4. REVIEW OF RESULTS OF SOLAR SPACE RESEARCH

A great deal is now known about the solar spectrum, from the atmospheric cutoff, about 3000 Å, through the extreme ultraviolet (XUV), and into the x rays. Generally, the shorter the wavelength, the more there is that remains to be done. A start has been made on spectroheliographic work in the XUV. Intensity measurements are far from being sufficiently exact. Monitoring work has been carried out in Lyman-α, in XUV spectra, and in various broad x-ray bands. Most of the results have been obtained with the Aerobee-150 vehicle and biaxial pointing control (BPC). Results of the monitoring type have been secured with OSO-I and OSO-II, and with the small Solrad satellites.

However, the United States has neglected much-needed vehicle developments for solar work. The only new vehicle for solar research to appear on the scene since OSO-I (1962) is the U.K. stabilized recoverable Skylark rocket. This promises to be of great importance for solar physics. The first three flights (of the prototype device) were successful, providing pointing in yaw and pitch to better than 10 min of arc, and comparable roll stabilization. The next units are expected by Elliott Brothers to point with less than 10 sec of arc noise in yaw and pitch, and $<1^\circ$ noise in roll. This rocket has already made it possible for the United Kingdom to secure XUV spectra of the chromosphere and corona separated from the photosphere, from 1000-3000 Å. For this purpose the spectrograph was equipped with a mirror, servo-pointed to ±2 sec of arc, which kept the Sun's limb positioned at 10 sec of arc from the slit.

In the following sections the present status of solar XUV accomplishments will be summarized in more detail, topic by topic; needs for future progress are included. Tables 3a and 3b present a summary analysis of the spectrum as known at present.

Table 3a.

λ(Å)	Instrument	Wavelength Precision (Å)	Resolving Power (λ/Δλ)	Lines and Identification
3000-2085	NRL echelle	0.01-0.02	10^5	≈ 1/2 of 7000
3000-1000	UK, 10" above limb	~ 0.1	~ 5 x 10^3	new coronal lines
2000-1200	NRL crossed-dispersion	0.05	10^4	≈ 1/3 of 1000
1200-500	NRL crossed-dispersion	0.03	10^4	most of 200
1200-250	AFCRL photoelectric	~ 1	~ 5 x 10^2	
500-149	NRL photographic	0.03-0.1	1-2 x 10^3	1/5 of 250
300-149	AFCRL photoelectric	0.1-0.2	~ 5 x 10^2	
340-170	NASA OSO-I photoelectric	~ 0.3	~ 2 x 10^2	
149-55	AFCRL photoelectric	~ 0.2	~ 10^2	
80-33	NRL photographic	~ 0.05	2 x 10^2	most of 60
25-13	NRL Bragg spectrometer	~ 0.1	~ 10^2	13 of 14

Table 3b.

λ(Å)	±ΔI/I (rel. reλ)	±ΔI/I, abs.	Notes	Needs
3000-2085	≈ 10%	≈ 20%	Stray light fills cores	Large grating spectrograph, center-to-limb precise pointing
3000-1000				
2000-1200	≈ 15%	≈ 30%	Semiquantitative center-to-limb	Large grating spectrograph, improved center-to-limb precise pointing
1200-500	≈ 30%	-	Semiquantitative center-to-limb	Large grating; increase reflectance, improved center-to-limb precise pointing
1200-250	10-25%	10-25%		Higher altitudes
500-149	≈ 30%	≈ 50%	A few spectroheliograms	Larger spectrograph, higher altitudes, more exposure, observations during active Sun
300-149	≈ 50%	x or ÷ 2		Higher altitudes, solar activity
340-170	≈ 30%	-	Time studies	Greater wavelength coverage
149-55	≈100%	x or ÷ 4	Signal too low	Higher altitudes, increased solar activit
80-33	≈ 50%	x or ÷ 2		Larger spectrograph, greater exposure, and solar activity
25-13	≈ 25%	≈ 25%	Contrast of plages vs. disk	Larger spectrometer, more time

MAPPING THE SPECTRUM

Here the emphasis is on precise wavelength measurements, recording as many lines as possible, and identifying them. So far, this work has been done with small grating instruments, shorter than about 2 ft in length, and with photographic recording for the most part. Attainments are listed in Tables 3a and 3b.

From 3000-2085 Å, the NRL echelle has produced spectra of the central region of the Sun, with a wavelength precision and resolving power that are sufficient for most purposes, except at $\lambda < 2200$ Å, where the intensity becomes low. The spectra suffer, however, from stray light inherent in the echelle, which limits severely the detection of lines of intensity -2 and -3, and fills in the cores of Fraunhofer lines. Although the dispersion is high, the spectra are narrow; hence intensity discrimination is limited by noise from granularity. The need in this photographic range is for a large grating spectrograph that is equipped with predispersion, and for freedom from stray light. A second need is for measurements of the change in spectrum from center to limb. For this purpose a well-stabilized Aerobee-150 is essential for further progress.

As mentioned earlier, the U.K. stabilized Skylark has already flown successfully and resulted in obtaining low-dispersion spectra just above the limb. A number of new coronal emission lines were discovered in the wavelength range 2000-3000 Å in the recent flight in April 1965. This vehicle appears to have a potential ability to meet some of the needs for future solar research.

From 2000-1200 Å, the spectrum has been well recorded with the NRL crossed-dispersion spectrograph. The wavelength precision and resolution can be improved, with the existing instrument, by a factor of 2, to about 0.02 Å and 2 x 10^5, respectively. Center-to-limb data are obtained, but are qualitative for lines, because of the narrowness of the spectra, and are limited to 1 min of arc spatial resolution at present. The need for future research is for a larger stigmatic grating spectrograph, similar to that discussed in the paragraphs above for the 3000-3085 Å region. Remarks similar to those in the same paragraphs apply also to the vehicle, as well as to the results obtained by United Kingdom investigators. Guidance to 5-10 sec of arc is essential.

The range 1200-500 Å has been covered with the NRL crossed-dispersion spectrograph, with just about all the wavelength precision and resolution it can produce. This instrument is intensity-limited below 1000 Å, however, and somewhat longer exposures would probably reveal ten times more lines. This range has also been covered with photoelectric grazing-incidence spectrographs, by the AFCRL; the resolution and wavelength precision are considerably less, but the intensity measurements are more accurate than for the NRL results. This is a spectral region of great interest, containing important continua and chromospheric and coronal lines. To make further progress a larger instrument is needed for increased wavelength accuracy and resolution, with longer exposure times and/or increased reflectance to record the spectrum more completely. Here again, the stabilized Aerobee-150 is greatly needed.

The range 500-149 Å has been recorded photographically with grazing-incidence spectrographs (equipped with aluminum filters 1000 Å thick to eliminate stray light) and photoelectrically with the AFCRL and GSFC scanning monochromators. 500 Å is, of course, an arbitrary division point, below which grazing-incidence instruments usually become faster than normal incidence. With the small NRL photographic instruments, a wavelength precision of 0.03 Å and resolution of 0.1 Å have been attained over part of the range, and are expected in the future over the entire range. The aluminum filter sets a limit at 149 Å, but the solar spectrum itself becomes weak below 171 Å and very weak below 149 Å. For further progress here, larger grazing-incidence instruments are required. Again, the solar-stabilized Aerobee-150 is needed, with a peak altitude of 275 km. Longer exposure times are also needed. An astronaut-operated grazing-incidence instrument permitting exposing for long time periods is being constructed for Apollo. Following this the Apollo Extension Systems offer even greater promise. Much research in the laboratory is required to develop satisfactory methods of measuring intensities.

From 149-80 Å, the spectrum has been recorded photoelectrically by AFCRL, but photography of this region awaits the perfection of metallic filters other than aluminum, for example, beryllium. The intensity of the signals obtained has been very low, so that the wavelength precision is greatly reduced by statistical noise and one can be certain of only a few strong lines. No doubt, when the Sun once again becomes active, the many lines that are surely present will be recorded with precision sufficient to make certain of their wavelengths and identifications. Except for a few stronger lines, one can say little more than that radiation is present, at an intensity level given by an averaged curve. For future progress, work on the beryllium filter is badly needed. Long exposure times will be required for photography. The photographic experiment is an excellent one for Apollo and the Apollo Extension Systems, and the photoelectric version will certainly find a place in a large orbiting telescope.

From 80-33 Å, the spectrum has been photographed with the NRL grazing-incidence instrument. This was possible because aluminum becomes transparent once again in this range, as the K edge at 8 Å is approached. The photoelectrically recorded AFCRL spectra extend to 55 Å, but the intensity is extremely low, and there is only qualitative agreement with the photographic spectrum. Some further progress here can be made with existing vehicles, since considerably improved grazing-incidence spectrographs can be mounted within the existing biaxial pointing controls (BPC). Longer exposure times than rockets permit are greatly needed; therefore, the use of Apollo and of the AES is strongly recommended. Larger instruments will also be needed to obtain the high resolving power that will be required to resolve the many closely spaced lines that are certainly present. Here, the spectrum will change greatly with solar activity.

Below 33 Å it is still entirely feasible to obtain spectra with grazing-incidence instruments; longer exposures and a grazing angle of 2° rather than 5° are required. The Bragg crystal spectrometer is a powerful instrument for this range. The first spectra here were obtained by such an instrument, flown in 1963 by NRL, and covering the range 13-25 Å. For future work with Bragg spectrometers, the triaxially stabilized Aerobee is greatly needed. This spectrometer could also be flown on AES, and operated by the astronaut. This would be of interest when solar activity is high, and especially during a flare.

The conclusions from this summary of spectrographic results and needs are:

a. The triaxially stabilized Aerobee-150 is greatly needed. Even with 1 min of arc pointing it would be valuable, but 5 sec of arc pointing is required for much of the work. If this is not made available soon, the British will leave us behind in XUV solar research.

b. Apollo will be quite useful for small spectrographic experiments, because of the long exposures available, with the recovery of film, and the possibility of exposing during a flare. The AES provides an opportunity to extend this capability to large spectrographs.

c. Much research in the laboratory needs to be done to increase spectroscopic capabilities in the XUV.

LINE PROFILES

From 3000-2200 Å, profiles having $\lambda/\Delta\lambda = 10^5$ are available from the echelle spectra. They suffer from stray light that fills in the cores of the strong lines, and they apply to the central region of the Sun only. The profiles of the H and K lines of Mg II are fairly satisfactory as to spectral resolution; the intensity data are probably good to about 5% across these lines.

Beyond 2000 Å, the only lines as yet studied at high spectral resolution are Ly-α and Ly-β of H. Ly-α was photographed on July 21, 1959, and on April 19, 1960, at 0.03 Å resolution; Ly-α and Ly-β were photographed on August 22, 1962, at 0.1 Å resolution. Spatial resolution was about 1 min of arc.

There are many chromospheric and coronal lines which should be studied at high spectral and spatial resolution. Larger instruments, triaxial stabilization, and longer exposures are required. Much could be done with a 5 sec of arc stabilized Aerobee. For time sequence studies the AES should be considered.

This type of work complements spectroheliographic studies and is necessary for deciding how to conduct them.

SPECTROHELIOGRAMS

The first detailed XUV spectroheliograms were those of Ly-α of H, obtained in 1959 by NRL. The spatial resolution was 30 sec of arc or better, and has not yet been surpassed. Since this time, photographic spectroheliograms using lines farther in the XUV have been obtained, using a single grating at normal incidence, and an aluminum filter. At their present size, 2 mm in diameter, it is difficult to make quantitative measurements. They do serve very well to show that He II (304 Å) is emitted strongly from the entire disk, and also from active regions, whereas Fe XV (284 Å) and Fe XVI (335, 361 Å) are emitted almost entirely from the active regions. Monitoring of the XUV spectrum by GSFC from OSO-I led to a similar conclusion, based on the changes in line intensities recorded from the entire disk as the Sun's rotation carried active regions into and away from view. To separate other than the strongest isolated lines from the other lines 171-500 Å by such a simple means would require far greater dispersion than is now attainable. More complicated spectroheliographs will be required. Partial spectroheliograms have been obtained by NRL with C III (977 Å), Lyman-β (1026 Å), and O VI (1032, 1038 Å). Recently, GSFC has obtained a spectroheliogram in Mg II (2800 Å).

To shorter wavelengths, pinhole-camera photography is now producing valuable pictures of the Sun in various x-ray bands, defined by filters. The first such photographs, obtained by NRL, suffered from rotation of the image produced by precession of the Aerobee. The U.K., however, thanks to its three-axis stabilized Skylark, has already obtained sharp rotation-free images in several bands. Quite recently, the Kitt Peak National Observatory has obtained the highest-resolved x-ray image with a pinhole camera mounted with a BPC on an Aerobee that was prevented from precessing by a new cross-spin stabilization.

Excellent x-ray images have also been obtained by GSFC in collaboration with American Science and Engineering, Inc., Cambridge, Massachusetts, with a cylindrical parabolic grazing-incidence lens. Two rocket flights have been made in which the Sun has been photographed in wavelengths below 10 Å, between 8-15 Å and between 44-60 Å. The spatial resolution of a March 1965 flight was approximately 1 min of arc, and from a cursory look at the photographs it is obvious that most of the radiation in wavelengths shorter than 15 Å is emitted by plage groups. Limb brightening can be seen in the 44-60 Å photograph, with this radiation being observed at least 50,000 km above the disk. Grazing-incidence optics now exist that should allow resolutions between 5 and 10 sec of arc.

A monitoring type of spectroheliograph, constructed by NRL for OSO-II of NASA, was placed in orbit in February 1965. The wavelengths monitored successfully were Ly-α (1216 Å), He II (304 Å), with a 1 min of arc resolution. Several satisfactory images were obtained. This equipment failed rather early, due to high-voltage breakdown problems; the source of the difficulty has not been established, but it appears possible that the instrument may have acted as a trap for solid particles which eventually built up to the point where they short-circuited critical parts of the electronics.

In order to make progress in spectroheliography it is absolutely essential to have an Aerobee-150 rocket, or the equivalent, which is stabilized to 5 sec of arc in yaw and pitch, and to 0.25° in roll. Three-axis stabilization is required, to make use of long exposure times; at least 5-ft length is necessary to produce solar images large enough to use for quantitative photographic photometry.

Future spectroheliography from OSO and AOSO satellites holds great promise, but much research may be required before the difficulties associated with the operation of optical photomultipliers at high voltage in space vacua are understood and eliminated.

INTENSITY MEASUREMENTS

Measurements of intensity in the XUV, even on a relative basis, or over a short wavelength range, are notoriously difficult. Much work needs to be done in the laboratory to develop standards of intensity, methods of calibration, and so on. This kind of work carries little glory, and has lagged.

The solar intensity distribution is thought to be determined from photographic spectra with an absolute accuracy ±10% from 3000 to 2500 Å. At shorter wavelengths the accuracy gradually deteriorates to perhaps ±20% at 2100 Å, and to ±50% at 1300 Å; Ly-α is, of course, variable with the solar cycle, and this is probably true of many other lines, and the chromospheric continua.

From 900 Å to 170 Å the most accurate intensity measurements are those of AFCRL, made photoelectrically. In the x-ray range various investigators have performed careful calibration of photon counters, but here the solar output varies greatly with activity.

SOLAR MONITORING

This has already been touched on in the preceding sections. Obviously, it is work that must be carried on in orbiting vehicles; except for relatively short-time monitoring, the vehicles must be unmanned.

The first and most extensive series of monitoring experiments are those of NRL, which have been conducted periodically from small satellites since 1960. Generally, hydrogen Ly-α, and several x-ray bands are recorded. Excellent results are obtained, but at present data transmission is limited to real-time telemetry. Therefore monitoring is far from continuous. OSO-I and OSO-II have, however, provided continuous monitoring of Ly-α, XUV spectra, and x-ray emissions from the Sun, except when in the Earth's shadow. OSO is an ideal vehicle, since it provides solar pointing and data storage. If placed in a polar orbit, it would appear to meet all monitoring needs.

Lyman-α from Flares

An ionization chamber sensitive to Ly-α was flown on OSO-I. During the lifetime of the experiment, several flares were observed. These showed enhancement of the Ly-α flux of 2-10% which, when the area involved is considered, indicates that the brightness enhancement in Ly-α from the localized region as compared with the background could be as much as 100 times greater.

Solar XUV Spectra

The use of the OSO-I as a stable platform permitted the acquisition of solar XUV spectra which can be tentatively associated with a corona disturbed to varying degrees by visible centers of activity. Periods of relatively low solar activity were followed by periods during which active centers appeared on the solar disk. The increases and decreases in flux can be associated with the appearance and disappearance of these centers. Analysis of the observed emission lines demonstrates that the lowest counting rates of the period were observed when the sunspot number was near zero and the calcium plage area on the Sun was also at a minimum. However, it is also clear that no exact correlation can be assumed to exist between the XUV fluxes and ground-based observations. Fe XV (λ284) radiation has a different time dependence on the age of active regions than has 2800 MHz microwave radiation. It increases more slowly than does the microwave radiation as the active center develops, but remains intense even after the sunspots and flare activity have disappeared and the microwave radiation is decreasing. The continued enhancement of the Fe XV line after all sunspots have vanished may be an indication of remaining coronal structures. In any event, these observations suggest that it is necessary to have knowledge of the recent past history of solar activity as well as current data in order to make a correlation of XUV radiation with other data.

The XUV emission lines display fluctuations which differ from one line to another. In particular, one may observe fluctuations in the helium line which are not found in the other lines. These short-lived variations can sometimes, but not always, be associated with the brightening of existing plages and the occurrence of radio noise storms at 169 MHz.

The coronal lines of Fe XV and Fe XVI are strongly associated with plages, but do appear to have residual intensities even if the Sun shows no sign of activity. A quiet Sun component does exist when one extrapolates the Fe XV counting rate to zero plage area. Assuming that the regions of increased Fe XV emissions are equivalent in area to the plages, one obtains a plage-to-quiet Sun Fe XV ratio of between 200-300:1, considerably beyond the latitude of photographic film. This quiet Sun component may perhaps be associated with coronal fine structure rather than being uniformly distributed over the solar disk.

We observe that those ions that exist at electron temperatures below about 1 x 10^6 °K (and these include the lower stages of ionization of iron as well as ions of Si VIII through Si X and Mg VIII and IX) show little association with active regions, while those ions existing above 1 x 10^6 °K show a strong association with plages and active regions. The fact that all lines show an increase with activity during the first two weeks in March merely indicates an increase in density in and around the active region. It is not clear from the data whether the large increases in Fe XV and XVI are due to a combined increase of electron temperature and density over plages, or whether localized regions in which these emissions might occur merely increase in number over plages. All lines are observed to fluctuate, if only slightly. The smaller the fluctuations, the less well they correlate with solar activity. However, since the experiment lacked spatial resolution, it is not possible to state how the smaller enhancements in intensity are distributed on the solar disk.

X-Rays Below 11 Å

An ionization chamber was flown on OSO-I to monitor 1-11 Å x rays. Because at the time of launch the solar cycle was approaching minimum, the sensitivity of the experiment was set to observe the quiet Sun. Full-scale sensitivity was 1.8 x 10^{-3} erg/cm2 -sec. This would allow observations for quiet periods, but was somewhat of a handicap in that when the Sun was relatively active the reading was off scale. The smallest measured value occurred on April 6, 1962, at which time a value of 1.8 x 10^{-4} erg/cm2 -sec was observed. The flux values for 2.5 solar rotations varied with plage activity on the disk of the Sun and were the first observations to demonstrate that practically all of the x rays in this wavelength region when no flares are present are emitted from the plage regions. The data also showed that x radiation from the Sun is quite variable and that completely quiet periods are rare. Only six orbits were found in which the flux did not vary by more than 5%.

Strong correlation was found between 1-11 Å x rays and the 9.1-cm radio fluxes. Calculations made using temperature and density data supplied by the High Altitude Observatory showed that the x radiation could not be a continuum. Since the calculated values were an order of magnitude too low, it was concluded that line emission must be a major component. This has been substantiated by subsequent NRL observations.

Attempts were made to correlate x-ray flare data with H-α flare observations; of a sample of 22 events, 11 correlated well. However, there were six full-scale x-ray events where one would have expected an H-α flare to be observed (based upon the fact that smaller x-ray events were observed for which H-α flares were reported), but for which no H-α flare was seen. The question of correlation is still open since it may be that if increasingly smaller events were classified as flares, one might find that all of these events would have been observed in H-α. On the other hand, one might well ask, is there a type of event in which x rays are emitted without a counterpart in H-α?

In attempts to compare the x-ray data with other measurements such as optical data and radio data, one concludes that x radiation is the most sensitive detector of solar activity. As an example of this, several events were found in which the x-ray experiment observed a 100% increase in flux, whereas for the 9.1-cm radio data, the event would not have been detected unless one knew at what time to examine the data closely. The data tracing varied approximately the width of the plotting pen line.

HARD X RAYS

Attempts to detect x rays of energies greater than 100 keV from the nonflare Sun have been made from balloons. The detector consisted of a scintillation counter collimated with an active shield consisting of a secondary scintillating material. Although the signal-to-noise ratio was very good, only an upper limit could be determined: for energies above 130 keV, an upper limit was 0.137 ± 0.053 photons/cm^2-sec, and for energies above 300 keV, 0.067 ± 0.044 photons/cm^2-sec. These numbers represent the excess counting rate looking at the Sun as compared with that looking away from the Sun. It is impossible to determine whether the excess count is genuine or not because there may be terminal effects that caused the count to differ. However, instruments have now been built so that in-flight calibration can be made.

An experiment to observe x radiation from the Sun of energies between 20 and 100 keV was flown on OSO-I. As a consequence of the large amount of data obtained, it is possible to set an upper limit of 3.4 ± 0.95 photons/cm^2-sec for nonflare solar fluxes. Eight x-ray bursts associated with solar flares were observed. This doubled the number of observations of this type of burst that had been observed up until that time. For one of the events, there was a good ground-based observation in H-α and it appears that the x-ray event was associated with the explosive phase of the visual flare. Two of the events were of particular interest because they exhibited a double peak in x radiation which raises interesting questions concerning the source of radiation.

THE WHITE-LIGHT CORONA

Monitoring of the outer white-light corona cannot be done from the Earth's surface, except at the time of a total eclipse. This is a project for orbiting vehicles. The first noneclipse photographs were obtained on June 28, 1963, by NRL, from an Aerobee rocket. Balloon altitudes are nearly sufficient, however, and the High Altitude Observatory has obtained coronal photographs with externally occulted balloonborne coronagraphs.

A white-light coronagraph constructed by NRL was operated successfully in OSO-II for the first 1040 orbits. Data reduction has hardly begun, but it appears that the corona was monitored satisfactorily as to its general form from R = 3 to 7 solar radii in both radial and tangential polarizations. The intensities were about twice those observed during eclipses, as was the case with the rocket coronagraph. This is ascribed to dust near the Earth and the fact that during an eclipse sunlight is prevented by the Moon from illuminating much of the relatively nearby atmosphere. It is hoped to detect streamers and changes in the K corona from the data obtained from OSO-II, but the noise in the signal was high, probably partly a result of the dust near the spacecraft. Much smoothing of the data will be required.

The improvements needed for corona monitoring are: a greater separation of the external occulter from the rest of the instrument, a cleaner environment near the spacecraft (though this requirement is not absolutely established), more telemetry, and video techniques. These improvements are planned in the AOSO coronagraph being constructed by the High Altitude Observatory. Another possibility is the use of a manned spacecraft with a guidable occulter system located at a relatively great distance.

APPENDIX:LIST OF PARTICIPANTS

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IV

Radio and Radar Astronomy

1. INTRODUCTION AND SUMMARY OF RECOMMENDATIONS

TASK OF THE WORKING GROUP

The Working Group attempted to carry out the following tasks:

(a) To review the present status of and known plans for the development of radio and radar astronomy in space.

(b) To foresee and outline the major instrumental and observational advances which should take place over the next 15 years.

(c) To form opinions on the use of man as part of the space experiments foreseen.

GENERAL REVIEW OF THE STUDY

The work of the Group can be divided by subject matter into three main subject headings:

(a) Long-wave radio astronomy. Here the Group considered observations made in the frequency range from 10 MHz (30 meters wavelength) to a few hundred kHz (300 kHz corresponds to 1 km wavelength).

(b) Millimeter and long-wave infrared astronomy. The wavelength range here was taken to be from 10 mm wavelength (30 GHz) to a few tens of microns. The short wavelength end of this spectral range was also discussed in the Working Group on Optical Astronomy.

(c) Radar astronomy. The wavelength range here did not require definition; most observations fall in the range between about 6 meters (50 MHz) and centimeter wavelengths. The subject of solar radio and radar astronomy was studied and is reported under this section of the work.

The background information and other considerations under these three headings, and under a fourth general category where techniques and specific questions involving man in space and Moon-based experiments were discussed are set out in detail later in Sections 2 through 5 of this report. Those sections also contain suggestions which the Group wished to make, but which were not of a nature suitable for formalizing into recommendations. The recommendations are listed in the next paragraphs. In this listing an attempt has been made by the Group to mark their most important recommendations by placing an asterisk against them. Such a choice is clearly difficult to make; it is intended to show those recommendations which, if followed, are in the opinion of the Group likely to give the greatest yield in new results.

RECOMMENDATIONS

Long-Wave Radio Astronomy

Recommendation 1. The Radio Astronomy Explorer (RAE) satellite series should be continued and expanded. In addition to having inherent value it is a prerequisite for the instrumental developments proposed in Recommendations 2 and 3. We foresee that, when running at peak rate; launches of at least one a year will be needed. Universities, NASA, and other research institutions should be encouraged to develop payloads and every effort made to shorten the lead times required for approval of a program.

The importance of solar radio astronomy during the maximum phase of solar activity requires that at least two RAE satellites instrumented for long-wave solar radio observations be flown near sunspot maximum to provide data on the low-frequency characteristics of solar radio bursts.

*Recommendation 2. A space radio telescope with an aperture of the order of 20 km appears to be close in size to the ultimate for observations between a few MHz and a few hundred kHz. With larger apertures, effects due to irregularities in the interplanetary medium will, according to our best present knowledge, become the factor that limits the resolving power of the telescope. We recommend that the National Academy of Sciences appoint a panel now to study possible conceptions of such a radio telescope and to initiate studies of the scientific and engineering problems connected with its construction.

*Recommendation 3. Since the study recommended in Recommendation 2 is for an ultimate space radio telescope, work should be started now that will lead to the use in space, within about ten years, of a high-resolution broad-band antenna system for radio-astronomical observations over the frequency range 10 MHz to a few hundred kHz. The task should start with a design study, of perhaps two years' duration, undertaken by a group with scientific and engineering competence. Such a group will require excellent leadership and should foster and develop cooperation among all those interested in the development of space radio astronomy.

The following guidelines are suggested for starting the work of the proposed group:

(i) Determine whether an antenna covering about 10 MHz to 500 kHz and having a beam area of about 100 square degrees at 1 MHz is feasible and meets the scientific needs.

(ii) Consider alternative design concepts, among which should be the possible use of a simple or compound interferometer system to give added resolution within the main beam area.

(iii) Consider and compare locations for such an instrument, particularly as between a high orbit (which would almost certainly be higher than a synchronous orbit) and a lunar base.

(iv) Consider this antenna as a possible payload for the Apollo Extension Systems.

Millimeter-Wave and Long-Wave Infrared Astronomy

Recommendation 4. The exploitation of millimeter-wave and far infrared observing capabilities from ground-based, aircraftborne, and balloon-borne facilities should be encouraged and supported by NASA.

Recommendation 5. Good telescopes for millimeter and far infrared wavelengths with apertures up to several tens of feet are feasible on Earth. However, much larger apertures of the order of 100 feet will be needed for future advances. Such instruments should avoid the gravity environment on Earth and avoid the Earth's atmospheric shielding. It is recommended that NASA make studies of the technology, feasibility, and cost of building and operating such telescopes in space. The launching and maintenance of such a telescope could possibly be a part of the Apollo Extension Systems.

Radar Astronomy

Recommendation 6. Since ground-based radar astronomy studies of the Moon and planets form a natural part of the NASA mission, they should be vigorously supported by NASA.

Recommendation 7. NASA should include in the Voyager series planetary orbiters containing a swept- or multifrequency radar system designed to detect and measure any planetary ionosphere and, from the radar scattering properties of the planetary surface, to derive information as to its nature and topography. Components of the same system can easily be used to make radiometric studies of the planetary surface and atmosphere.

In view of the great interest of the magnetosphere, ionosphere, and atmosphere of Jupiter, the first Voyager to approach that planet should carry a variety of radar and radiometric experiments designed to study these regions of the planet.

Recommendation 8. NASA should conduct, in the Voyager series, investigations of planetary surfaces and ionospheres by means of bistatic radar systems with one element of the radar on Earth and the other on the probe.

A similar type of experiment in which the probe, carrying either a transmitter or a receiver is sent on a trajectory such that the probe is occulted by the planet, is recommended as a powerful technique for the study of planetary atmospheres and ionospheres.

Recommendation 9. NASA should use lunar orbiter missions to test the instruments to be employed in planetary probes needed to conduct the experiments suggested in Recommendations 7 and 8, and also to add to our understanding of the nature of the lunar surface. These missions would be possible payloads for the Apollo Extension Systems.

Recommendation 10. A substantial extension of existing radio and radar observations of the solar corona and interplanetary medium is recommended. This requires that large ground-based antenna systems for transmitting and receiving be built, together with the necessary high-power transmitting equipment. New advances in antenna and computer technology should be fully exploited.

This equipment would make solar radar and radio astronomy observations. Suitable interplanetary probes carrying multifrequency receiving and transmitting equipment should be launched so that observations of the signals propagated between the ground station and the probes can be used to study the solar corona and the interplanetary medium.

The ground-based equipment should be regarded as a national facility, and its construction should be given high priority so that observations may be commenced during the coming sunspot maximum.

Miscellaneous

Recommendation 11. This Working Group recommends that NASA devote a much larger fraction of its resources to the construction of ground-based deep-space telecommunication terminals. The object is to increase the amount of information that will be returned from solar and planetary spacecraft now proposed.

2. LONG-WAVE RADIO ASTRONOMY

THE GOALS OF LONG-WAVE SPACE RADIO ASTRONOMY

The goals of long-wave radio astronomy may easily be defined. There is a region of the radio-frequency spectrum where the Earth's ionosphere acts as a shield to prevent ground-based observations of the radio emissions that reach the vicinity of the Earth. This region of the spectrum extends from frequencies of a few MHz downward to a lower frequency limit, where observations would prove to be impossible because of the effects of the interplanetary plasma.

The upper-frequency limit of this blocked spectral region varies as the electron density in the ionosphere varies. Thus, occasional observations may be made from Earth at frequencies as low as 1 MHz, as Grote Reber and G.R. Ellis have shown. Consistent ground-based observations on frequencies as high as 10 MHz are still, however, most difficult to achieve, partly because the critical frequency of the F region often exceeds this value and partly because, even on those occasions when radiation can penetrate the F region from directly above, long distance oblique incidence propagation is still possible. Thus many, ground-based radio transmitters can send strong interfering signals to a radio-astronomy antenna and prevent radio-astronomical observations from being made.

Above the F-region ionization maximum, however, the electron density steadily falls, to merge eventually with that of the plasma surrounding the Sun. In this region of space, long-wave radio astronomy is practicable. An antenna placed here can both receive signals from outside the Earth and, by the shielding of the ionosphere which is now between it and the ground, be freed from interference by man-made signals generated on Earth.

With such an antenna all the kinds of observations which radio astronomers have made from the Earth become possible for this new region of the spectrum. Eventually all these observations will be made; until then their relative scientific value can only be predicted. At present, space radio-astronomy experiments in this new spectral region are being chosen to combine the greatest scientific interest with the greatest instrumental simplicity.

The Brightness of the Radio Sky

As an example, all radio-astronomy experiments that have been flown in satellites have tried to measure the average brightness of the radio sky at a few places in the spectral range from 10 MHz to 725 kHz. Such experiments can be made with essentially nondirectional antennas and are thus simple to perform. Yet the scientific results are needed to understand the mechanisms of emission and absorption of radio waves within the galaxy. The spectrum of this galactic background radiation shows a continuous increase in sky brightness as the frequency is reduced until about 2-3 MHz is reached; here the spectrum turns back to lower values. This has been interpreted as the effect of absorption due to ionized hydrogen in the galaxy. Further experiments can test the truth of this interpretation and then, with improved angular resolution, study the distribution and properties of ionized hydrogen in the Galaxy.

Solar and Planetary Radio Emissions

Another goal, soon to be reached, is the extension into this new frequency range of the observations of radio waves emitted by the Sun and the planet Jupiter. Both of these are remarkable and sometimes energetic radio sources. Although radio waves from the Sun have been studied for twenty years and a wide variety of bursts observed and classified, it is true that the origins of these phenomena are still far from fully understood. Equally, the relations between these kinds of solar activity and terrestrial effects still present many problems. Study of the low-frequency solar bursts will give information on the plasma in which they originate; at these frequencies this is several solar radii out from the Sun. The radio waves from the undisturbed Sun also require study in this new spectral range.

Jupiter is the source of large radio bursts, already well observed from the ground in the frequency range from about 50 MHz to the Earth's ionospheric limit near 10 MHz. The planet also emits, probably because of a radiation belt system, strong nonthermal radio radiation at microwave wavelengths.

The currently accessible spectral range, within which the bursts may be seen, is clearly limited by the Earth's ionosphere at the lower-frequency end. An extension of observations here will help in understanding both how the bursts originate and perhaps also in showing whether Jupiter itself is subject to the same sort of Sun-induced effects as the Earth.

These solar and Jovian emissions are very powerful and can be studied with relatively simple satelliteborne antennas. Some degree of directivity is desirable, as that being designed into experiments such as the Radio Astronomy Explorer (RAE) series of satellites.

Galactic and Extragalactic Radio Astronomy

The goals described so far are important, and they are discussed first because they can be achieved with fairly simple systems. To carry out the most important studies in radio astronomy, the study of radio waves from all the diverse kinds of radio sources already known, requires a major instrumental step. If that step could be taken, so that reasonable directivity, gain, and ability to study polarization were available in a radio-astronomy experiment in space, a wide variety of new questions could be studied.

The sky background could be mapped with resolution adequate to see the distribution of ionized hydrogen in interstellar space. Measurement of its absorption may lead to new insight into the mechanisms of star formation. The contributions to the sky background from our own galaxy and from the extragalactic medium can be separated. Polarization studies may improve knowledge of the magnetic fields in interstellar space although, possibly, large Faraday rotation effects may make the results difficult to interpret.

Studies of the burst phenomena of the Sun and of Jupiter would be continued and extended. Similar phenomena from other planets may be discovered in the new spectral region.

As soon as the stronger of our known radio sources, among which are the supernova remnants, the strong radio galaxies, and the quasi-stellar sources (quasars), can be detected at lower frequencies and isolated from the galactic background, many fruitful researches should emerge.

The fluxes of these sources must be measured and their known spectra extended. In this low-frequency region it is possible that the radio-wave absorption, which occurs perhaps in or near the source or within our galaxy, can be studied and the effects of self- and galactic-absorption separated. Detailed data on the low-frequency spectra can give knowledge of one or more of the following characteristics of quasars and radio galaxies: the density of cosmic-ray electrons, the magnetic field strength, the plasma density, and the low energy cutoff of the cosmic ray electrons. In both these kinds of sources unknown yet highly efficient mechanisms of energy conversion are at work.

At high frequencies, measurements of polarization in sources already suggest the presence of a galactic or a source magnetic field. There is also some evidence that the radio flux from quasars varies with time. These phenomena should be studied at low frequencies also.

Although angular resolution will be practically difficult to achieve directly at these long wavelengths, the technique of getting effective resolution by observing sources as they are occulted by the Moon should be most valuable. Even on Earth, where the Earth-Moon source geometry is not subject to control, occultation work is of major value. In space it should be a most powerful technique. So also may be the application of interferometric or aperture synthesis techniques.

POSSIBLE LIMITATIONS AND DIFFICULTIES

Before proceeding to suggest experimental techniques in long-wave space radio astronomy, some limitations must be discussed.

Although removing the antenna from the Earth does remove the problem of ionospheric shielding, it still leaves the antenna embedded in an ionized medium. This can cause at least two fundamental but different kinds of lower limit. First, the antenna can be used only for frequencies significantly above the local plasma frequency. Second, it is known that the interplanetary medium is irregular in density; the irregularities can produce scintillations in the intensity and oscillations in the position of small-diameter radio sources.

The effects of the average density of the interplanetary medium depend only on the electron density. Something is known of this as a function of distance from the Earth; more knowledge is needed and will come from high-altitude rocket, probe, and satellite experiments. It has been estimated that the plasma limit for observations at a height of 6000 km could be 0.15 to 0.30 MHz, but might be as high as 0.50 MHz. Measurements on Elektron II showed 0.30 MHz at 10,000-15,000 km. As more results appear, the dependence on height of the lowest useful observing frequency will become better known. At present it seems reasonable to say that quite high orbits, probably above the synchronous orbit, may be needed for major low-frequency space antennas.

The effects of plasma irregularities on the scintillation of small-diameter radio sources have become the subject of a number of ground-based studies. The results already show that irregularities, of the scale of the order of at least 100 km in size, are distributed in the plasma and solar wind around the Sun out to well beyond the Earth. Studies of these irregularities are valuable scientifically and will continue; here we are mainly concerned with the limits they impose on the resolution of a long-wave radio telescope. These limits have been calculated approximately by W.C. Erickson and suggest that, for a frequency of 1 MHz (300 meters wavelength), observations with finer angular resolution than 24 minutes of arc would be impossible even with an antenna pointed away from the Sun. This limitation may be thought of as similar in effect to the atmospheric seeing limit for optical telescopes. It suggests that, to the best of our present knowledge, this effect will set a limit to the size of a long-wave space radio telescope beyond which it would not be useful to go in pursuit of better angular resolution. At 1 MHz the angular limit quoted above implies that the telescope should not be more than 43 km in aperture.

Radio Noise Limitations

Although the ionosphere will shield a space antenna from the Earth, such a telescope still may suffer from ionospheric effects. The USSR Elektron II and IV satellite experiments gave evidence of sporadic radio emission from the ionosphere. At 0.725 MHz this had the character of bursts of radio noise, though sometimes it was just a general rise of the noise level. The sporadic noise was sometimes two orders of magnitude above the cosmic noise being measured.

Man-made noise will be serious on those occasions when the ionospheric critical frequencies fall, but may also be serious if the use of the radio-frequency spectrum in space grows. This problem is already under study by a committee of the National Academy of Sciences; the present report will be brought to the attention of that committee.

The Immediate Future of Long-Wave Space Radio Astronomy

The likely scientific gains and the possible fundamental limitations on growth of this science were discussed by the Working Group and have been summarized above. The immediate plans in this country for space radio astronomy rest at present with the groups at the University of Michigan, Harvard College Observatory, and the Goddard Space Flight Center. Much of the scientific success soon to be achieved depends on the success of the GSFC Radio Astronomy Explorer (RAE) satellite program. In discussing this program, several points emerged:

(i) The results achieved so far demonstrate the potential value of the observations. Sky brightness can be measured. The dynamic spectra of solar, terrestrial, and planetary radio bursts should be observed during the program. The results in all these fields will be valuable scientifically and essential for the design of future space radio-astronomy experiments.

(ii) The present program is one which could be an excellent training ground for graduate students and recent graduates. However, lead times for approval of programs for the RAE series of satellites must be reduced not only to attract and keep good students but also to allow for a rapid feedback of new techniques and results into new experiments.

(iii) Rocket probe experiments will continue to be important as rapid tests of new techniques and ideas. There is no vehicle that could fill the gap between the Astrobee 1500 and the Scout (for instance, capable of taking 50-70 lb of payload to 20,000 km) except perhaps the Air Force Blue Scout, Jr.

(iv) The RAE satellites are very suitable for observing the phenomena of solar bursts, using either a swept frequency or a multifrequency radiometer. The next solar maximum falls at about the time when it should be possible to have such instruments flown; plans should be made soon to fly solar radiometers at or near the solar maximum.

(v) The rate of launching RAE satellites could grow to be about one per year and the series might well continue for some time.

The group, in making Recommendation 1, had these considerations in mind.

Recommendation 1. The Radio Astronomy Explorer (RAE) satellite series should be continued and expanded. In addition to having inherent value it is a prerequisite for the instrumental developments proposed in Recommendations 2 and 3. We foresee that, when running at its peak rate, launches of at least one a year will be needed. Universities, NASA, and other research institutions should be encouraged to develop payloads and every effort made to shorten the lead times required for approval of a program.

The importance of solar radio astronomy during the maximum phase of solar activity requires that at least two RAE satellites instrumented for long-wave solar radio observations should be flown near sunspot maximum to provide data on the low-frequency characteristics of solar radio bursts.

Large Space Radio Telescopes

If radio astronomy in the long-wave part of the spectrum is to have the capability of observing reasonable numbers (perhaps a hundred or more) of discrete radio sources, antenna systems of large size will be needed. As an example, at a frequency of 1 MHz (300 meters) a filled-aperture antenna needs to be 10 km across in order to give a beamwidth of 2 degrees. Antennas of this size would still have a very poor angular resolution when compared with most present-day ground-based instruments. Since the flux from radio sources is high in this long-wave region the large aperture required for resolving power need not be fully filled to provide a large collecting area. The degree of filling required depends on the type of observation.

The long wavelengths also give considerable relief in the structural and dimensional antenna tolerances which are needed. These, as in ground-based telescopes, need to be maintained to only about $\lambda/16$. The pointing precision, or knowledge of the direction in space toward which the radio beam of the telescope is pointed, again needs to be known to only about one-twentieth of the radio beamwidth.

Since any antenna system used at these wavelengths is always receiving signals from the sky background, which has a brightness temperature of the order of $10^{7\circ}$ K, the requirement for low-noise radiometers is not stringent. Problems connected with the behavior of the antenna impedance, pattern, and collecting area, both because of the need for operation over a wide frequency range and because of its immersion in a plasma, are quite serious. They are, however, presently under study and are partially understood.

The mechanical problems of designing, launching, unfolding, or erecting, and using a large radio antenna in space could be discussed by the Group only in general terms. It seems certain that a structure made basically of wires held in place by some stabilizing system could meet the needs and be practical. Stabilization with a few small vehicles carrying thrust devices or possibly with gravity gradient methods might work.

Any antenna system of large size could be used over a period of several years for many different programs. It would be very reasonable to use the ability of men to erect, visit, adjust, modify, or repair the system from time to time.

These considerations led the Group to formulate Recommendations 2 and 3:

Recommendation 2. A space radio telescope of aperture of the order of 20 km appears to be close in size to the ultimate for observations between a few MHz and a few hundred kHz. With larger apertures, effects due to irregularities in the interplanetary medium will, according to our best present knowledge, become the factor which limits the resolving power of the telescope. We recommend that the National Academy of Sciences appoint a panel now to study possible conceptions of such a radio telescope and to initiate studies of the scientific and engineering problems connected with its construction.

Recommendation 3. Since the study proposed in Recommendation 2 is for an ultimate space radio telescope, work should be started now which will lead to the use in space, within about ten years, of a high-resolution broadband antenna system for radio-astronomical observations over the frequency range 10 MHz to a few hundred kHz. The task should start with a design study, of perhaps two years' duration, undertaken by a group with scientific and engineering competence. Such a group will require excellent leadership and should foster and develop cooperation among all those interested in the development of space radio astronomy.

The following guidelines are suggested for starting the work of this group:

(i) Determine whether an antenna covering about 10 MHz to 500 kHz and having a beam area of about 100 square degrees at 1 MHz is feasible and meets the scientific needs.

(ii) Consider alternative design concepts, among which should be the possible use of a simple or compound interferometer system to give added resolution within the main beam area.

(iii) Consider and compare locations for such an instrument, particularly as between a high orbit, which would almost certainly be well above the synchronous height, and a lunar base.

(iv) Consider this antenna as a possible payload for the Apollo Extension Systems.

It seemed reasonable to start planning for the instrument that Recommendation 2 suggests: a telescope whose performance would be approaching the limit set by irregularities in the interplanetary medium. It might have a beam area of only a few square degrees at 1 MHz, and thus would be hundreds of square kilometers in area. It would be a long-term national effort, probably requiring special thought on the problems of administration and management as well as on the scientific and engineering aspects. For these reasons, a start from a National Academy group seemed logical.

The antenna suggested in Recommendation 3 is still large, but probably within the abilities of an effort which drew on the best competence of those already in or willing to enter the field. It is to be hoped that proposals for a start on the instrument described in Recommendation 3 might be made to NASA without any more formal planning, and that a suitable proposal for a design study might receive support.

3. MILLIMETER-WAVE AND LONG-WAVE INFRARED ASTRONOMY

TECHNIQUES AND LIMITATIONS

The extension of astronomy into the wavelength region between 1 cm and a few microns (1 micron = 10^{-4} cm) has in the past been limited by lack of detectors of high sensitivity and by the obscuration of the Earth's atmosphere. In the last few years, the development of better detectors has allowed some work to be done in the 1-mm and 11-micron regions (using a supercooled germanium bolometer) and at 8 mm and 3.5 mm with improved radio techniques. Although the atmosphere of the Earth is an absorber throughout the whole wavelength range, there are regions of the spectrum where the absorption is low enough to permit observations from suitably chosen sites on the ground. So far, these observations have been mainly at 8 mm, 3.5 mm, 1 mm, and 11 microns.

Radiometer Development

The germanium bolometer, as developed by F.J. Low, has proved to be a very valuable detector over the wavelength range from 1 mm to 11 microns. Although it is not the only detector of high sensitivity, it is probably the best, and its performance is summarized below to show what it can now achieve.

The device is basically a simple bolometer, which detects radiant energy by absorbing it. The temperature of the bolometer element rises and thus its resistance changes by an amount depending on the incident radiation. The novelties of the new bolometer are in the choice of the bolometer material, the choice of a very low (liquid helium) working temperature with its very low noise level and enhanced change of resistance with incident energy, and in the techniques for making the whole detector. Problems of excluding unwanted energy and of defining a bandwidth of the device have been overcome.

At 1 mm the bolometer has been used on telescopes as large as the 200-inch Palomar instrument. The worst atmospheric effect is the noise fluctuation arising from water vapor irregularities in the air. This effect has been considerably reduced by using a technique that switches the telescope beam on and off the object being observed. The path through the disturbing part of the atmosphere hardly differs in the two beam functions, so that use of a switched radiometer technique can reduce the atmospheric effects to a small value by cancellation. The method is, of course, of no value for observations of extended sources.

The use of this technique at 1 mm with the best bolometers has reduced the radiometer fluctuations, measured as a root-mean-square (r.m.s.) input temperature fluctuation, to 15×10^{-3} °K with a time constant of 10 sec. The bandwidth of the system was about 10%. (In frequency 1 mm corresponds to 300 GHz.) The total absorption in a dry atmosphere (total precipitable water = 2 mm) at 1 mm is 15%. Using such a bolometer on the 200-inch telescope (which was designed for optical work and is therefore not a very efficient reflector at 1 mm) gives in practice a limit for the observable flux from a radio source at 1 mm of about 10 flux units [1 flux unit = 10^{-26} watts $(\text{meters})^{-2}(\text{Hz})^{-1}$].

Further development of bolometers can be foreseen such as reducing the working temperature from its present value, about 2°K, to perhaps as little as 0.25°K. This, together with other improvements, may increase the over-all sensitivity by a factor of 10.

The bolometer performs very well in a radiometer, partly because it accepts such a wide band of frequencies. Nevertheless, it can be used for spectral studies provided that the features being studied are not too narrow and that signal levels are not too low.

With techniques that are somewhat more conventional, at least to the radio astronomer, most work at 8 mm and 3.5 mm has been done with crystal mixer radiometers having total bandwidths in the 1 GHz region. With such radiometers, RMS fluctuations of about 0.1°K using a 10-sec integration time can be achieved.

Although it is tempting to assume that low-noise devices such as masers, parametric amplifiers, or perhaps tunnel diodes could be used in the millimeter-wave region, the technological difficulties have so far prevented any progress.

Ground-Based Antennas

The antennas are listed in Table 1, together with the shortest wavelengths at which they have been used or appear to be usable, to show the extent of millimeter-wave antenna technique.

Table 1. Reflectors Used for Millimeter-Wave Radio Astronomy

Reflector diameter	Location	Shortest wavelength at which it should be usable
16 ft	U. of Texas Austin	3 mm
15 ft	Aerospace Corp., El Segundo, Calif.	1 mm
200 in	Mt. Palomar	Optical
36 ft	For National Radio Astronomy Observatory, on Kitt Peak, Ariz.	Under construction; limit should be 1 mm

Observations at 11 microns have been made using a variety of reflectors, including the 84-inch at Kitt Peak National Observatory and the 200-inch at Mount Palomar.

RECENT RESULTS

The past three years have produced many new results. These are summarized below because of their importance in showing the directions of future research.

The Sun

The Sun shows blackbody temperatures of 5800°K at 1 mm and 6400°K at 3.5 mm. There is no limb brightening or darkening at 1 mm, and no signs of solar activity have yet been seen at 1 mm. At 3.5 mm, active regions have been observed and on four occasions a region showing enhanced 3.5-mm radiation has later been the source of a solar flare. Some regions that are 200-300°K cooler than the rest of the Sun appear to be areas in which the magnetic field is low or vanishes.

The Moon

The Moon has been studied extensively at all accessible wavelengths, during entire lunations and at 3.2 mm during a total eclipse. The results are numerous and show temperature differences associated with surface features and differences in temperature behavior at different parts of the surface. The observations give information which depends for its interpretation on a number of physical properties of the surface materials and thus cannot alone uniquely determine any single one of these properties.

The Planets

Venus, Mars, Jupiter, and Saturn have been observed at 1 mm and at 11 microns. The blackbody disk temperature of Venus is now well established over a wide frequency range.

Radio Sources, Quasi-stellar Sources (Quasars), and Red Stars

The Omega Nebula (Messier 17) has been detected with the 200-inch telescope at 1 mm. The Crab Nebula (radio source Taurus A) has been observed at 3.5 mm.

Quasars represent a very important task for millimeter-wave and long-wave infrared observations. The B component of 3C 273 has been observed in the 1-11 micron range. The spectrum may show an irregular behavior. It has also been seen with the 200-inch telescope at 1 mm and the flux appears to be higher than the value suggested from extrapolation of the known parts of the spectrum.

Since some quasars vary in optical intensity, and at least four show variability at radio wavelengths, they are of great interest for study in this 1-mm to 11-micron region. The spectra of quasars must also be determined in this very short-wave end of the radio spectrum.

The present results are somewhat tentative but of great interest. They raise many questions and suggest many new experiments. Measures of diameter and structure by interferometers or lunar occultations might eventually be possible even at millimeter wavelengths. Polarization measurements are of the greatest importance and are needed to understand the geometry of the magnetic fields in the quasars and radio sources. Possible irregularities or abnormalities in spectra should be looked for.

A few stars with surface temperatures as low as 700°K have been observed in the long-wave infrared region. Perhaps 200 such cold objects have been found photographically. Betelgeuse has a large emitting envelope at 11 microns; so do other M-type stars. The neon emission line at 12.8 microns has been observed and identified.

FUTURE PLANS

Ground-Based and Airborne Experiments

Ground-based research will grow considerably within the next few years. Observations can be made of bright objects with dishes carried by airplane or balloon to heights of 45,000 feet or above in some spectral ranges such as 100 to 300 microns. Such observations of the Moon may lead to a knowledge of the variations of the dielectric constant of the surface with frequency. The value of the scientific results together with the comparative simplicity of the techniques led to the adoption of Recommendation 4.

Recommendation 4. The exploitation of millimeter-wave and far-infrared observing capabilities from ground-based, aircraftborne, and balloon-borne facilities should be encouraged and supported by NASA.

Space Experiments

The possibilities and difficulties of a large millimeter-wave telescope in space were discussed at some length. No millimeter-wave telescope larger than 16 feet in diameter has yet been proved on Earth, but one of 36 feet is being built. This is probably about the limit of size at which uncompensated structures can be built on Earth and still maintain reasonable shape as their orientation with respect to gravity is altered. The thermal gradient problems will also prove to be close to the limit of manageability, even if such a telescope is protected by an astrodome.

A space radio telescope for these wavelengths would still be a parabolic dish. It might be made as a complete surface or, as is done with the elements of the Hanbury-Brown and Twiss stellar interferometer, as a set of independent reflecting surfaces with a common focus. To give a suitably large step in gain over ground-based instruments, a diameter of as much as 100 feet should be considered.

Problems of gravity deflections in space do not exist. There are problems of making such a dish on Earth and erecting it in space, since positional accuracies of the surface of 50 microns or better (one or two-thousandths of an inch) are needed. Such a dish would be a diffraction-limited antenna at 1 mm and a valuable energy collector in the micron range.

Thermal deflections will present a much more serious problem. Even the best thermal stabilizing paints now known will probably not solve this problem. It may be possible, however, to find techniques whereby thermal equalization over the structure to better than 1°C (which is about what is needed for a steel structure) could be achieved.

If such an antenna were built, it could be placed in a low orbit below the ionosphere and above the main atmosphere. Pointing precision of about 1 sec of arc would be needed. The problems of maintaining radiometer performance suggest that cryostats capable of holding the temperature to about 0.25°K would be needed.

The problems of servicing and maintaining equipment and probably the task of erection, suggest that such a system would require regular visits by men. The antenna would be used on a variety of programs over a period of several years.

Since such an antenna represents a considerable technological step, yet one which is certainly only of the same order of difficulty as those which are being solved in the Apollo program, it seems reasonable to start studies for such a telescope. These considerations led to Recommendation 5.

Recommendation 5. Good telescopes for millimeter and far-infrared wavelengths with apertures up to several tens of feet are feasible on Earth. However, much larger apertures of the order of 100 feet will be needed for future advances. Such instruments should avoid the gravity environment on Earth and avoid the Earth's atmospheric shielding. It is recommended that NASA make studies of the technology, feasibility, and cost of building and operating such telescopes in space. The launching and maintenance of such a telescope could possibly be a part of the Apollo Extension Systems.

4. RADAR ASTRONOMY

THE PRESENT STATUS AND RECENT RESULTS FROM GROUND-BASED WORK

Ground-based radar astronomy observations have now been made in some detail on quite a number of the objects within the solar system. Such observations will certainly continue to expand, although there does not seem to be much hope of extending them to the outermost planets and there is certainly no present hope of extending them to interstellar distances.

The techniques are well established and well understood. The refined systems which have been used within the past few years have brought the Sun, the Moon, and three planets under good observation. These techniques allow information to be collected about a number of physical properties of the object being studied. Table 2 shows a summary of the measurable quantities and the information which can be derived from them.

All these techniques have been applied to the Moon, and the detailed information now available is very considerable. Mercury, Venus, and Mars have been detected and studied. One group claims to have detected Jupiter, but another group working at a much lower frequency has not confirmed this result. The rotation of Mercury has recently been measured by radar; the result, a period of 60 ± 5 days, is very different from the previously accepted optically based value of 88 days. Observations of the Sun fall into a special class, since it is both a very noisy object and also one whose behavior may be expected to be extremely variable. Observations since 1961 have been made, using a special radar system on 38 MHz at El Campo, Texas, and already show important correlations between radar cross section and solar activity. Sufficient sensitivity is available to get some range-Doppler results, but the system (although very large physically) has neither enough angular resolution to resolve the Sun nor the steerability to track the Sun.

Table 2. Information Available from
Radar Observations of the Sun, Moon, and Planets

Property Measured	Gives Information on
Total delay time of the echo	The orbit, for a Moon or a Planet
Doppler shift of returned signal	Planetary radii, movements of the plasma envelope; propagation within the planetary atmosphere
Total returned power	Reflectivity of the surface; composition of the surface
Dispersion in the delay time of the echo	Roughness of the surface, slopes of elementary parts of the surface
Degree of polarization of the returned echo	Roughness of the surface
Dispersion in frequency of the echo	Roughness of the surface; the rotation of the planet
Delay time and Doppler shift of frequency together	Range-Doppler method for getting detailed maps at high resolution of the surface scattering properties

These ground-based radar-astronomy techniques can and will be improved. Greater signal-to-noise levels at the receiver have to be obtained. This may be achieved with monostatic systems on or near the Earth by building larger antennas, by using higher-powered transmitters and, to some extent, by increasing the radio frequency at which the observations are made. The increases which are required may be achieved on the ground; there seems little need or hope for achieving them in a radar astronomy system both terminals of which are erected in space in the vicinity of the Earth.

The results which have been obtained so far, particularly on the properties of the Moon, have been directly useful to the NASA mission. It may be expected that the planetary results also will have a direct value. These were the considerations which led to the adoption of Recommendation 6.

Recommendation 6. Since ground-based radar astronomy studies of the Moon and planets form a natural part of the NASA mission, they should be vigorously supported by NASA.

SPACE RADAR ASTRONOMY

Although there is little to be gained in space radar astronomy by putting both the transmitter and receiver in space near the Earth, very considerable advantages arise if either or both is carried to the vicinity of the planet or other object being explored. A convenient distinction in radar may be made by calling "monostatic" the case where transmitter and receiver are close together, and "bistatic" when they are widely separated.

Monostatic Space Radar Astronomy

The power received in any monostatic radar system depends on the inverse fourth power of the range to the target. Thus for planetary targets, very large antennas and transmitters have been built on the Earth to bring the signal from the target above the receiver detection limit.

However, if both terminals of such a radar-astronomy system are placed near the planet to be studied, only quite modest powers and antenna sizes are needed to get valuable results. For example, a swept-frequency or multifrequency radar system, resembling the topside sounder Alouette but operating over a wider frequency range, would be an excellent instrument to be placed on a planetary orbiter. Such a device would detect the existence of an ionosphere and measure the electron distribution as a function of height. Such observations give valuable information concerning the density and nature of the atmospheric constituents. In addition, if the higher frequencies employed do penetrate the ionosphere then the same instrument gives a considerable amount of information about the nature and topography of the planetary surface.

Such experiments are of obvious value when applied to planets such as Venus and Jupiter. They have the additional advantage that if the radar equipment is properly designed, it can very simply be used to make radiometric measures of the surface as well. Although the Voyager program is still only in its early stages, from estimates of the weight and complexity of this type of equipment the suggestion is made that such a system could be well within the limits of Voyager's capability.

A Possible Specific Mission to Jupiter

One possible Voyager mission to Jupiter was discussed. The remarkable magnetosphere, ionosphere, and atmosphere of Jupiter is a very important, and possibly unique, field for study. A mission to do this, in addition to making measurements of the interplanetary plasma and magnetic field, should carry radar and radiometric probes, for example:

(1) A topside swept-frequency 1-20 MHz sounder.
(2) Downward-looking millimeter and decimeter-wave radiometer systems.
(3) A swept-frequency decameter-wave radio monitor.
(4) A very low-frequency radio receiver.
(5) Equipment for observing from the Earth occultations of the vehicle by the planet. Three radio frequencies in the 50-5000 MHz range should be chosen.
(6) Optical photometers probably in the H-α line, to observe possible night-side auroral activity.

Such a probe would be ideal as an orbiter in a near-polar orbit; a fly-by mission would be of some, but less, value.

These considerations led to Recommendation 7.

Recommendation 7. NASA should include in the Voyager series planetary orbiters containing a swept- or multifrequency radar system designed to detect and measure any planetary ionosphere and, from the radar scattering properties of the planetary surface, to derive information as to its nature and topography. Components of the same system can easily be used to make radiometric studies of the planetary surface and atmosphere.

In view of the great interest of the magnetosphere, ionosphere, and atmosphere of Jupiter, the first Voyager to approach that planet should carry a variety of radar and radiometric experiments designed to study these regions of the planet.

Bistatic Radar Experiments

The use of only one of the elements of the radar near the target also allows higher signal levels to be achieved in the system. It has an additional difference from the monostatic case, which is often of value, in that the scattering geometry changes from the simple back-scattering case. Thus information different from, and of greater variety than that of the monostatic experiment is obtained.

The close approach of one of the elements of the system to a planetary surface allows a very considerable gain in angular resolution, so that such experiments on planets could be expected to give results as good in resolution as those now achieved from the Earth on the Moon by range-Doppler techniques.

The same techniques as are used in bistatic radar can be applied in the case where the exploring probe is occulted by the planet. Observations of the phase and amplitude of signals from the probe can yield a complete profile of electron density in the planetary ionosphere and also give information on the density of the neutral atmosphere. At least two radio frequencies, with suitable modulation techniques, should be employed simultaneously in such measurements.

Such bistatic radar experiments can, if they employ two suitably chosen and modulated frequencies, yield information about the integrated electron density between the Earth and the probe and, in addition, can be used to refine trajectory information.

A similar refinement, useful for the control of terminal maneuvers, can be provided from the fact that both mono- and bistatic space radar systems give good measures of the distance of the probe from the planet.

So far, only relatively simple experiments of this kind have been conducted. The Mariner IV fly-by of Mars was a particularly successful example of the occultation type experiment. Much more information can be obtained from two frequencies. Experiments of the kind to fly on Pioneer out to 1 or 1.5 AU, in which phase-path and group-path are measured on two different frequencies, will measure the plasma electron density and its variations in interplanetary space with great precision.

These considerations led to Recommendation 8.

Recommendation 8. NASA should conduct, in the Voyager series, investigations of planetary surfaces and ionospheres by means of bistatic radar systems with one element of the radar on Earth and the other on the probe.

A similar type of experiment in which the probe, carrying either a transmitter or a receiver is sent on a trajectory such that the probe is occulted by the planet, is recommended as a powerful technique for the study of planetary atmospheres and ionospheres.

Lunar Orbiters

The surface of the Moon has already been well studied from the ground, and such studies, together with more direct observations which will certainly take place, have provided and will continue to provide much knowledge of the lunar surface. However, if monostatic or bistatic space radar systems are used to observe the Moon, they will first add considerably to our knowledge of the lunar reflection properties and second, perhaps more important, they can serve both as a test bed for equipment and as a source of ideas for the planetary exploration program.

These views are summarized in the following recommendation:

Recommendation 9. NASA should use lunar orbiter missions to test the instruments to be employed in planetary probes needed to conduct the experiments suggested in Recommendations 7 and 8, and also to add to our understanding of the nature of the lunar surface. Instruments for these missions could be possible payloads for the Apollo Extension Systems.

Special consideration was given to the problems and the future of solar studies. These have been referred to at various places in this part of the report and are summarized in the Appendix, Section 6. This paper was the result of a meeting at which members of the solar astronomy group, together with Dr. J. James from the M.I.T. El Campo station and Dr. K. Bowles from the Bureau of Standards, discussed the future requirements of this branch of the science. These discussions were led by Dr. A. Maxwell. As a result, Recommendation 1 (already quoted) includes the reference to the needs for solar observations in the RAE satellite series, and Recommendation 10 was agreed upon by the Working Group.

Recommendation 10. A substantial extension of existing radio and radar observations of the solar corona and interplanetary medium is recommended. This requires that large ground-based antenna systems for transmitting and receiving be built, together with the necessary high-power transmitting equipment. New advances in antenna and computer technology should be fully exploited.

This equipment would make solar radar and radio astronomy observations. Suitable interplanetary probes carrying multifrequency receiving and transmitting equipment should be launched so that observations of the signals propagated between the ground station and the probes can be used to study the solar corona and the interplanetary medium.

The ground-based equipment should be regarded as a national facility, and its construction should be given high priority so that observations may be commenced suring the coming sunspot maximum.

5. RELATED SUBJECTS

TECHNIQUES

During the work of the Group several items where technical advances are needed were discussed. They are collected here for convenience.

Long-wave antenna structures. The antennas for long-wave radio astronomy will fairly certainly be systems of light conductors extended over large distances. The problems of erection, stabilization, and pointing control clearly are not well defined or understood.

Antennas for 10 centimeters to a few meters. Although there is no immediate desire to see large antennas for this wavelength region in space, the studies for ground-based structures of 600 feet or more which are now starting might well indicate ways of building even larger space structures. Similarly, work on the general concepts of large steerable antennas in space could well assist the designing of the ground-based instruments. It would be well to try to establish a sensible boundary line between building on the ground or in space.

Millimeter-wave antennas. The main problems here would appear to be in the control of thermal differences, in achieving pointing control and in finding erection techniques capable of obtaining the high dimensional accuracy needed.

Novel antennas. Ideas by which resolution may be obtained without large size may, in some cases, be physically correct. Studies of such systems, which, for example, would include nonlinear elements, could be valuable.

Radiometer techniques in the millimeter-wave region. Thermal detection radiometers, utilizing the germanium bolometer or one of several other possible low-temperature devices, provide high sensitivity and broad bandwidth at millimeter and submillimeter wavelengths. Further improvements in existing designs for ground-based observations and adaptations to the requirements of space telescopes should be undertaken. In addition to the broadband radiometry, it will be necessary to have spectrometers that can provide a wide range in resolution and spectral coverage. Furthermore, the need to obtain the most information possible from the spectrum at $\lambda 0.02$ mm to 2.0 mm will require the development of spaceborne cryostats which maintain temperatures as low as 0.25°K.

THE SIZE OF NASA GROUND-BASED ANTENNA SYSTEMS

Although this subject is clearly not within the scope of this Working Group, the optimum size of NASA ground antenna facilities for transmission of commands and reception of telemetered data was considered. The data rates and signal levels available with the currently available or planned antennas are low, and especially low for planetary missions. The cost of the entire set of missions is large. It appears probable that in future missions the gains from using larger ground-based antenna systems would outweigh the relatively small extra cost.

This led to the following recommendation:

Recommendation 11. The Working Group recommends that NASA devote a much larger fraction of its resources to the construction of ground-based deep-space telecommunication terminals. The object is to increase the amount of information that will be returned from solar and planetary spacecraft now proposed.

THE APOLLO EXTENSION SYSTEMS (AES)

It will be evident from the report that, starting from a consideration of the scientific needs, the Group has suggested several projects in radio and radar astronomy. None of these can be defined closely enough to know whether it would fit with the Apollo Extension Systems, but it seems possible that the antennas proposed for long-wave work (Recommendation 3) and for millimeter-wave work (Recommendation 5) are of the size which would need AES. Similarly, lunar orbit missions in AES could with value carry the space radar astronomy experiments suggested in Recommendation 4.

There are useful millimeter-wave experiments that could be done before the construction of a large dish, which would need collecting areas only 10 or 20 square feet in size, capable of working down to 20 microns. These experiments, though of priority below those suitable for the large millimeter-wave dish or the long-wave antenna, would nevertheless be valuable.

LUNAR EXPLORATION SYSTEM AFTER APOLLO (LESA)

The Group did not find it possible to come to any significant conclusions on LESA. From a strictly scientific point of view, despite the possible attraction of the back of the Moon as an interference-free site, all the future plans that the Group considered appeared to be more easily and better done in orbit than on the Moon.

Another point of view is possible: if experiments are to be conducted from the Moon, what should be done there? When read from this point of view, the LESA reports were less strongly criticized; some of the Group nevertheless considered that the reports underestimated the size of antennas needed for long-wave radio astronomy. The reports also did not give enough consideration to the importance of radio-astronomy observations in the wavelength range below 1 mm.

The present report of the Group may be of some help in developing the LESA plans. The Group has tried to say what should be done. If in fact for other reasons any of the experimental objectives that the Group has suggested could be more easily met by a radio observatory on the Moon, then the choice is easy and obvious. But if there are no oth reasons which press for the use of the Moon as a radio-astronomical base, the Group would wish the main effort to go into designing the experiments to be put in the most suitable orbits. Studies of the lunar environment for its mechanical suitability should be made. Similarly, whether or not it is required for radio astronomy the control of the use of the radio spectrum should be well planned and well executed. This, of course, we recognize, after seeing the situation on Earth, as a task which is certainly beyond NASA and perhaps beyond the abilities of the human race.

APPENDIX 1: LIST OF PARTICIPANTS

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APPENDIX 2: SOLAR RADIO AND RADAR ASTRONOMY

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NEW NATIONAL FACILITIES

The technology is at hand to mount a major program for combined active and passive radio-wave probing and observation of the solar corona and interplanetary medium, from the lower corona to well beyond the orbit of the Earth.

There would be four principal categories of investigation in such a program. They are based on:

(i) Ground-based solar radar, with separated transmitting and receiving installations.
(ii) Ground-to-space and space-to-ground transmission experiments utilizing various solar and deep space vehicles.
(iii) Sweep-frequency observations of solar radio noise bursts.
(iv) Observations of the scintillations of cosmic and space-probe radio sources caused by plasma irregularities in the solar wind.

Various research groups have already expressed interest in substantial expansion of programs in each of these fields. It is suggested that a major effort should now be made, possibly leading to construction of new national facilities, to provide extensive new ground-based equipment for these programs. This should be used cooperatively by various university and government research groups. Such a combination of programs is important on both scientific and practical grounds, for the following reasons:

(i) The scientific objectives of these four areas are intimately related, and this combination of observations would represent a closely coordinated study of quiet and active solar and interplanetary phenomena of a kind that has not been possible heretofore.
(ii) There is very substantial overlap of equipment needs, particularly for the very large and costly antennas and powerful transmitters.

SOLAR RADAR

The feasibility of using ground-based radar as a new technique to study the Sun has been conclusively demonstrated. Routine observations since 1961 have produced data on the distribution in range and Doppler frequency of returned energy from the echoing regions in the solar corona.

The radar Sun is highly variable from day to day, with its long-term average cross section being closely correlated with sunspot number.

Range-Doppler information has been interpreted in terms of large mass motions in the corona, and a general outward flow of gas whose velocity increases with distance from the photosphere.

Present studies at 38 MHz at El Campo, Texas, are severely hampered by limitations in system sensitivity (only one out of every four 16-minute echoes contains sufficient energy to make a range-Doppler display), antenna beam steerability (only one measurement is obtained per day), and angular resolution (the radar Sun is not resolved in angle).

Considerably more system sensitivity (15 to 20 dB), beam steerability (sufficient to track the Sun for about 6 hours daily), and angular resolution (about 10 min of arc) are required to realize the potentialities of radar for detailed studies of the quiet and active Sun in the echoing regions from perhaps 1.2 to 3 solar radii. A transmitting site for floodlighting the Sun, and receiving antenna array at a separate site for angular resolution, would be required. Such an upgraded system will require no new technology.

PROPAGATION PROBES (BISTATIC RADAR EXPERIMENTS)

Radio-wave propagation between the Earth and space vehicles provides a method of making sensitive observations of the radial distribution, irregularities, and dynamics of the interplanetary plasma, from the inner corona out to many AU. Current and prospective programs incorporating this type of experiment include the Pioneer and Sunblazer series of spacecraft, and the Voyager series of planetary spacecraft. In the relatively modest Sunblazer project, pulsed transmissions from the spacecraft to the ground are employed, while the first Pioneer propagation experiment provides for powerful ground transmissions to be received in the spacecraft where the measurement results are encoded onto the telemetry channel. Each approach has its advantages and disadvantages, and it is not yet clear which will be favored for more advanced missions.

Ground-based equipment is at hand for the first Pioneer experiments, and relatively inexpensive phased arrays are proposed for the first Sunblazer. But for future missions designed for more detailed measurements, and for experiments conducted over considerably greater distances, larger antennas and more powerful transmitters will be required. Major components of the ground-based facilities suggested above for advanced solar radar observations could be used to fill this need.

Recommendation. The Committee recommends a substantial extension in existing radio and radar observations of the Sun. This will necessitate the construction of large, new, versatile instruments, exploiting to the full recent advances in antenna techniques and computer technology. Such new equipment should be regarded as a national facility, and its construction should be given high priority, so that observations may be started during the coming sunspot maximum.

SOLAR RADIO EXPLORER SATELLITES

Observations of the dynamic spectra of solar radio bursts should be extended to wavelengths beyond the long-wave limit imposed by the terrestrial ionosphere. The immediate observational requirements in this spectral region (approximately 0.25 - 16 MHz) can be met with a satellite of the Explorer size. The survey character of these observations suggests that a series of several satellites will be required to provide proper coverage during the solar cycle.

Recommendation. This Committee recommends that a series of Solar Radio Astronomy Explorers be flown during the coming sunspot maximum, to provide data on low-frequency characteristics of solar radio bursts.

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V

X-Ray and Gamma-Ray Astronomy

1. INTRODUCTION AND SUMMARY OF RECOMMENDATIONS

X-RAY AND GAMMA-RAY ASTRONOMY: AN INTRODUCTION

If one rates scientific discoveries by the novelty of the phenomena they disclose, then certainly the observation of strong x-ray sources beyond the solar system has been the outstanding discovery of space astronomy to date. In three years, the observations have progressed from the first evidence of a localized flux to the detection of about a dozen discrete sources. It took radio astronomy a dozen years to progress from Jansky's original discovery to the detection of the radio source Cygnus A.

The x-ray flux in the 1 to 10 Å region from the strongest of the x-ray sources is about one-tenth of that from the quiet Sun. The fact that none of the celestial phenomena known previously had led astrophysicists to predict the existence of x-ray sources even remotely approaching the strength of those observed justifies the expectation that x-ray astronomy will play a fundamental role in advancing our understanding of the Universe.

Most, if not all, of the observed x-ray sources appear to be within the galaxy. Present plans to extend the observations include the use of larger rockets and balloons which will carry larger and improved detectors, and satellites which will provide longer observing times. Both approaches will almost certainly bring a large number of weaker galactic sources within range of study and permit the observation of strong extragalactic sources. A diffuse x-ray flux, which may be the integrated effect of the contributions of all external galaxies, has already been observed. Every ingredient exists in the young field of x-ray astronomy to guarantee its development in a manner that may soon lead to results comparable in importance to those of radio astronomy.

The future of gamma-ray astronomy (above 1 MeV) is not as clearly defined as x-ray astronomy at present, because as yet no cosmic gamma-ray sources have been detected with any certainty. The first observations of gamma rays from space vehicles have been sensitive enough to discover surprises of the magnitude revealed by x-ray astronomy, but similar discoveries have not turned up. There is some evidence from satellite observations, however, for the existence of a diffuse gamma-radiation background from space. Radio-astronomical data provide almost certain evidence that discrete sources of gamma rays exist in the 100-MeV range. Current theories of nucleogenesis indicate that there should be significant amounts of gamma line emission from the decay of heavy radioactive elements, even in a supernova remnant as old as the Crab Nebula. But the gamma-ray flux from these processes is estimated to be two or more orders of magnitude less than would be detectable by instruments already flown or scheduled for flight. The fundamental knowledge to be gained from studies of gamma-ray processes is so important that every effort must be directed toward achieving the necessary levels of detection sensitivity.

Astronomy reveals a host of phenomena in which nature provides remarkably powerful mechanisms for the acceleration of elementary particles to relativistic velocities. Through x-ray and gamma-ray astronomy we have an indirect means of detecting the existence of these energetic particles and unraveling the processes that produce them. The charged particles themselves travel through vast regions of space but their directions are so altered by collisions with magnetic fields and matter that all trace of their origin is destroyed. On the other hand, the x-rays and gamma-rays that have their origins in the same primary events travel in straight lines from source to observer and may survive billions of years without deflection.

X-ray and gamma-ray astronomy provide tools for probing many of the most fundamental problems of cosmology. These include the mystery of the origin of cosmic rays, the density of cosmic rays in galaxies and in the Universe, the strength of galactic and intergalactic magnetic fields, the hypothesis of continuous creation of matter, and the temperature, density, and composition of galactic and intergalactic matter. Already x-ray astronomy brings into question some of our fundamental concepts of stellar evolution leading to the collapse of stars and the supernova catastrophe.

Before presenting any specific conclusions or recommendations, the Working Group wishes to emphasize its strong feeling that x-ray and gamma-ray astronomy must be assigned a priority comparable with that accorded older established fields of space astronomy. Surely, no one will deny that the exploration of an entirely new field is likely to produce scientific results as important, to say the least, as the refinement and extension of observations in established fields. Thus, the fact that NASA has very appropriately committed large resources to the needs of the classical branches of astronomy must not prevent it from providing adequate support to a new branch of observational astronomy, as soon as its potential value becomes established.

OBJECTIVES OF OBSERVATIONAL X-RAY ASTRONOMY

With regard to x-ray astronomy, the following problems are singled out as being both of great scientific significance and accessible to experimental investigations, now or in the near future (the order of listing does not reflect their relative importance):

(i) search for weaker discrete sources (10^{-3} to 10^{-6} times the strength of those observed so far);
(ii) precise determination (to 1 min or better) of the location of the discrete sources in order to make possible identification of the x-ray sources with optical or radio objects;
(iii) study of the structure of the discrete x-ray sources with a resolution of better than 5 sec, or establishing an upper limit of this order of magnitude for their size;
(iv) study of the spectral distribution of the radiation from the various discrete sources; search for emission lines, absorption edges, and the long wavelength cutoff expected from interstellar absorption;
(v) search for polarization of the x radiation from discrete sources;
(vi) directional and spectral study of the diffuse radiation, with the aim of establishing its galactic or extragalactic origin, and investigation of the properties of the media in which it arises and through which it passes;
(vii) search for time variation of both long and short duration in the intensity of the discrete sources.

OBJECTIVES OF OBSERVATIONAL GAMMA-RAY ASTRONOMY

With regard to gamma-ray astronomy, the less certain nature of the observations to date suggests a somewhat less specific set of problems:

(i) study of the directional and spectral characteristics of the diffuse gamma-ray flux in both the 1-MeV and >100-MeV regions, with the aim of establishing its real intensity (as distinguished from an upper limit), its origin (whether galactic or extragalactic, or both), and production mechanisms;
(ii) study of the gamma-ray flux from strong radio sources with instruments of vastly improved sensitivity and angular resolution;
(iii) study of nuclear gamma rays from supernova remnants, such as the Crab Nebula, with instruments capable of improved background rejection.

EXPERIMENTAL APPARATUS AND TECHNIQUES

The Working Group discussed the experimental techniques available or under development, in terms of their promise for future observations. The following conclusions emerged:

(i) X-ray detectors, provided with mechanical collimators (the only type of instrument used so far for nonsolar x rays), will continue to play an important role in the investigation of most of the problems mentioned above, especially if the detector areas can be substantially increased. An active research program using detectors of this type, carried aloft by balloons, rockets, unmanned satellites, and manned space vehicles, should be strongly encouraged. Controls for stabilization and orientation must be developed to the limits of present technology.

(ii) Occultation methods have provided some of the most definitive measurements of source structures in radio astronomy. The only identification of an x-ray source with an optical or radio object has been made utilizing this method. Lunar occultations will continue to provide fine resolution of size and accurate positional data, and for these measurements large area detectors are essential. Such detectors could be employed effectively on the lunar surface or on a lunar orbiter.

(iii) Total-reflection telescopes, similar to those already successfully employed for taking x-ray pictures of the Sun, offer great promise for nonsolar x-ray astronomy. Existing telescopes carried by rockets could photograph the strongest discrete sources, if the rockets were kept pointed in the right direction with an accuracy of 1 min of arc for periods of the order of one minute of time. Larger telescopes, such as could be accommodated aboard unmanned or manned satellites, appear to be the only tools capable of many of the refined observations that will be needed beyond the early exploratory stage. The angular resolution provided by x-ray telescopes (about 5 sec of arc) is greatly superior to that obtainable with mechanical collimators. Telescopes used as concentrators afford the possibility of polarization experiments and of spectral measurements by means of dispersive techniques that would not be feasible otherwise. Moreover, telescopes will be competitive with large area detectors for the discovery of very weak sources. Therefore, a program of x-ray astronomy using total-reflection telescopes should be started at the earliest possible time and pushed vigorously to exploit its ultimate capabilities.

(iv) Studies of both diffuse and point sources of the 100-keV to 10-MeV range will require the continued development of actively shielded collimated detectors, as well as the development of techniques using Compton coincidence telescopes or pair spectrometers. Since this is an energy region where line emission is expected, energy resolution of high order will be required. Solid-state detectors should prove useful. Initial observations and background studies may be made with balloons. However, sky surveys at sensitivities of possibly 10^{-5} cm^{-2} sec^{-1} and employing detection systems with angular resolution of about 1° will require the long exposure time available only from a satellite.

(v) Detection of more energetic gamma rays from discrete sources will require instruments of large collection factor (i.e., area exposure time), angular resolutions of better than 1°, and, most important, a proven capability for background rejection. Both spark chambers of wide angular sensitivity (but with 1° resolution or better), and gas Čerenkov detectors of narrow angular sensitivity, both merit and require development. Above 300 GeV, the radiation can be detected with ground-based instruments.

(vi) Instruments of the type discussed under (v) above may, if their background rejection properties are adequate, be suitable for further clarification of the diffuse gamma-ray problem. Probably more suitable, however, are instruments especially designed to detect diffuse radiation. Proposals should be judged on the basis of their promise for achieving significant increases in sensitivity beyond that of instruments already flown, or now being prepared for flight.

VEHICLES

The Working Group finds that the implementation of this program will require the full utilization of currently available vehicles, as well as the development of new vehicles with special characteristics. Specific needs have been found for the following:

Balloons

Balloons will play an essential role in observations in the energy range above 15 keV. In addition, they will be necessary for testing prototype instruments designed for x-ray and gamma-ray observations from satellites. In order to carry out the present and foreseeable objectives, four recommendations are made:

Recommendation 1. A substantial increase in the number of balloon flights should be authorized for x-ray and gamma-ray astronomy. Considering the number of investigators who are presently carrying out or planning balloon experiments in this field, the Group foresees a need for at least 40 balloon flights per year.

Recommendation 2. Two new types of reliable balloons are needed. One type would be capable of lifting 250 lb of scientific payload to an altitude in excess of 145,000 ft, while the other would be capable of lifting payloads of 2,000 lb to 130,000 ft. Attainment of these objectives will require fundamental advances in balloon technology. The developmental program should be the responsibility of an experienced group of balloon scientists and engineers, preferably in the National Center for Atmospheric Research (NCAR).

Recommendation 3. A program should be funded for the engineering development and construction of a prototype of a controllable star-guided orientation system, with an accuracy better than 1 min of arc.

Recommendation 4. To meet the present demands for balloon flights for astronomical and other purposes, as well as to accommodate the expected increase in these demands, the physical plant and personnel of the NCAR balloon launching facilities should be expanded, as appropriate.

Rockets

Rockets will continue to provide a means for performing experiments complementary to those carried out from satellites, particularly exploratory experiments, as well as providing the experience for successful performance of satellite experiments. In particular, the following recommendations are offered:

Recommendation 5. A twofold increase in the number of rocket flights (from the present number of about 6, to about 12 per year) is required for x-ray astronomy. This will provide for new groups entering the field and will facilitate accumulation of data at a proper rate.

Recommendation 6. Weight-lifting capacity and volume in excess of those provided by the Aerobee vehicle are required for certain applications.

Recommendation 7. Pointing accuracies considerably finer than those now available are essential to take full advantage of the inherent precision of existing x-ray optics. An accuracy of 15 sec of arc to 1 min of arc is required, with small drift or jitter rates. While developmental pointing systems with the desired characteristics have been flown, they are not available to experimenters at this time. Pointing jitter should eventually be reduced to 5 sec of arc, to match the resolving power of focusing x-ray optics. This aspect of rocket technology should therefore be pursued with particular emphasis.

Satellites

X-ray astronomy has advanced to the point where x-ray Explorer satellites and a substantial fraction or all of an Orbiting Astronomical Observatory (OAO) are justified and needed. The volume and payload capacity of the Apollo Extension Systems (AES), with the potential advantage of participation by a man, can also be fully utilized for x-ray and gamma-ray astronomy. The following recommendations reflect some of these considerations:

Recommendation 8. Proposals for x-ray experiments suitable for inclusion as primary OAO experiments should be considered as possible substitutes for already accepted OAO experiments in the more conventional fields of astronomy.

Recommendation 9. The first unassigned OAO should be set aside for x-ray observations, to be supplemented if possible by much lower priority gamma-ray observations.

Recommendation 10. The OAO pickaback opportunities should be recalled to the attention of the scientific community and suitable experiments in x-ray or gamma-ray astronomy should be accommodated. If at all possible, spacecraft components should be rearranged in minor ways in order to permit use of optical focusing devices with focal lengths comparable to the length of the OAO.

Recommendation 11. Experiments utilizing focusing x-ray optics now at hand should be flown on pointed rockets and satellites currently available, or being developed.

Recommendation 12. Plans should be begun for orbiting an x-ray telescope of greater focal length than can be accommodated in present satellites. Lengths in the range of 30 ft to 100 ft are considered necessary to achieve the desired sensitivity and resolution. Unfolding of an extensible system in space may be possible.

SUPPORTING TECHNOLOGY FOR X-RAY INVESTIGATIONS

X-ray astronomy involves techniques of a very specialized nature which have been developed rather slowly by a handful of physicists, working in only a few laboratories, largely for the purposes of solid-state spectroscopy and crystallography. An urgent need exists for the development of standards of absolute photometry, including monochromatic and continuous sources, detectors, crystal and ruled grating analyzers, reflectors and filters, and adaptation of light converters and image amplifiers to x-ray imaging.

Recommendation 13. It is recommended that a strong program in the technology of soft x-rays be implemented under the management of one of the NASA research centers.

GAMMA-RAY ASTRONOMY

Energies in the Range 0.1 - 30 MeV

Experiments in the next few years over the energy range 0.1 - 30 MeV will continue to be of an exploratory nature, designed to search for point sources and gamma-ray lines, and to study the diffuse component. Important observational attempts, particularly on known x-ray or radio sources, will soon be made from balloons. Experiments designed to study the diffuse component or to accomplish a sky survey must be conducted from a vehicle with minimum capabilities at least equal to those provided by Explorer-class satellites. With the foregoing considerations in mind, the Working Group makes the following recommendations:

Recommendation 14. Continued support should be given to the development of detection devices using actively shielded collimators or Compton scattering telescopes, as well as to the development of promising new techniques.

Recommendation 15. Space flight assignment should be given only to instruments which indicate clear improvement in background reduction, energy resolution, or angular definition, and which have been proven on balloon investigations.

Recommendation 16. Greater support should be given to balloon programs as well as Explorer-class satellites, to implement observations in this energy range.

Recommendation 17. Support should be provided for the continued study of solar x-ray and gamma-ray spectra during both quiet and active phases of the solar cycle.

Energies Greater than 100 MeV

No flux of photons with energy greater than 100 MeV has been definitely detected. Because of the low photon intensity and large background radiation, only upper limits to the flux have been determined. In addition, the ratio of the radiation from point sources to the diffuse radiation may be very small, making the detection of point sources all the more difficult.

With respect to the experimental aspects of high-energy gamma-ray astronomy, the Working Group makes the following recommendations:

Recommendation 18. Instruments with large collection factors (area x exposure time) should be developed which provide angular resolutions of better than 1°, which can determine the energy spectrum, and which possess a proven ability to reject background radiation.

Recommendation 19. Experiments should be undertaken in balloons to search for point sources and to define the design requirements for satellites.

Recommendation 20. Since measurement of the diffuse radiation flux would be a significant experiment and could be performed in the immediate future with small Explorer-type satellites, it should be attempted.

Recommendation 21. Since experiments to study spectral composition and directional structure of the diffuse flux and to measure weak fluxes from and spectra of discrete sources will ultimately need a large orbiting vehicle, one of the forthcoming large orbiting vehicles should be scheduled for this purpose.

2. CURRENT STATE OF THE OBSERVATIONS

X-RAY ASTRONOMY

About a dozen discrete x-ray sources (0.5-15 Å) have been detected thus far with rocketborne instruments. Half of these appear to be relatively isolated and fully resolved; the rest are clustered more densely and may be further resolved with slightly improved observations. The most remarkable feature of the distribution is a concentration toward the galactic plane, which implies that the sources are galactic objects. Six of the observed sources lie within only ±20° of longitude from the galactic center and have a range of only a factor of 2 in brightness. One possible conclusion is that they are at roughly similar distances from the Sun and possibly in the general vicinity of the galactic center. The brightest source lies in Scorpius, and is farthest from the galactic equator, at about +25° of galactic latitude. It is 10 to 20 times as bright as those clustered toward the galactic center. Figure 1 summarizes the observational information obtained to date.

Only one source, the Crab Nebula, has been positively identified with an optical or radio source. By observation of a lunar occultation, the

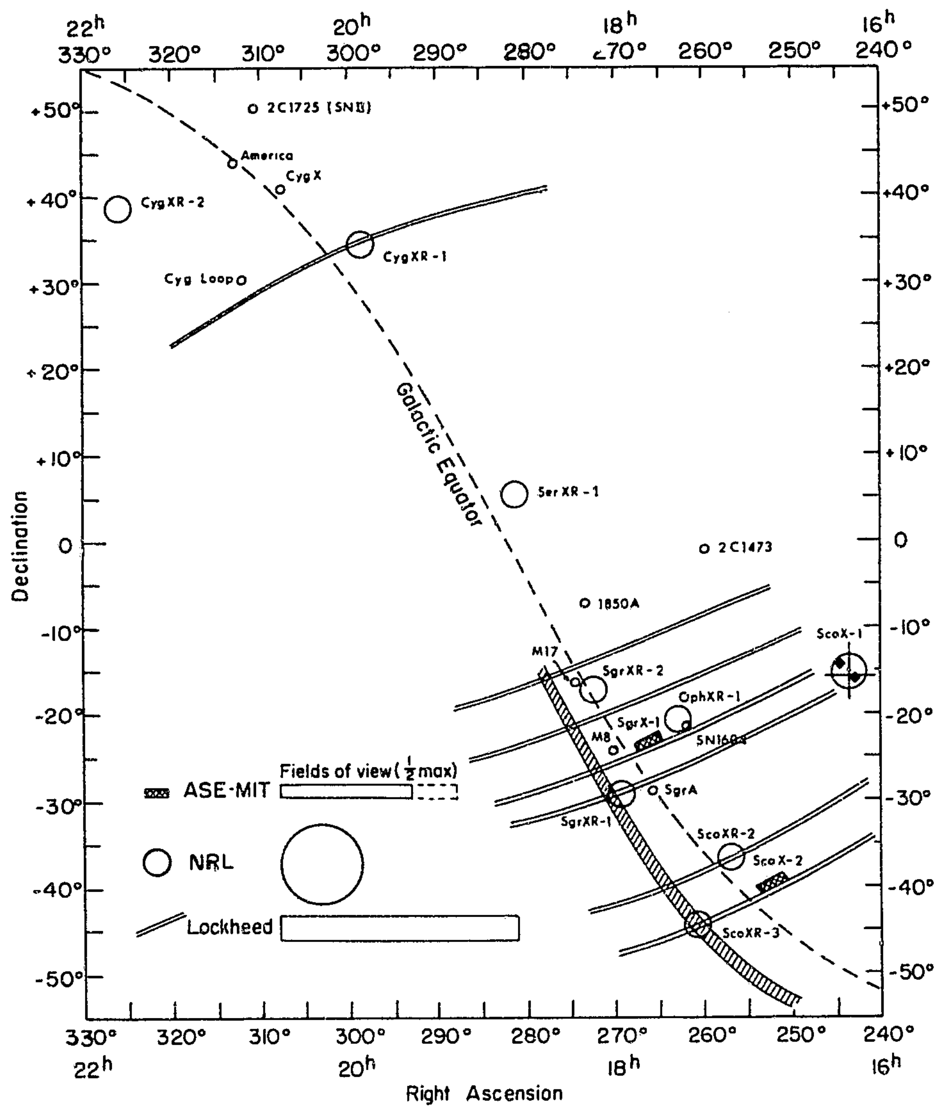

Figure 1. Summary of observational x-ray data to date. Source definition is indicated by the size of the circle or rectangle. The scan paths represent source observations in one dimension observed by the Lockheed group from a pointed and slowly rotating rocket. The proximity of the resolved sources to the galactic plane is clearly evident. (Courtesy of American Science & Engineering, Inc.)

x-ray emission was found to be associated with a region about one light year in diameter, approximately centered in the optical nebula. Other source positions are known within limits varying from ±15' to about 1.5°. Within the estimated position errors and sensitivity limits of the measurements, it appears that the radio center of the Galaxy is not an x-ray source. Kepler's supernova lies close to an observed source but at an angle somewhat larger than the estimated uncertainty in the position of the source. The strongest source, in Scorpius, has been located to within ±15'.

There appear to be at least two classes of sources: those accompanied by radio and optical emission, and those which remain as yet unidentified with any radio or optical object. The positive identification of an x-ray source with the Crab Nebula has suggested that some x-ray sources are supernova remnants. Nevertheless, Cassopeia A, the strongest radio source, believed to be a supernova of Type II, has been scanned with high sensitivity, but no x rays have been detected. The Tycho Brahe 1572 supernova of Type I has also been well scanned with only negative results. Conversely comparison of the observed x-ray positions with ancient records of visible supernova events of the past 2000 years reveals no correlations other than that with the Crab Nebula.

With the exception of the Crab Nebula, whose x-ray emission region has been defined by the occultation observation, information has been gathered only on the upper limit of the size of the x-ray sources. The observations thus far have been made with large area detectors and mechanical collimation devices, rather than with reflecting and focusing x-ray optics. The smallest field of view in the scan direction has been approximately 2°. With each improvement in resolution, the observed sources have been found still to lie inside the instrumental resolution. A modulation collimator has been successfully employed to observe the brightest Scorpius source and has shown its angular extent to be smaller than 7 min of arc in at least one dimension.

Spectral information is still very meager, but there are substantial differences in the spectral distributions of the sources thus far observed. Observations have been made with Geiger counters, proportional counters, and scintillation counters; and filter photometry has been applied in combination with choices of gas fillings to control the broadband spectral efficiencies of gaseous detectors. From such rough samplings of spectral bands in the 0.5 to 15 Å range, it seems evident that the emission of most of the individual sources cannot be fitted to any simple spectrum. It is likely that nonthermal and thermal processes occur in the same object and that nonuniformities exist in physical structure, composition, and temperature through its volume. In the case of the Crab Nebula, however, a fairly coherent explanation of the entire spectrum from radio observations to the shortest wavelength balloon observation (about 0.2 Å), can be constructed on the basis of magnetic bremsstrahlung. One is, nevertheless, left with the problem of explaining the persistence of x-ray emission for 1000 years when the lifetime of the radiating electrons is of the order of only years or even months.

Observations by means of detectors with different fields of view indicate a diffuse x-ray flux amounting to about 1 photon cm^{-2} sec^{-1} $ster^{-1}$ in the 1.5 - 8 Å range. The estimate of the flux is based on the difference between responses of the detectors when looking in the upward direction and when looking downward. It is not absolutely clear that this flux arrives from outside the Van Allen belt regions, or that it is extragalactic rather than the integrated effect of numerous sources within the Galaxy too weak for individual detection. Certain bits of evidence, however, favor the extragalactic hypothesis. No indication of horizon brightening, as would be expected from a nearby atmospheric source has been found in flights from the White Sands Missile Range. If the diffuse flux were galactic, it would be stronger toward the Milky Way, but no such concentration was found. It has also been proposed that the diffuse flux is the net contribution of all galaxies, each radiating from collections of discrete sources similar to those in the local Galaxy. The intensity of the observed diffuse flux is roughly what would be expected in this hypothesis.

X-RAY ASTRONOMY AND GROUND-BASED ASTRONOMY

An optical search to 23rd magnitude has been made in an area 40 min of arc by 50 min of arc around the position of the brightest x-ray source in Scorpius. There is nothing obviously identifiable with the x-ray source unless it looks like a faint galaxy or a star. The presence of about 140 galaxies per square degree to 23rd magnitude suggests that the source is not likely to be completely obscured by interstellar dust, and that the average extinction may be about two magnitudes. The possibility that a star or galaxy in the field is indeed the x-ray source cannot be excluded until an excellent x-ray position (to better than 1 min of arc) has been determined, or until the object is shown to be extended by more than 2 sec of arc. On the other hand, a coincidence of x-ray and optical positions within 1 min of arc, together with a 2 sec of arc upper limit on the diameter, would be convincing. When positions for x-ray objects are accurate to a few minutes, a search should be attempted with the aim of making such optical and radio identification.

The problem of the diffuse x-ray flux is also related to optical and radio astronomy, especially if the attenuation of this flux by the interstellar gas of our Galaxy and of other galaxies can be observed. The radio data concerning neutral hydrogen in our Galaxy already offer a detailed basis for comparison.

GAMMA-RAY ASTRONOMY

The presently available data on the total incoming radiation per unit solid angle, although purporting in some cases to represent actual intensity measurements, can safely be interpreted above 8 keV only as upper limits. These data, from x-ray energies up to 100 MeV, are shown in Figure 2. The line in the figure represents an empirical inverse-square law for the differential number spectrum; the corresponding integral spectrum (the total flux above energy E) would be given by $J\ (>E) = 0.02/E$ (MeV) $\mathrm{cm}^{-2}\ \mathrm{sec}^{-1}\ \mathrm{ster}^{-1}$.

At an energy near 10^9 eV other points (again an upper limit of the gamma-ray flux) have been provided by the frequency of cosmic-ray air showers which lack the usual muon component. The integral frequency at this energy is about 7×10^{-13} photons $\mathrm{cm}^{-2}\ \mathrm{sec}^{-1}\ \mathrm{ster}^{-1}$, or one per

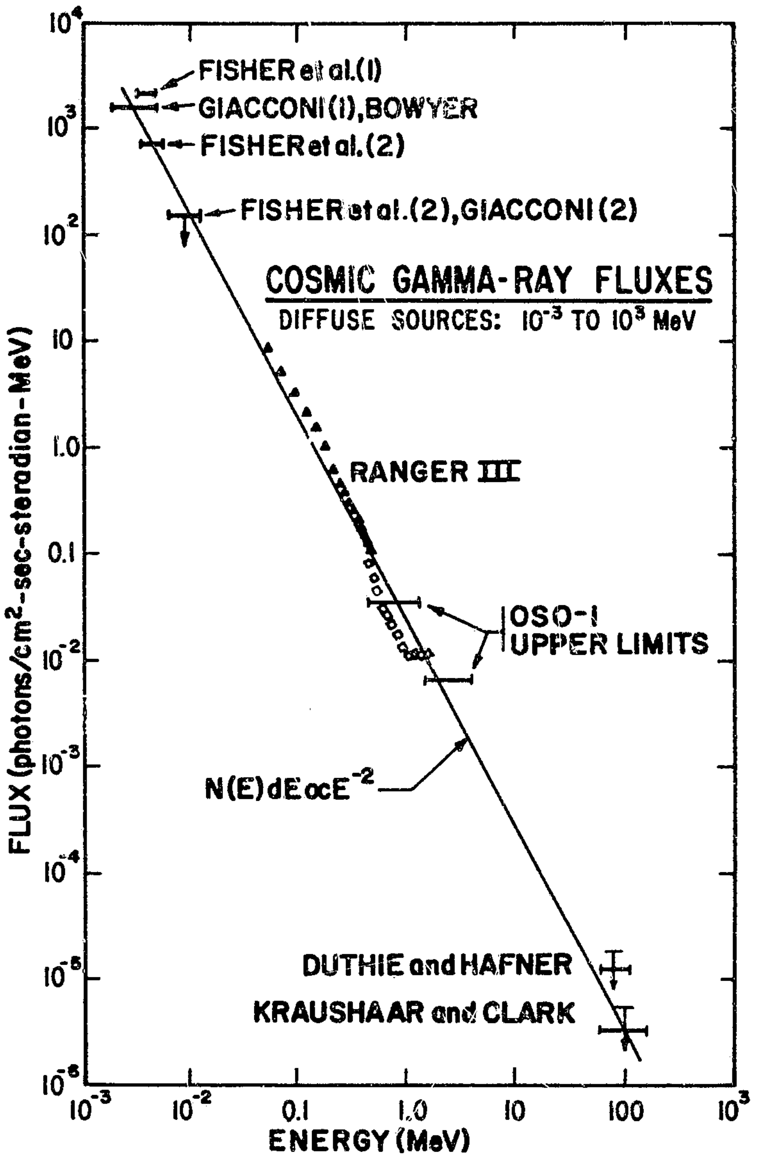

Figure 2. The present state of knowledge of the diffuse cosmic photon spectrum. Numbers in parentheses after names refer to first and second rocket observations. (Based on graph furnished by L.E. Peterson, University of California, San Diego.)

3500 charged cosmic rays of the same energy. If one extrapolates the line in Figure 2 following a curve generally parallel to the primary cosmic-ray spectrum, the curve should intersect this point.

No directional variation has yet been detected in any of the radiation described above; the curve should be taken as an indication of the possible total flux averaged over all directions, in line and continuous emissions from both point and diffuse sources.

Upper limits have also been obtained for the intensities in two particularly significant gamma-ray lines: 10^{-3} cm^{-2} sec^{-1} ster^{-1} at 0.5 MeV (representing positron-electron annihilation) and 4 x 10^{-4} cm^{-2} sec^{-1} ster^{-1} at 2.2 MeV (representing neutron-proton capture to form deuterium).

Directional detectors have been used to search for point sources in various energy bands. At 100 MeV, many suspected sources have been scanned with sensitivities in the range 10^{-3} to 10^{-4} cm^{-2} sec^{-1}, all with negative results. At 1000 MeV, a substantial portion of the northern sky (including Cygnus A, Cassiopeia A, 3C48, and 3C47) has been scanned at a sensitivity somewhat better than 10^{-4} cm^{-2} sec^{-1}. And at 10^{7} MeV, the four strongest radio emitters, including Cygnus A and the Crab Nebula, have been scanned at a sensitivity approaching 10^{-11} cm^{-2} sec^{-1}. All of these experiments have been negative and provide only upper limits of the intensity, although a possible flux has been reported by the Harwell-Dublin group. Further experiments now in preparation will at least refine the upper limits by one or two orders of magnitude, if they do not succeed in detecting definite amounts of radiation.

Even at present levels of sensitivity, the established upper limits of intensity have permitted significant cosmological and astrophysical conclusions. It has been noted that in the x-ray region, the diffuse flux is for X below that predicted to originate in the intergalactic medium by the hot universe model and hence permits the discarding of this model. Similarly, the weak intensity of the 0.5 MeV line and at 100 MeV rule out the hypothesis of a universe containing equal amounts of matter and antimatter. Indeed, the latter data also provide a significant limit to the product of gas density and ordinary cosmic-ray density in intergalactic space, and determine that only an extremely small fraction of the cosmic rays can be antiparticles. The failure to detect 10^7 -MeV photons from the Crab Nebula rules out the model in which the high-energy electrons are secondary to a hundredfold richer, nonradiating proton complement. Other examples can be given. The important thing to note, however, is that even slight refinements in the data, whether they lead to definite intensities or only reduce the upper limits, will strengthen conclusions like those mentioned above, and permit further inferences about the acceptability of various theories of nucleosynthesis, stellar collapse, supernova explosions, stellar and galactic evolution, and the nature of the Universe on the grandest possible scale.

A SAMPLE SPECTRUM

Figure 3 summarizes, by way of illustrating the foregoing statements, the present experimental information on the electromagnetic spectrum of the Crab Nebula; the higher frequency points in this figure all being upper limits. Several features can be noted in this figure. One is the tremendous range spanned both in frequency of the radiation and in counting rate. Another is the way in which many widely diverging techniques complement each other in providing the complete picture. A third is that the series of points which indicate the upper limit of the flux at high frequency connect smoothly with the definite measurements in the x-ray range and below, a fact suggesting that a rather modest increase in sensitivity may convert some of these upper limits to definite points on the spectrum.

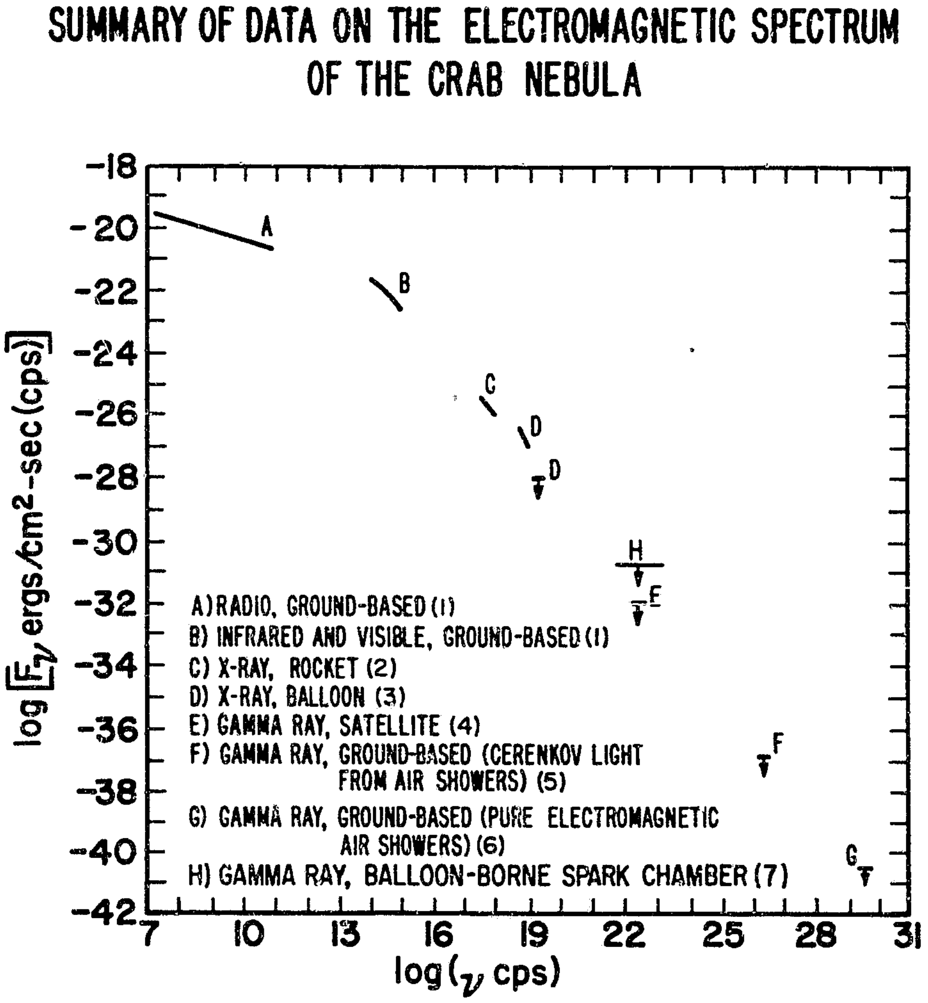

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Figure 3. Summary of data on the electromagnetic spectrum of the Crab Nebula.

3. THEORETICAL CONSIDERATIONS

THEORIES OF THE ORIGIN OF COSMIC X RADIATION AND GAMMA RADIATION

A discussion of theories for the production of cosmic x radiation and gamma radiation immediately provides insight into the cosmological significance of the field of x-ray and gamma-ray astronomy. Present ideas concerning possible production mechanisms play an important role in the planning of future experiments and in the design of instruments for observations. Consequently some of the more tenable ideas are presented here.

At the present level of knowledge, only five conclusions concerning sources of cosmic x rays can be drawn from the experimental observations:

(i) Discrete sources exist in the Galaxy.

(ii) At least some sources of small angular diameter exist in the Galaxy.

(iii) Different sources show major spectral differences.

(iv) The spectral distribution of at least one source (the brightest source in Scorpius) cannot be described by a single parameter.

(v) A diffuse and nearly isotropic flux of x rays exists.

In the discussion of possible x-ray production mechanisms in discrete sources, we must be aware that several different mechanisms may be simultaneously at work. Unique spectral properties of different source mechanisms are suggested below.

The picture is much less clear with regard to the production of cosmic gamma rays, for thus far no discrete sources have been observed, and only upper limits on the possible intensity of the diffuse flux of high-energy photons have been placed from experimental observations. It is nevertheless widely believed that both discrete sources and a diffuse flux of high-energy gamma radiation must exist. Theories for the production of these gammas consequently are also presented.

MECHANISMS OF X-RAY PRODUCTION

There are a number of physical processes by which photons of x-ray energies can be produced. The most likely mechanisms are:

(i) Magnetic bremsstrahlung (also commonly known as synchrotron radiation);

(ii) Collisional bremsstrahlung, recombination radiation and line emission;

(iii) Compton scattering; and

(iv) Blackbody emission.

Magnetic Bremsstrahlung

The form of radiation known as magnetic bremsstrahlung results from the deflection of electrons and positrons in the local magnetic field. Radio and visible synchrotron spectra can usually be approximated by a power law. The radiation emitted by relativistic electrons by this mechanism is linearly polarized.

In the event that optical and radio synchrotron emission is observed from a discrete x-ray source (e.g., the Crab Nebula), then the near constancy or slow change of the spectral index would be an argument in favor of the synchrotron emission mechanism in the source. But the lack of such a constancy would not necessarily be an argument against such a source mechanism, since a depletion of high-energy electrons in the source is to be expected. Interstellar absorption may also affect the observed spectrum. Observation of the linear polarization of the x rays would provide support for their synchrotron origin.

Perhaps the greatest difficulty in interpreting the x radiation from the Crab Nebula as synchrotron emission is the short lifetime (order of 30 years) expected for the 10^{14} eV electrons needed to produce 10 keV x rays in a magnetic field assumed to average 10^{-4} gauss over the dimensions of the source. It is possible, however, that local magnetic field variations of two to three orders of magnitude exist within the nebula, and that the x rays are produced by lower-energy electrons wandering into these internal magnetic fields. The electrons could thus survive for very long times before radiating. Suggestions have also been made concerning possible mechanisms for the continuous acceleration of electrons to high energies in the vicinity of a hypothetical neutron star remnant of a supernova. Thermal bremsstrahlung and blackbody radiation, in the x-ray region, would both be expected under these circumstances. Spasmodic or periodic sources of such high-energy electrons would lead to time variations of the x-ray intensity on a time scale of perhaps less than ten years, possibly even down to milliseconds. Detection devices with higher time resolution may make possible the observation of such variations.

Collisional Bremsstrahlung

Collisional bremsstrahlung is produced in the Coulomb scattering of electrons by nuclei in local gaseous matter. Thermal electrons in a hot, optically thin gas can produce photons in the x-ray region. If the electrons initially possess a Maxwellian distribution, then the resultant photon spectrum would be an exponent of the form exp $(-E/kT)$. Superthermal electrons (i.e., with energies of tens of keV) can also produce x rays of the observed energies through bremsstrahlung in a cold plasma. While the photon spectrum would depend on the initial electron spectrum, in neither case should the photons exhibit polarization.

Indications are that if the x-ray emission is due to collisional bremsstrahlung, a plasma temperature of about 10^7 °K is required. The observed flux of higher-energy x rays from the Crab Nebula would nevertheless seem to require a temperature of the order of 10^8 °K. In the case of the Crab Nebula, therefore, one must attempt to explain the existence of such hot local areas through a continuous internal source of energy. Heating from the radioactive decay of heavy nuclei produced in a supernova explosion has been mentioned as one possible source. Transferral of possible vibrational or rotational energy stored in a neutron star to the surrounding environment through magnetic interaction, particle acceleration, or progressive waves which steepen into shock waves above the surface, have also been mentioned as possible sources of the heating.

In addition to the thermal bremsstrahlung emitted, there would be recombination radiation and line emission from the gas. Assuming normal cosmic abundance of the elements, one can compute the collisional excitation of emission lines from such a hot gas. The results of these calculations indicate that some of the characteristic emission lines and radiative recombination edges should be detectable above the bremsstrahlung continuum. The Ne X line at 1.02 keV, for example, would be the strongest, and an energy resolution of only about 20% is required to separate out comparable counting rates from the line and the continuum at that energy. About ten other lines are generally not more than a tenth as intense as the Ne X line relative to the continuum. Recombination edges due to captures into the ground state of 0 VIII at 0.87 keV and Ne X at 1.36 keV should show discontinuities of factors of 1.5 in intensity across the recombination edge. Observation of line emission and recombination edges, therefore, would be important evidence that at least part of the x-ray emission from discrete sources is thermal bremsstrahlung. Measurement of the relative intensities of lines would provide information on the abundance of the elements within the sources, also on the temperature of the gas. To determine other physical characteristics of x-ray emitting regions, line shape measurements might be considered as spectral resolution techniques are improved.

Finally, simple considerations of the bremsstrahlung from a hot plasma cloud show that the particle densities in the cloud can be deduced from the observed photon flux and the angular diameter of the emitting region, if a total mass for the source is assumed. A distance for the source is thereby also deduced. Alternatively, the particle density and mass of the source can be deduced from measurements of the flux and angular diameter and knowledge of the distance of the source. Optical identifications should aid in determining the distances of sources.

Compton Scattering

Compton scattering radiation results from the scattering of high-energy electrons by photons of the local radiation field. It appears to have been ruled out as an important mechanism for x-ray production in discrete galactic sources on both experimental and theoretical grounds. From the theoretical standpoint, the probability that a synchrotron photon undergoes such a Compton scattering before escaping from the gaseous region is small, even for the conditions in quasi-stellar radio sources. From the experimental standpoint, a search for the corresponding 20 MeV gammas that would be produced in bremsstrahlung of the energetic electrons in Scorpius gave negative results, placing an upper limit on the proton density in the region of the source of less than 3×10^{-3} protons per cm^3. This is smaller than the average density of protons in interstellar space by a factor of 300.

Blackbody Emission from the Surface of a Neutron Star

Here the spectrum would be a Planck blackbody spectrum with the emitted energy proportional to T^4. The spectrum would be easily distinguishable from power law and exponential spectra. Surface temperatures of the order of 10^7 to 10^6 °K are predicted for such objects, depending on age. This is not entirely inconsistent with temperatures deduced from spectral observations of the two major x-ray source regions, namely, Scorpius and the galactic center. A small object, say, 10 km in diameter, at this temperature might be observable in the x-ray region at a distance of about 1,000 parsecs but not observable in the optical or radio portion of the spectrum.

Theoretical objections have been raised to the neutron star concept, the principal objection being that the cooling of a star by neutrino emission might be far too rapid to account for the long lifetime (order of 1,000 years) of the x-ray sources. The results of the calculations have depended on a possibly significant overestimation of the number of pions in the interior of a neutron star. Therefore, there is still some reason to believe that thermal x-ray emission from neutron star remnants of supernova explosions may be observable tens of thousands of years after the explosion, at least at low energies.

In addition to the expected blackbody radiation, there may be distinct spectral features (both in emission and absorption) in the spectrum of neutron stars from 10 to 100 keV, corresponding to the K and L levels of ionized elements in the range from $Z = 50$ up to 100. These features may not correspond to those ordinarily measured in the laboratory, owing to high Zeeman splitting of lines in the possibly intense surface magnetic fields, to gravitational red shifts, and to thermal broadening. Observation of such lines could nevertheless help in understanding the nature of the x-ray sources.

MECHANISMS FOR GAMMA-RAY PRODUCTION

Possible radiation mechanisms leading to the production of energetic cosmic gamma rays are the following:

(1) Decay of neutral pions ($\pi 0 \rightarrow \gamma + \gamma$), which may be produced in

(i) high-energy cosmic-ray proton collisions with the interstellar and intergalactic gas, or

(ii) proton-antiproton annihilation;

(2) Bremmstrahlung of relativistic electrons and positrons;
(3) Compton scattering of relativistic electrons by photons;
(4) Synchrotron radiation by high-energy electrons;

(5) Annihilation of positrons in the interstellar and intergalactic medium ($e^+ - e^+ \rightarrow 2\gamma$); and

(6) Nuclear de-excitations from excited states of heavy nuclei, following thermonuclear reactions and following neutron capture.

The first four processes produce a general cosmic photon continuum which is certain to exist, although the magnitude of the resulting flux is uncertain. Processes (5) and (6) will produce line emission.

The basic data needed to compute the nature of the photon spectrum are: the flux and spectra of high-energy protons and electrons, the mean density of gaseous matter, the low-energy radiation flux, and the magnetic field in galactic and intergalactic space as well as in discrete sources. Other astronomical data such as galactic and cosmic distances and the rate of expansion of the Universe are also important. It has already become apparent that interpretation of the observations on cosmic gamma rays can, in fact, be used to determine or at least to set limits on these important astronomical parameters. It is significant that these parameters would be extremely difficult to determine from the more conventional radio and optical observations alone. The limits already established from very preliminary observations of cosmic gamma rays are extremely meaningful when applied to cosmological theories. As with cosmic x rays, more refined observations of the spectrum of cosmic gamma rays will provide very restrictive tests of models of the Universe.

A large amount of information about the nature of discrete sources, such as the Crab Nebula, can be gained from gamma-ray astronomical studies. For example, it has been suggested that in a supernova outburst considerable nucleosynthesis takes place. Estimates have been made of the number of radioactive elements produced in such a supernova outburst and the complex line spectrum has been calculated. Detection of these gamma-ray lines would provide definite proof of some details of the theories of nucleosynthesis and the origin of the elements.

Test of the synchrotron radiation hypothesis in the Crab Nebula would be the detection of ultrahigh-energy (5×10^{11}eV) gamma rays which would be produced by Compton scattering of high-energy synchrotron electrons by the lower-energy synchrotron photon spectrum.

Gamma radiation propagates in the interstellar and intergalactic space with almost no absorption, permitting observations from regions hidden to other forms of electromagnetic radiation. Thus, gamma rays may become a new means of probing the galactic center and of the Universe.

THE DIFFUSE X-RAY FLUX

There are several possible source mechanisms for the diffuse flux. This flux may originate from within the Van Allen belt region; it may represent the integrated effect of numerous unresolved sources within the Galaxy; it may be due to the emission from x-ray sources in all external galaxies; or, it may arise from bremsstrahlung in a hot intergalactic medium or from Compton scattering with relativistic intergalactic electrons. The validity of these hypotheses can be investigated by studying the spectral and spatial distribution of the diffuse flux.

On the assumption that the average x-ray luminosity per galaxy is roughly the same as that for our own Galaxy, the extragalactic origin of the diffuse flux appears to be reasonable. This has an important consequence if the x-ray emission spectra of galaxies include strong lines. Due to the differential red shift from expansion, the combined line emission from all extragalactic combined sources would be smeared into a continuum with an edge at the proper emission line energy. The shape of this continuum depends on the large-scale structure of the Universe, that is, on the cosmological model. Thus, the determination of the spectral distribution of the diffuse flux offers the possibility of yielding significant information on the fundamental cosmological problem.

To estimate the average directional intensity due to the contributions from galaxies along a line of sight, we may assume an average source density throughout the Universe equal to the product of the Galaxy's x-ray luminosity multiplied by the density of galaxies. This is then integrated out to the Hubble radius. Taking 10^{-75} cm^{-3} for the density of galaxies, and 10^{28} cm for the Hubble radius, one finds 10^{-1} cm^{-2} sec^{-1} $ster^{-1}$ for the expected average intensity of extragalactic x rays.

One cosmological conclusion has already been drawn from the intensity of the diffuse flux. By assuming that it has its origin entirely from thermal bremsstrahlung in a hot intergalactic medium (the "hot universe" model), the magnitude of the observed diffuse flux appears to establish an upper limit of 10^7 °K to the temperature of the intergalactic medium, which is two orders of magnitude below the temperature required by the hot universe theory. It is expected that future more refined observational data on the diffuse flux of cosmic x rays will provide more restrictive tests of cosmological theories.

ESTIMATE OF X-RAY FLUXES FROM EXTRAGALACTIC OBJECTS

The available data on x-ray sources in the Galaxy can be used to arrive at plausible estimates for the x-ray fluxes that might be anticipated from external galaxies. For example, the 10 cm^{-2} sec^{-1} flux from the general direction of the galactic center may arise from sources near the galactic center. Assuming this to be the case, and that the Galaxy is typical, the flux from the Andromeda Nebula would be expected to be smaller than that from our own by the square of the distance ratio. This gives a flux of about 2×10^{-3} cm^{-2} sec^{-1}. In the case of the Virgo cluster, the greater distance is more than compensated by the large number (over 2,000) of galaxies which could be within the field of view. Assuming that the average x-ray luminosity of these galaxies is the same as ours, the estimated total flux from the cluster is 2×10^{-2} cm^{-2} sec^{-1}. These numerical values may well be lower limits, since no allowance was made for interstellar absorption effects which may be substantial in the direction in which the local sources near the center of our own Galaxy were observed. Furthermore, the average luminosity of galaxies may be substantially higher than that of our own, as could be the case if some types of galaxies were unusually powerful emitters of x rays.

The prospects of extragalactic x-ray astronomy might also be gauged by rough extrapolation from radio and optical fluxes of synchrotron radiation. Almost all radio galaxies emit a fairly smooth spectrum over the full observable range of radio frequencies from about 50 Mc/sec to 10,000 Mc/sec and the range of observed spectral indices runs from about 0.2 to 1.5 if fitted to a power law. Although in many cases the index is nearly constant over the entire radio-frequency range, more often the index increases at higher frequencies, indicating a deficiency of higher-energy electrons. The most likely sources to provide detectable x-rays would be the relatively young sources which have both the highest radio brightness and the smallest spectral index. Some specific examples are offered below to illustrate the possibilities of extrapolation from radio data.

The strongest extragalactic radio source is Cygnus A. Its flux at 100 Mc/sec is 7.5 times that from the Crab. At 6 meters the spectral index is 0.5, compared to 0.3 for the Crab Nebula, but at wavelengths shorter than 50 cm, the index increases to 1.2. Extrapolated to the x-ray range, this would indicate a flux ~2 orders of magnitude weaker than that from the Crab. The distance to Cygnus A is 2×10^{8} parsec. If the intergalactic hydrogen content averages 10^{-28} g/cm^{-3} unit optical depth would occur at 10 Å and, at shorter wavelengths, the attenuation would rapidly decrease. Accordingly, Cygnus A should be detectable with an order of magnitude increase in detector sensitivity over that used in past observations, unless the spectral index increases very rapidly toward shorter wavelengths.

The galaxy M 82 provides visible evidence of a colossal galactic explosion. The Palomar photographs show matter being spewed out in all directions. In the plane of the galaxy the explosion is slowed down by the relatively dense gas, but above and below the plane the particles escape relatively freely at relativistic energies. M 82 is located at high galactic latitude and its distance is 10^{7} parsec. Intergalactic absorption should be negligible and galactic absorption minimal. Its radio flux is about 100 times weaker than that from the Crab and its spectral index is 0.23 from radio to visible frequencies. It is, therefore, a likely x-ray emitter at an intensity perhaps 100 times weaker than the Crab.

3C273 is the brightest quasar. It has been resolved into two components, A and B, with spectral indices of 0.9 and 0.0, respectively, near 400 Mc/sec. Eighty percent of the radio flux originates in the B component, with the flat spectral characteristic. At 400 Mc/sec, 3C273 B is 50 times weaker than Taurus A. Recent optical evidence indicates that the spectral index increases rapidly in the optical range of wavelengths, and its x-ray flux may be considerably less than 1/50 that from the Crab. Because of its great distance (5 x 10^8 parsec) intergalactic extinction will be strongly wavelength dependent and unit optical thickness will occur at about 8 Å if the density of intergalactic space is 10^{-28} g/cm^{-3}.

4. METHODS OF OBSERVATION

NONFOCUSING X-RAY OPTICS

All of the x-ray instruments flown thus far have employed simple mechanical collimation to define the field of view. Sensitivity has been achieved through the use of large area detectors—Geiger counters, proportional counters, and scintillators. The practical limitation on the further development of such devices is the space available on rocket and satellite carriers and the load lifting capability of balloons. In the Aerobee 150 rocket, the instrument compartment is a 15-in. diameter section, between 2 and 3 ft long. In x-ray surveys already carried out, this space has been almost completely utilized. The one successful x-ray observation thus far accomplished from a balloon used about 100 cm^2 of crystal scintillator, but apparatus now under construction will employ a 5000 cm^2 proportional counter.

There are opportunities for order of magnitude increases in the sizes of large-area detectors, if larger rockets can be obtained. Existing military rockets, such as the Sergeant and Nike Hercules, offer roughly 10 times the instrument compartment cross section of the Aerobee and about 5 times the payload, with substantial gain in altitude performance and flight time. It is important that such systems be made available to scientific users. Apollo vehicles, at various stages in the series of unmanned flights, can accommodate 5 to 10 square meters of x-ray detectors without resorting to unfolding techniques.

The Crab Nebula has been observed with a Geiger counter sensitive to the 1-15 Å spectral region, combined with an anticoincidence counter. The anticoincidence rejection efficiency was about 75%. The observed signal-to-background ratio was 7 and the flux above background about 3 counts cm^{-2} sec^{-1}. In practice 95% of the background counts produced by the interactions of cosmic rays in the detector and collimator can be rejected through the use of active anticoincidence shielding. The remaining cosmic-ray induced background, which amounts to an equivalent of 0.1 count $cm^{-2} sec^{-1}$, is still generally large compared to that due to the diffuse component of the cosmic x rays, except for detectors with very large fields of view.

From these numbers we can estimate the detection sensitivity of detectors with larger areas using the Crab as a standard source intensity. It is only necessary to consider the statistics of accumulated signal count relative to background. Let us suppose the total time of observation is 1 sec. Faster scanning will produce poorer statistics; slower scanning will improve the detection sensitivity. With unstabilized rockets the 1-sec integration is difficult to achieve; with stabilized rockets it can be well exceeded. A 1-m^2 counter with 95% efficient anticoincidence background rejection will have a residual background of about 1000 counts sec^{-1}. In 1 sec the 3-sigma background fluctuation will therefore be about 100 counts. The Crab would produce 30,000 counts in 1 sec, and a source only 1/300 of the brightness of the Crab would thus produce a signal equal to 3 sigma (background). Increasing the area still further will improve the sensitivity of detection as the square root of the area. In the Apollo Extension Systems (AES) program, it is feasible to consider a counter area of 100 square meters, a field of view of about one square degree and a scan rate of one degree per second. Between the largest detectors that can be erected from AES, with the aid of man, and the area limit of the Aerobee rocket, there is a wide range of possibilities in intermediate rocket and satellite sizes with appropriate compromises in scan rates. For example, even with the Aerobee rocket, it is possible, with stabilization, to utilize the full flight time for the observation of a single source, and thus gain 2 to 3 orders of magnitude in integration time over the unstabilized random scan mode.

Various collimators have been used with large area detectors. Hexagonal honeycomb gives a nearly circular field of view and has been used effectively for locating sources in a broad expanse of sky. Rectangular cellular collimators have been employed to increase the resolution in the scan direction at the expense of expanding the field of view in the direction perpendicular to the scan. The modulation collimator, consisting of parallel grids, has provided upper limits on source dimensions as small as 7 min of arc. With a well-stabilized Aerobee rocket and a smoothly controlled scan it is feasible to extend modulation measurements to yield a resolution of about 10 or 20 sec of arc.

Position measurements, except for the Crab occultation observation, have thus far been made from unstabilized or rather poorly stabilized rockets and have necessarily been relatively uncertain.

Spectral information from large area detectors has been derived by use of pulse height discrimination and by filter photometry. Large area proportional counters can achieve 20% energy resolution in practice and are expected to demonstrate such performance in flight in the immediate future. Much improvement in filter photometry is also possible within the Aerobee class of instrumentation. In the case of the Scorpius source it is even feasible to attempt x-ray crystal spectrometry with 0.1 Å resolution, from an Aerobee stabilized to one or two degrees.

Polarization measurements can provide important clues to the nature of the x-ray emission process. Studies of the performance of a liquid hydrogen target for Thomson scattering indicate that as little as 3% polarization from the Scorpius source could be detected in the 300 sec of observing time available during an Aerobee flight.

Large-area detectors are especially well suited to occultation measurements. The only positive identification of an x-ray source thus far has been obtained from the observation of the occultation of the Crab Nebula by the Moon. A remotely controlled mechanical occulter of large size, separated by considerable distance from a manned laboratory, is, in principle, feasible. A lunar orbiter can be employed to much greater advantage.

The most advantageous base for occultation measurements is the surface of the Moon itself. For example, a large-area detector may be placed on the Moon near the center of a lowland region surrounded by a high rim or crater wall. The rise and set of x-ray sources at the rim can be recorded continuously. If the rim is about 50 km distant from a detector whose vertical dimension is 10 cm, the subtended angle is about 0.5 sec of arc. Because the Moon rotates at the slow rate of about 0.6 sec of arc per sec of time, the rate of star-rise and star-set is relatively slow compared to the situation on Earth. The occultation angle of 0.5 sec of arc will be covered in about 1 sec of time, which will permit the accumulation of a large number of counts by a detector a few square feet in area. The counter would be collimated to restrict the field of view in the vertical direction, but would accept a wide field in the horizontal direction in order to scan a broad band of the sky.

FOCUSING X-RAY OPTICS

Introduction

Focusing x-ray optics have already been used successfully for solar observations. Such telescopes can now be utilized for the observation of cosmic x-ray sources. In principle, they can produce images of cosmic x-ray sources with angular resolutions comparable to what can be achieved in visible light. In addition, they can focus a parallel beam of x rays impinging on a large collecting area on to a small target area, where it can then be detected with a much improved signal-to-noise ratio. The latter application permits the use of dispersive techniques for high-resolution spectroscopic measurements, and affords new possibilities for polarization measurements. The x-ray reflecting telescope should prove to be a powerful tool for the study of galactic and extragalactic sources at wavelengths greater than 1 Å.

The Working Group therefore recommends the following:

(i) experiments utilizing focusing x-ray optics now at hand should be flown on pointed rockets and satellites currently available, or being developed;

(ii) efforts should be made to improve the present rocket and satellite pointing controls to reduce the limit on pointing jitter to 5 sec of arc or less;

(iii) plans should be initiated for orbiting an x-ray telescope of greater focal length than can be accommodated in present satellites. Lengths in the range of 30 ft to 100 ft are considered necessary to achieve the desired sensitivity and resolution. Unfolding of an extensible system in space may be possible.

Nature of X-Ray Optics

The optical devices for focusing x rays consist of reflectors on which x rays impinge at small angles and are totally externally reflected. Total reflection optics are essentially achromatic at wavelengths longer than a certain cutoff value that depends on the atomic number of the reflecting material. The lowest cutoff wavelength that can be achieved at present is at 3 or 4 Å. This limit can probably be extended to about 1 Å. Reflection optics do not substantially alter the spectral shape and the state of polarization of the impinging radiation.

Imaging Systems. Two or more reflections are necessary to remove first-order aberrations of a focusing system. The reflecting surfaces of the objective were a paraboloid and a hyperboloid. A ray diagram for these two surfaces is shown in Figure 4.

For most cases in which imaging systems are used in practice, the lower limit on the intensity of a discrete source that can be detected by an x-ray telescope is set, not by the signal-to-noise ratio, but rather by the requirement that a significant number of photons be detected within an image resolution element. This can be understood if one considers that the area of a resolution element in the focal plane is many orders of magnitude smaller than the area of collection. The part of the background that depends on the area of the detector element, such as that produced by cosmic rays, will therefore be reduced by a corresponding factor when compared with detectors with only mechanical collimation, and can be further reduced by increasing the angular resolution of the device. The diffuse cosmic x-ray flux is much weaker than the background from other

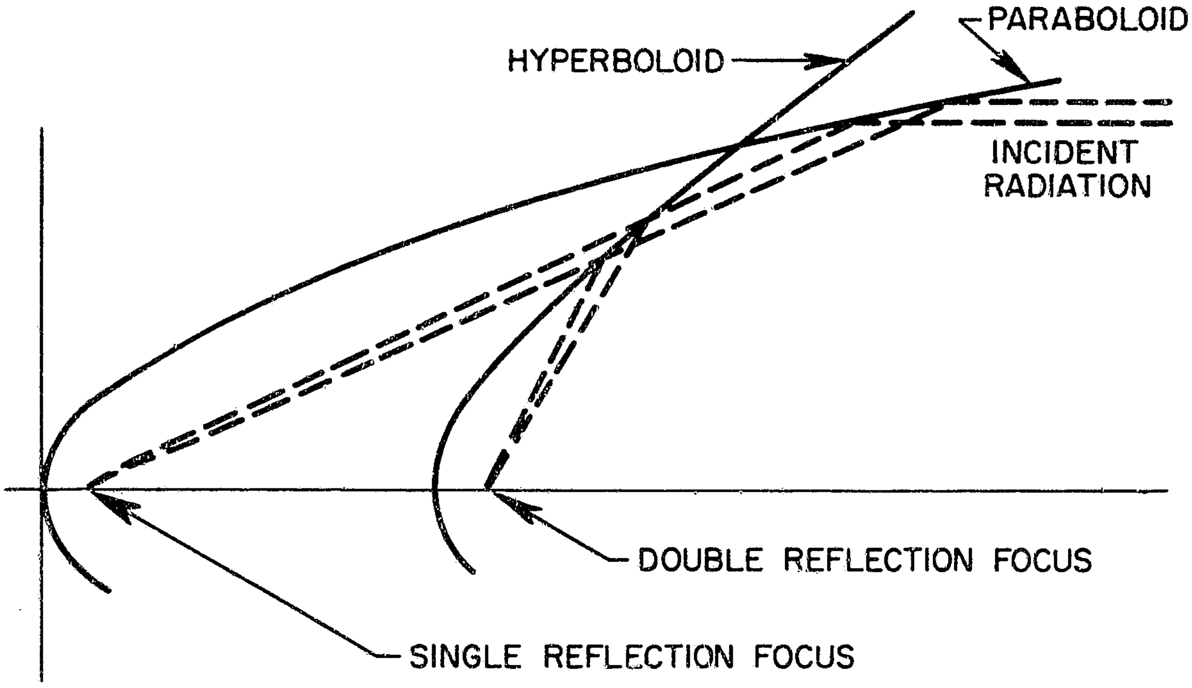

Figure 4. Schematic representation of the optical principles utilized in the grazing incidence x-ray telescope. (Courtesy of American Science and Engineering, Inc.)

factors. In view of these considerations, the effective sensitivity of a focusing telescope is proportional to At, the product of the area of collection and the time available for observation. The sensitivity of mechanically collimated detectors, which is determined by the signal-to-noise ratio, is, on the other hand, proportional to (At) 1/2.

Telescopes of moderate dimensions (2 meters focal length) could permit in one hour of observation the imaging of sources 10^{-2} or 10^{-3} times the intensity of the Crab, with 5 sec of arc resolution. Telescopes with focal lengths of 30 meters or more should permit an increase in sensitivity to the point where sources with intensities about 10^{-6} of the Crab may be detected.

One-Dimensional Focusing System. To conduct surveys for either discrete or extended x-ray sources in the low-energy (<12 keV) range, one may use focusing systems with line-shaped fields of view. A large field of view in one dimension can be obtained with a nest of curved mirrors, arranged somewhat like a simple Venetian blind. Both high sensitivity for detecting discrete sources and precise positional information in one coordinate may be obtained with such a system. Its signal-to-noise ratio lies between that of the large-area detectors with cellular collimators and that of the imaging telescopes discussed above. For medium-sized instruments and with moderate observing times, the sensitivity may be comparable to that obtained in a full imaging system.

Detection Techniques. Although conventional x-ray detectors can be used with focusing x-ray optics in certain applications, the highest resolution will be realized with photographic recording or electronic imaging systems. Of these two, photographic recording, which allows longer integration times and extremely high resolutions, may well prove necessary to realize the full capabilities of a very large high-resolution system. This may require the use of a man to retrieve the photographs. Electronic image detectors, which are used at present in other branches of astronomy, are more sensitive than photographic emulsions and may ultimately be capable of comparable resolutions in the x-ray region. Their principal limitation may be the large rates of data transmission that they require.

Investigations Using Focusing X-Ray Optics

In this section, different kinds of problems are matched with vehicles and other conditions needed to solve them, with special reference to x-ray optics.

In general, one can say that for most problems satellites will prove superior to sounding rockets, except in flexibility and cost. In particular, the long integration times possible with a satellite will allow the detection and spectral investigation of sources orders of magnitude fainter than can be done from rockets.

Several significant experiments can be carried out immediately with existing focusing x-ray telescopes mounted on pointed rockets and satellites, even though pointing accuracy and stability are still limited. Pointing accuracy and stabilization much better than are now available and longer focal lengths will, however, be needed to make possible detection of much fainter sources and detailed observations of their structure. Initially, pointing and stabilization should match the 5 sec or better angular resolution, now in view for shorter-focus telescopes, but design goals of 1 sec of arc pointing accuracy and stability and a 30-meter focal length may be achievable in the next decade. The NASA Orbiting Astronomical Observatory (OAO) satellites and the Apollo Extension Systems (AES) program (perhaps with a gimballed or tethered telescope) would lend themselves to achieving these goals.

Angular Size and Location of Sources; Surveys for Weak Sources. Focusing optics with focal-plane modulation grids can be used on rockets with modest pointing accuracy (0.25°) and moderate drift rate (0.25° sec^{-1}) to measure the size and location of all known sources, as well as somewhat weaker sources, with an accuracy of perhaps 5 sec of arc. Satellites with comparable accuracies and drift rates would reach two orders of magnitude fainter, and so would detect many weaker sources. Such measurements, however, would not define the structure of the sources. Surveys for weaker sources, or of the structure in the diffuse flux, could be carried out with the one-dimensional or fully focusing optics with a finer angular resolution than that attainable with cellular collimators.

Structure of Sources. With rockets, the internal structure of x-ray sources can be studied using photographic image recording, but the resolution would be comparable only with that of the rockets' pointing stability. Electronic image recording techniques might be able to compensate for jitter in pointing. With a jitter of 1 min, the structure of only the strongest known sources could be observed; with 5 sec, all known sources could be studied, and weaker ones as well. The angular resolution in images obtained from satellite instruments now seems to be limited only by the pointing jitter of satellites of, e.g., the OAO type. If jitter is reduced sufficiently, however, the limiting factor will be the resolution of the detector. Photographs, which are now superior from the standpoint of combining image resolution, integration of weak fluxes, and compactness of information storage, can of course be recovered from rockets; but their use in satellites to realize their full potentialities will require the development of a film recovery system, perhaps based on the use of man in space.

Spectral Investigations. The availability of focusing optics opens up new possibilities for the measurement of the spectra of discrete sources with high spectral resolution. The combination of large collecting areas and dispersive optics permits an analyzed beam to be detected by a very small and well-shielded detector, the background counting rate for which can in principle be made negligibly small. The characteristics of available spectrometer designs indicate that an energy resolution of 1% can be attained from satellites. The stronger sources should be accessible to measurements made from rockets with such techniques.

Polarization Studies. Experiments to measure polarization of x rays based on Thomson scattering in a target of liquid hydrogen or lithium hydride are being prepared for use on rockets. Equipment utilizing the Borrman effect is now under development and may prove capable of determining the amount and direction of polarization. The use of x-ray optics in conjunction with polarization analyzers would of course considerably improve the signal-to-noise ratio. Experiments that appear marginal with rockets could be performed with confidence from a satellite, which according to our best estimates should allow the detection of polarization as weak as 1% in all known sources.

GAMMA-RAY ASTRONOMY IN THE RANGE 0.1 - 30 MeV

Introduction

Experiments in the next few years over the energy range 0.1 - 30 MeV will continue to be of an exploratory nature, designed to search for point sources and gamma-ray lines, and to study the diffuse component. Important observational attempts, particularly on known x-ray or radio sources, will soon be made from balloons. Experiments designed to study the diffuse component or to accomplish a sky survey must be conducted from a vehicle with minimum capabilities at least equal to those provided by Explorer-class satellites.

The Working Group recommends that:

(1) Continued support be given to the development of detection devices using actively shielded collimators or Compton scattering telescopes, as well as for the development of promising new techniques;

(2) Space flight assignment be given only to instruments which indicate clear improvement in background reduction, energy resolution, or angular definition, and which have been proven on balloon investigations;

(3) Greater support be given to balloon programs as well as Explorer-class satellites, to implement observations in this energy range;

(4) Support be provided for the continued study of solar x-ray and gamma-ray spectra during both quiet and active phases of the solar cycle.

Gamma-Ray Detector Technology

In the 0.1 to 30 MeV energy range, the short wavelength of the photons precludes efficient broadband focusing or interference techniques. Therefore, one must use detectors which depend on scattering or absorption phenomena. At energies above a few MeV detection devices which utilize the pair production process become practical.

Currently, three combinations of Na I or Cs I scintillation counters are being used by workers in the field. Single omnidirectional counters were used on Ranger III and OSO-I. A combination of counters known as a Compton coincidence telescope was used on the OSO-I to form a directional counter with about 10° angular aperture. This failed to give significant results because a high flux of background gamma rays was produced by cosmic rays interacting with the local matter of the satellite. A directional counter, with an active collimating shield of Cs I, has been flown on the OSO-II, and is planned for use on the OAO-A and the OSO-C as well as for use on a number of balloon observations. Pair spectrometers, which may be designed to give good angular resolution as well as energy resolution and background rejection, have not yet been employed for gamma-ray astronomy observations.

Energy resolution gamma radiation is limited by present counter technique to about 5 or 10% for gamma energies near 1 MeV. Past experiments have not even taken advantage of this inherent resolution. However, the best prospects for a significant improvement in resolution, barring a major breakthrough in scintillation counter technique, lie in the development of solid-state counters with large volumes. Present devices have volumes up to 10 cm^3, and resolutions of better than 1% at 1 MeV. If volumes of 100 cm^3 were available in solid-state detectors without a corresponding loss in stability or linearity, these devices could replace the scintillator-phototube combination in many instruments. Utilization of the resolution inherent in semiconductor devices will also require the development of space-qualified multichannel analyzers of high precision.

Angular resolution of present and immediately foreseeable detectors is about 10° or a solid angle of 0.1 ster. Devices such as the Compton telescope or the shielded detector with an active collimator can be extended to an aperture of a few degrees, or about 10^{-2} ster. It is not clear at present what techniques may be employed to achieve resolutions of minutes of arc in gamma-ray detectors.

The reduction of the background counting rate in this energy range is closely associated with the angular resolution problem. The background has three principal sources: (1) secondaries produced by cosmic rays interacting with the matter in the instrument or nearby; (2) incomplete rejection of the gamma rays arriving in directions other than through the forward aperture; and (3) production in matter, other than the source under observation, within the forward aperture of the instrument. The last item may include instrumental matter, residual atmosphere above a balloon, or even production in the Galaxy or metagalaxy. Anticoincidence techniques must therefore not only reject direct particle effects, but also the effects of unwanted gamma rays outside the instrument aperture. The rejection efficiency must be particularly good in the case of devices with low efficiency for detection, such as Compton telescopes or pair spectrometers. It is important that as instruments of smaller angular aperture are constructed, the counting rate from background effects be reduced in proportion to the reduction in the solid angle. Of course, sources with angular sizes smaller than the effective instrument aperture may be studied by scanning the source until enough events have been obtained to perform a statistically significant background correction.

Future Developments. In the next three to five years, the development of present devices will probably be carried to its technical limit. Areas of single detectors appear to be limited to several hundred square centimeters because the background detected in coincidence devices tends to increase faster than the area, and the dead time due to active anticoincidence shields tends to offset the gain in area. It is therefore usually desirable to increase the observing time rather than the area.

One may visualize, for example, groups or clusters of shielded detectors. Arrays of scintillators or solid-state counters may be connected as a Compton coincidence or pair-spectrometer hodoscope to observe over many elemental solid angles simultaneously. Although technically difficult, spark chambers may eventually be applied to this energy range.

Furthermore, the search for new detector techniques will continue. Progress in this regime of the gamma-ray spectrum will most certainly follow improvements in detector technology.

Vehicles for Experiments

Rockets will generally not be useful in this energy range, chiefly because of the difficulty of mounting large-area detectors on small rockets. Therefore, exploratory observations must be made from balloons and satellites. The balloon will be a major vehicle for certain specific observations and detector studies. Extended observations and sky surveys must be made from satellites. Although present detectors have weighed 20 to 75 lb, counter arrays weighing several hundred pounds may be anticipated. Owing to the constraints imposed on satellite construction, in order to reduce the background, particularly with low-efficiency devices, satellites of the Explorer class may offer particular advantages.

Each new satellite mission should be a refinement over previous missions. In addition to providing new observations, each new mission should be designed to provide information required to instrument succeeding satellites.

An orbiting gamma-ray observatory must be equipped with booms capable of supporting relatively large detector masses distant from the satellite body. Unlike the situation in optical astronomy, it is not likely that the field will have developed so that large general purpose instruments can be reasonably planned within the next decade.

Use of Man and Manned Missions

During the next few years the important observations in the energy range 0.1 to 10 MeV will be made from balloons and Explorer satellites. The use of man and manned space vehicles may permit important supplemental observations and may allow certain instruments, possibly not feasible otherwise, to be launched. These missions should in no way be regarded as a substitute for the balloon and satellite work, but would in fact rely heavily on experience gained with such vehicles in order to provide for an efficient utilization of the Saturn capability.

The use of large manned observatories operating in this energy range cannot now be planned. Observations from the lunar surface have the disadvantage of operating in a high-background situation, and imposing considerable restraints on the data acquisition.

The Saturn capability may be used either in a manned or unmanned mode, or for the injection of special-purpose, rather self-contained payloads into orbit. In most cases it will be necessary to separate the instrument from the large vehicle mass, and to provide an orbit either below the trapped radiation zones, or beyond the magnetosphere.

The manned capability inherent in the Apollo Extension Systems may be used in conjunction with special devices, such as a Compton telescope array, for example. The astronaut might remove the large subassemblies of such a system from the instrument compartment, assemble them, remove the assembled instrument to a large distance from the spacecraft, and align it coarsely, so that an automatic tracking system may then acquire a celestial object of interest. The data could then be telemetered to the ground, where scientists could interpret the data in nearly real time. The astronaut could be instructed as to when calibrations, say, with a radioactive source, are required or when enough events have been accumulated to form a statistically significant measurement. It may be possible to carry out simple changes in the geometry or logic of the instrument. In this manner the astronaut would serve to provide flexible and intelligent control of the experiment.

GAMMA-RAY ASTRONOMY IN THE RANGE ABOVE 100 MeV

Introduction

No definite flux of photons in this energy range has been detected. Because of the low photon intensities and large background radiation, only upper limits to the flux have been determined. In addition, the ratio of the radiation from point sources to the diffuse radiation may be very small, making the detection of point sources all the more difficult.

With respect to the experimental aspects of high-energy gamma-ray astronomy, the Working Group makes the following recommendations:

(1) Instruments with a large collection factor (area x exposure time) should be developed which would provide angular resolutions of better than 1°, which could determine the energy spectrum, and which would possess a proven capability for background rejection;

(2) Experiments should be undertaken in balloons to search for point sources and to define the design requirements for satellites;

(3) Since measurement of the diffuse radiation flux would be a significant experiment and could be performed in the immediate future with small Explorer-type satellites, it should be attempted.

(4) Since experiments to study spectral composition and directional structure of the diffuse flux and to measure weak fluxes and spectra from discrete sources will ultimately need a large orbiting vehicle, one of the forthcoming large orbiting vehicles should be scheduled for this purpose.

Distinguishing Features of Gamma-Ray Measurement

Low Quantum Rate. In contrast to x rays, ultraviolet light, visible light, and infrared and radio waves, the total primary flux of high-energy gammas—including both the diffuse radiation and all point sources—is known to be no more than a few quanta per square meter per second and per steradian. A particularly important quantity to measure, namely, the diffuse flux from interstellar collisions of cosmic rays with gas atoms in the plane of the galaxy, is expected to be about 10^{-4} cm^{-2} sec^{-1} ster^{-1} in the direction of the galactic center. A substantial portion of the sky has already been scanned for point sources, with none appearing at intensities as high as 10^{-3} cm^{-2} sec^{-1}. Optimistic theoretical estimates for the expected flux values yield only 10^{-5} cm^{-2} sec^{-1} for the most favorable source (the Crab Nebula), and realistic estimates for the various probable sources are in the range 10^{-7} to 10^{-8} or less. Surprises may appear, but these figures determine the sensitivity thresholds that are likely to be found necessary for detectors.

Potentially Large Background. The primary flux of charged cosmic rays is at least 1000 times that of the primary high-energy gamma rays. Furthermore, in the atmosphere and in the walls of a vehicle or the detector itself, these charged particles are efficient generators of secondary gamma rays which (except for the directionality of radiation from point sources) are indistinguishable from the primary gammas one hopes to detect. Associated with this secondary production is the likelihood of long-range penetration and large-angle scattering, which rules out simple collimating schemes. In order to detect sources as weak as 10^{-7} cm^{-2} sec^{-1}, one will need a rejection factor for such general background on the order of ten million; and to reach a sensitivity of 10^{-8} the rejection factor must be 100 times higher.

Diffuse Flux. In addition to the radiation from point sources, a diffuse radiation is expected. The measurement of the diffuse flux will be of profound significance, particularly in revealing the composition of intergalactic space, but also for seeing the structure of the Galaxy, especially its central part. The ratio of diffuse flux to point source strength will probably be substantially higher than it is for x rays and radio waves, which makes moderately good angular resolution a requisite for distinguishing point sources. In particular, the diffuse flux is expected to be between 10^{-5} and 10^{-4} cm^{-2} sec^{-1} ster^{-1}. For comparison, the current experimental upper limit is 3×10^{-4}. To observe a point source producing 10^{-7} cm^{-1} sec^{-1} against this diffuse radiation, angular resolution of about one degree is needed. Furthermore, in order to resolve the expected structure in the Galaxy, the angular resolution of the detectors should be no worse than 0.5°.

Discriminating Type of Detector. A high information content per recorded particle is necessary to attain the large (factor 10^7) background rejection which is required. Simple Geiger counter or scintillator arrangements have not yet proved satisfactory. Pictorial instruments (emulsions, spark chambers, etc.) suggest themselves. There is currently under development an instrumental system combining spark chambers and emulsion. The gamma rays convert to electron-position pairs in the emulsion, which are in turn detected below in the spark chamber, triggered by a Čerenkov counter. A suitable anticoincidence system eliminates charged particles. The advantages of the instrument are, 1) the large collecting areas possible, but at the same time, 2) an angular resolution of about 1°, because the point of the conversion of the gamma ray can be seen in the emulsion, and 3) the possibility of using a thick converting layer without loss of information. Background events may also trigger the spark chamber, but there can be no confusion once the tracks are identified in the emulsion. The need for rapid analysis of large numbers of events, mainly background on which the sought-for flux is superimposed, calls primarily for electronic ingenuity and built-in detector selectivity. Experience in the solution of similar but more serious problems in high-energy nuclear physics suggests that this one is probably easily solvable. Any system in which emulsions are involved will of course need to be recovered and will have a limited exposure time in orbit. This suggests that, in addition to balloons and rockets, the development and use of recoverable satellites should be encouraged.

Long Exposure Times. Precise values cannot yet be given for the required product $At\epsilon$, where A is detector area, t the time of observation in a particular direction, and ϵ the detector efficiency: it is the function of balloon experiments in the first phase of these studies to define this requirement more closely. However, indications are that in order to detect point sources, this product may have to exceed 10^9 cm^2 sec, and since ϵ cannot exceed 1/3, the short duration of sounding rocket flights would require areas so large (1000 square meters) that rockets are clearly ruled out as appropriate vehicles for this purpose. Balloons limit the observation of single directions to about 10^4 sec per flight, and on this account as well as the enhanced background effects in the atmosphere, the use of balloons will limit the sensitivity to about 10^{-6} cm^{-2} sec^{-1} for point sources, although even this value requires a rather large and heavy detector (a square meter of active area, probably weighing at least 1000 lb). After preliminary work with balloons, it will be important to take advantage of the combination of long observation time and low background possible in orbiting vehicles, from which sensitivity limits as low as 10^{-8} cm^{-2} sec^{-1} are possible, using detectors that can now be built.

Fairly Large Weight. It is essential that the detectors provide energy resolution. To do this, the apparatus for measurement of high-energy gammas must be dense, and the dimensions anticipated in meters. Experiments may be expected to grow gradually in size and weight from present levels of 500 lb weight and 1000 cm^2 area to future experiments with sensitive areas of at least 10 m^2 and a weight of 10,000 lb.

Angular Resolution and Orienting Ability. It follows from the foregoing conditions that angular resolution of about a degree is necessary, and for some purposes 0.5° would be highly desirable. Some types of apparatus currently visualized will require only a record of their orientation, instead of active pointing. Future equipment, however, will require orienting and stabilizing mechanisms. Precision better than 0.5° is not likely to be required in the near future, largely because known means of detection cannot define the angles of gamma rays more closely.

Prospective Program

Use of Balloons and Small Satellites. The upper limit to the strength of high-energy gamma point sources set by observations so far range from 10^{-4} to 10^{-2} photon cm^{-2} sec^{-1}, whereas balloon experiments are capable of achieving sensitivities as low as 10^{-6}. Hence, an extension of present exploration to several orders of magnitude fainter is possible by this means. It is quite possible that discrete sources with intensities in the range 10^{-6} to 10^{-4} photon cm^{-2} sec^{-1} exist. It certainly seems advisable to pursue this exploratory path, at least for the next few years— partly to discover any sources that may exist, but also partly to define the design requirements for eventual much more expensive experiments from outside the atmosphere. From within the atmosphere it is very difficult to determine the diffuse part of the primary high-energy gamma intensity, because the atmospheric source is also diffuse and overwhelmingly strong; but it would be at least possible to lower the upper limits on the diffuse cosmic component somewhat. On the other hand, measurement of the diffuse component is one of the significant things that is simple enough to put on a satellite of the Explorer type in the immediate future.

Ultimate Need of Large Orbiting Vehicle. With the lower background and longer exposure times possible in orbiting vehicles outside the atmosphere, two important things can be accomplished: (1) the diffuse gamma flux, its spectral composition, and its directional structure can be determined, thereby mapping the quantity which is the product of gas density and cosmic-ray density over the Galaxy and also its average value in intergalactic space; and (2), discrete sources to a level between 10^{-6} and 10^{-8} cm^{-2} sec^{-1} can be observed, in which range it is very likely that a number of sources will be found. Taken together with the radio, optical, and x-ray data from such discrete sources, the gamma-ray spectrum will unambiguously determine the radiation mechanisms, as well as many of the critical parameters specifying the physical conditions in those remarkable objects. The greatest sensitivity mentioned here as being possible may even be high enough to detect gamma rays from the strongest quasars.

It should be made clear, however, that the attainment of sensitivites as high as 10^{-8} requires a fairly large-scale effort: e.g., the product At must then exceed 10^{11} cm^2 sec, as could be obtained with an apparatus having a sensitive area of 10 m^2, directed at single sources for exposure times of about two weeks. This would be pointed only in directions where optical, radio, or x-ray data had indicated the presence of an interesting object to be investigated. A Saturn-boosted vehicle would be appropriate for these purposes, and the men on board could exercise intelligent control of the observations if that should prove helpful.

5. VEHICLE REQUIREMENTS

THE ROLE OF BALLOONS

Introduction

The Working Group recognizes that balloons will be essential vehicles for astronomical observations in the x-ray and gamma-ray region during the next several years. Because of the relative simplicity and comparatively low costs of balloon research and because of the comparatively short times required to complete an experiment, balloons will also play a vital role in enabling universities to participate in such research and in attracting graduate students to the field. The Working Group therefore recommends that balloon experiments and balloon vehicle development be made an integral part of the space astronomy program, and that the support of these efforts be expanded to meet the scientific needs which are now apparent.

Advantages of Balloons

The value of balloon observations in x-ray astronomy was demonstrated by the recent measurement of the spectrum of high-energy x rays from the Crab Nebula, carried out with simple apparatus in a balloon flight at 130,000 ft. Similar flights with more sophisticated equipment at this and higher altitudes can yield a complete sky survey of discrete sources of x rays with energy greater than 20 keV, and can determine the precise source locations, spectra, and angular sizes. Because of the long observation times which balloons provide at a relatively low cost per flight, balloon experiments are the best means for attaining these objectives during the next few years. They have the advantage of short lead times, amounting to only a few months, so that they can be planned and executed in quick response to new discoveries and technical developments.

Moreover, balloons are invaluable for two other major reasons: as vehicles for testing ideas and equipment for later use in satellites, and as a means of enabling universities to participate in the program. Indeed, the latter point is a crucial one: if any but a very small number of universities are to have a direct part in the NASA program, balloons are the solution. The time-scale of experimental development can be reasonably short compared to the average time of residence of a graduate student, so that he can see a piece of research work through from beginning to end, and most of the instrumentation can be handled by a well-equipped university machine shop. But it is more than simply a question of whether universities can participate or not; there is the problem of attracting students into the field. Indeed, many of the most competent investigators now engaged in satellite and rocket experimentation gained their early experience in balloon work.

Need for Further Fundamental Balloon Design Studies

The minimum altitude for useful x-ray observations is approximately 130,000 ft. At the present time flights to this altitude with scientific payloads of up to 200 lb are routine and reliable. Flights above 140,000 and even 150,000 ft have been achieved, but only at much greater cost and with much less reliability. On the other hand, several current lines of technical development, both in balloon materials and in structural design, give promise of major increases in the performance and reductions in the cost of balloons for these extreme altitudes. A relatively small development effort, properly guided by qualified balloon engineers and scientists, should be able to achieve these improvements within a short time and thereby greatly enhance the already substantial advantages of balloon observations in x-ray astronomy.

Balloon Instrumentation

Several of the x-ray balloon observations for which a scientific need can be seen at the present time will require certain instrument items in common. These should be engineered and produced so that they are available to the various interested investigators. One such item is a star-guided orientation control system with 1 min of arc accuracy or better, together with an appropriate telemetry and command system.

As with x-ray astronomy, in the field of gamma-ray astronomy balloon observations have been and will continue to be important in the search for discrete sources at increasingly lower thresholds of detectable intensity, as well as in the development and testing of instruments to be used in satellite experiments. New techniques that have been developed recently for suppressing interfering background counts in the energy range from 0.1 to 10 MeV should be fully exploited in balloon experiments, for example, those aimed at the detection of radioactively produced gamma rays from supernova remnants. As for observations of gamma rays in the energy range above 10 MeV, there is general agreement that the next significant exploratory step beyond those taken with the present generation of gamma-ray telescopes will require the construction of instruments with about 1° angular resolution and sensitive areas of over 1 m^2. These can and should be used first with balloons at the highest attainable altitudes to search for discrete sources. Since the intensity of background high-energy gamma radiation generated in the overlying atmosphere varies in direct proportion to the pressure altitude, every increase in altitude will lower the threshold of detectable source intensity. These measurements will provide essential data needed in the design of the large area instruments that must ultimately be flown in satellites. Instruments with the necessary area, resolution, and discrimination will almost certainly weigh over 1000 lb. Thus a need exists for the development of balloons capable of lifting such massive experiments to altitudes above 130,000 ft.

Need for Expanded Launching Facilities

During the past year the rate of scientific balloon flights has overtaxed the capacity of the National Center for Atmospheric Research facilities at Palestine, Texas, where most of the x-ray and gamma-ray flights are now launched. The prospect for the next three years is an increasing demand for flights not only for x-ray and gamma-ray astronomy, but also for other fields of research. These demands cannot be met without a substantial increase in the physical plant at Palestine and in the personnel involved in launch operations.

Summary of Recommendations

In view of the anticipated needs of x-ray and gamma-ray astronomy for flights with currently available balloons, and for flights with balloons of higher performance characteristics that lie within the immediate range of technical development, the Working Group recommends the following:

(1) Increased support for balloon experiments;

(2) An increase in the number of authorized flights to accommodate an estimated 40 experiments in x-ray and gamma-ray astronomy per year;

(3) Funding of a three-year program for the development of two types of reliable balloons with the following performance characteristics:

- (a) 250 lb of scientific payload to be carried to 145,000 ft;
- (b) 2000 lb of scientific payload to be carried to 130,000 ft.

Attainment of these objectives will require fundamental advances in balloon technology.

The developmental program should be the responsibility of an experienced group of balloon scientists and engineers, preferably in the National Center for Atmospheric Research, and is expected to cost a total of about $2.0 million for the three-year period.

(4) Funding of engineering development and prototype construction of a commanded star-guided orientation control system capable of better than 1 min of arc accuracy.

THE ROLE OF ROCKETS

The discovery of the first discrete x-ray sources was accomplished with a rocketborne instrument and it is expected that a very substantial fraction of x-ray astronomical research will continue to use sounding rockets. Rockets, balloons, and satellites supplement each other. Each has an important role that may change. At present, the Working Group notes the following important points concerned with the role of rockets in x-ray research:

(1) Like balloonborne instruments, but in distinction to satelliteborne instruments, rocketborne instruments can be conceived, built, and flown on a relatively short time scale. New developments can be investigated promptly. This has the important function, among others, of ensuring that the more elaborate and more expensive satelliteborne experiment will be as technically advanced as possible.

(2) The relatively short flight time in rocket experiments is partially offset by the fact that the instruments are recoverable for further flights.

(3) X-ray technology will certainly continue in a state of rapid development and innovation. Rockets provide an ideal tool for the trial of new instruments before assignment to a satellite.

(4) Competent experimental groups entering the field of space research can gain valuable practical experience through carrying out rocket investigations before attempting satellite investigations.

(5) Rocketborne experiments are much more adaptable to graduate student training than are satellite-borne experiments. A graduate student has a reasonable chance to participate in the conception, construction, flight, and data analysis of a rocket-borne experiment. This degree of participation is fast becoming practically impossible in the case of satellite research, possibly excepting the simplest kind.

The Working Group, therefore, makes the following recommendations:

(1) Over the next several years rockets will continue to be a primary x-ray exploration vehicle. More groups may be expected to participate in this important work and should be encouraged to do so. The number of rockets currently available—about six per year—is barely adequate for even the present active groups. At least a dozen rockets each year is considered essential for maintaining an appropriate pace of scientific progress and as necessary preparation for more ambitious satellite missions.

(2) As the work progresses, increased capabilities of rocket vehicles are required. An increased volume and weight-lifting capacity beyond the Aerobee class of rockets appears desirable for survey experiments. Existing military rockets should be exploited for this purpose.

(3) Greater accuracy of pointing is required to exploit the fine angular resolutions which can be achieved with currently available detectors. Pointing accuracies finer than 0.25° and small jitter rates are essential. Pointing accuracies of 1 min of arc have been achieved in developmental systems. Such systems should be made available for use in x-ray astronomy and their characteristics improved, particularly with respect to jitter or drift rates.

THE ROLE OF SATELLITES

Explorers

In comparison with larger more complicated satellites, small relatively inexpensive satellites of the Explorer class, which are often designed to carry out a single experiment, will continue to have an important role in x-ray and gamma-ray astronomy. These satellites offer flexibility in that an initial important discovery made from balloons or rockets can be followed relatively quickly with more refined observations from an Explorer. In addition, since an Explorer satellite often contains a single experiment or a small group of compatible experiments, greater design flexibility can be maintained during the program and a greater degree of control on the mission can be exercised by the experimenters, than in the more elaborate scientific satellite programs (though not greater than in rocket and balloon flights). These same characteristics—relative flexibility, shorter lead times, greater scientific control—allow more opportunity for graduate student participation in this exciting field of research. For all these reasons, these satellites allow the greatest advantage to university groups.

Weight and space limitations probably exclude programmed pointing equipment from these vehicles. It would seem therefore, that this type of satellite will be most useful for surveys in which the modest detector areas can be compensated for by long exposure times. It is impossible to foresee the ingenuity that the Explorer type of satellite opportunity will stimulate. The Working Group, therefore, strongly endorses the continued encouragement of groups who wish to conduct their research in this more independent manner.

Pickaback Experiments on the Saturn Instrument Unit

Experiments carried aloft while attached to the instrument unit of the Saturn rocket offer advantages intermediate between those of rockets and Explorer-type satellites. The experiment module can have a very simple interface with the spacecraft electronics. The experiment orbits the Earth in operation for about five hours and remains under the orientation control of the Saturn guidance system. Important advantages offered by this procedure are: (1) the small incremental launch effort; (2) the relatively large number of anticipated launches, spaced by a few months; and (3) the possibility of pointing at specified objects.

Orbiting Astronomical Observatory (OAO)

Only two x-ray experiments and two gamma-ray experiments are scheduled for flight on the series of OAO spacecraft authorized to date. All of these experiments were conceived as pickaback or back-up devices and are in no sense really matched to the OAO characteristics. The Working Group therefore recommends that:

(1) Proposals for x-ray experiments suitable for inclusion as primary OAO experiments be considered as possible substitutes for already accepted OAO experiments in the more conventional fields of astronomy;

(2) The first unassigned OAO be set aside for x-ray observations, to be supplemented if possible by much lower priority gamma-ray observations;

(3) The OAO pickaback opportunities be recalled to the attention of the scientific community and suitable experiments in x-ray or gamma-ray astronomy be accommodated. If at all possible, a minor rearrangement of spacecraft components should be performed in order to permit use of optical focusing devices with focal lengths comparable to the length of the OAO.

RECOMMENDATION FOR A SUPERNOVA PATROL

Supernovae are believed to occur in our Galaxy at a rate of about 1 per 100 years. They are, according to present theories of nucleogenesis, the source of all heavy elements. But observational support for this important conjecture is lacking. The Working Group believes it is important that x-ray and gamma-ray instruments be available for supernova detection, should such a phenomenon occur in our galactic neighborhood. Exisiting programs (the NRL-NASA solar x-ray monitoring satellite, x-ray and gamma-ray detectors on the OSO, OAO, and OGO series of satellites) offer incomplete spectral coverage with uncertain probability of supernova data recovery. The Working Group feels that a promising practical program for passive supernova x-ray and gamma-ray data recovery would involve a small "universal" package containing an x-ray detector sensitive in the 5-8 Å region and a gamma-ray detector capable of energy resolution in the 50 keV - 3 MeV region. Both detectors should be sensitive over a wide angle, and small and simple enough to permit their inclusion on enough spacecraft so that all parts of the sky are covered at least 80% of the time. The Vela Hotel Project, for example, could perhaps be requested to add instruments of these kinds to their standard orbital packages.

The Working Group therefore recommends that support be made available for the design and construction of a prototype universal supernova monitor, and that serious consideration be given for the inclusion of this monitoring device on many future spacecraft. Two levels of sensitivity would be desirable in such a package: one level, sensitive to only very high flux values, would be appropriate to any supernova that occurs in the local Galaxy; the other more sensitive channel would be appropriate for the more numerous supernovae occurring in other nearby galaxies.

6. OBSERVATIONS MAKING USE OF MAN IN SPACE

The Working Group has considered many aspects of the possible use of man in making x-ray and gamma-ray observations in space. The following conclusions emerged from our deliberations:

(1) There are certainly observations in which man will be helpful and even necessary. Two concerns, however, were expressed.

(a) The effort and expenditures for experiments of the type contemplated are great. An appreciable fraction of an investigator's useful life may be invested. It was felt that if an experiment is accepted for a manned mission, some assurance that it would be regarded as an essential objective of the mission is necessary to establish an atmosphere in which scientific and manned space flight interests can be successfully joined.

(b) The cost of manned ventures is very great and can only be charged to the broad national interest in the conquest of space. But it is inevitable that scientific missions will have to bear some of the responsibility for justifying the cost and it is therefore essential that only experiments that are clearly excellent be accepted for Gemini and Apollo Extended Systems.

(2) The Working Group recognizes that it cannot and should not attempt to design and present specific experiments. Nevertheless, we further recognize that only through discussing specific experiments can the general nature of our task emerge. There are clearly many technical operations for man to perform, such as assembly of components and erection of structures, as well as the carrying out of observing routines according to prescribed programs. Such tasks require a trained technician or engineer. Initial observational programs are expected, however, to require responses to be programmed in detail in advance. These require scientific judgment and initiative on the part of the astronaut.

(a) Large area (100 square meters) x-ray detectors equipped with mechanical collimators appear to be the optimum system for certain types of surveys and for occultation measurements. Such a detector could be carried aloft in pieces, assembled, and placed into orbit with its own power supply, guidance, and telemetry system.

(b) Focusing x-ray telescopes appear to be the most promising detectors for detailed studies of source structure and for high-resolution surveys. Large instruments of this type would best be assembled in orbit by a man. Photographic recording has many advantages over electronic methods when large amounts of data must be accumulated, and here man has the potential role of rendezvousing with the telescope to retrieve and replace film, as well as to repair and adjust the telescope.

(c) Gamma-ray detectors for photons with energies of 100 MeV and greater will require very large instrumental components such as spark chambers and Čerenkov counters. The need to assemble them in space is not so clear as it is for x-ray instruments, but their complexity and high initial cost may possibly warrant repair and adjustment by rendezvous with man in orbit.

(d) In the 1 MeV or nuclear gamma-ray region, detectors of modest proportions must be located far from massive objects and pointed in a particular direction (e.g., the Crab Nebula). Complex arrays of these detectors could be assembled, coarsely oriented, and occasionally calibrated by an astronaut.

(e) An x-ray astronomy observatory based on the Moon would benefit from all the obvious advantages of a stable base and the possibility of long-term tracking of sources. The demands on man are similar to those analyzed in great detail in such comprehensive reports as the LESA study ("A Study of Scientific Mission Support of a Lunar Exploration System for Apollo", final report, North American Aviation, Inc., 1965). During the X-Ray and Gamma-Ray Working Group study, Moon-based observations were not considered in detail except for the possibility of the occultation studies described under Nonfocusing X-Ray Optics in Section 4. In that type of observation, man would have several tasks, e.g., to calibrate the x-ray observations by timing the occultations of visible stars with an optical telescope; to determine with auxiliary x-ray detectors the azimuthal position of the occulted source; and to exercise judgment in altering the geometry of the occultation apparatus to gain detection sensitivity at the expense of resolution, or vice versa.

Not only can lunar occultations on the Moon be used to considerable advantage for x-ray observations; such techniques will evidently also be extremely effective in locating discrete sources of high-energy gamma radiation.

APPENDIX: LIST OF PARTICIPANTS

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VI

Physics and Geophysics

1. INTRODUCTION

TASK OF THE WORKING GROUP

The task of the Working Group on Physical Sciences was to review those physical experiments of a basic nature that could be advantageously undertaken in space. These were to be assessed as to their importance, while both short- and long-term technological requirements and practicability were to be borne in mind. The Working Group, by and large, excluded other fields covered in the Summer Study; consultants (see Appendix) were invited to discuss certain specific topics. Thus, the Working Group was almost wholly concerned with the question: "What would physics be like if it had developed in a space environment?"

Note was taken of the main characteristics of the space environment: (1) low temperature, (2) low pressure, (3) weak magnetic field, and (4) zero-gravity field. Some fourteen typical areas or subjects were surveyed: surface physics, plasma physics, low-temperature physics, high-energy physics, magnetohydrodynamics, fluid dynamics, experiments in a weak magnetic field (a few gammas), experiments in a zero-gravity field, experiments in an atmosphere-free regime, relativity experiments, gravitational experiments, fundamental physical constants, antimatter, and planetary investigations dependent upon the establishment of an equipotential reference datum in space. The Group did not consider experiments to map the distribution of magnetic fields and particles in interplanetary space and the more immediate surroundings of the Earth, since these appeared to have been adequately covered in the report of the 1962 Summer Study at the State University of Iowa ("A Review of Space Research," NAS-NRC Publ. 1079, 1962) and subsequent reports, and since NASA has a vigorous program in support of such investigations.

SUMMARY OF DISCUSSIONS

The types of experiments useful to conduct in space should capitalize upon the ambient conditions—zero gravity, low temperature, low pressure, and weak magnetic background. Although many types of experiments can be proposed, most can be coped with at or near the Earth. Thus, for example, one might immediately think of surface-physics investigations, including catalysis, in the ultrahigh vacuum beyond the atmosphere. However, ambient pressures of 10^{-10} torr, at which the formation of a monomolecular layer on a clean surface takes about half an hour, are readily obtained by pumping; and partial pressures of 10^{-12} torr in specific containers are achieved by gettering. Similarly, balloon altitudes are adequate to satisfy the requirements of high-energy physics in the study of nuclear reactions of cosmic rays.

Such considerations lead one to look to combinations of features of the environment. Two that appear of interest are (1) optical experiments requiring a very long path *in vacuo* and (2) plasma-physics experiments requiring a very large volume *in vacuo*.

On the whole, four types or sets of experiments appeared valuable and feasible: relativity and gravitation, physical constants, plasmas, and geophysical height sensing.

Although many significant fields of investigation were recognized and discussed by the Group, the actual number of experiments regarded as feasible over the next decade or two was relatively small. These experiments were classified on the basis of their value to fundamental physics, as having significant value for application, or as having interest but no recognized basic value or application at this time. They were also classified in terms of their specific need for a space environment, their utilization of man in space, if any, and the type of support required.

Relativity and Gravitation

The principal experiments having basic importance in physics lie in the fields of relativity and gravitation. Although most of these experiments have been considered previously by other study groups, the following six were selected for discussion on the basis of their differing modes of approach:

1. The use of the asteroid Icarus or a special satellite for determining the relativistic rotation of the line of apsides (perihelion rotation) of a planet or asteroid; its use to determine the contribution of solar oblateness to the motion of the line of nodes of the orbit.
2. Gravitational deflection of light.
3. The precession of a spinning body in space.

These three experiments represent the complete body of relativistic effects realizable in the near future and capable in principle of distinguishing between (a) Einstein's rather specific Theory of General Relativity and (b) the more general theory of relativity, for which part of the gravitational effects are due to a scalar field. The determination of the Sun's oblateness is essential for the unambiguous interpretation of the excess perihelion rotation.

4. The secular change in gravitational constant through laser tracking of corner reflectors on the Moon to determine lunar deceleration. The experiment would also have geodetic value and geophysical applications to the study of the Moon's interior through studies of its libration. It is also conceivable that this experiment might permit the detection of gravity waves.

5. The use of a time-keeping satellite for studying changes in gravitational interaction.

6. An Eötvös experiment in space for studying the relation of inertial and gravitational mass.

Physical Constants

Experiments leading to the more precise definition of physical constants were discussed. The following were considered to be of interest at this time:

1. The determination of a better value of the gravitational constant through a Cavendish experiment in space. Although this is a dimensional constant, it is important for establishing the mass and gravitational attraction of the planets.
2. The determination of a better value for the velocity of light. Although the uncertainty in this value is of small consequence on the Earth, the degree of uncertainty is of sufficient magnitude to be important in measuring astronomical distances. No specific approaches are noted, and, indeed, the question as to whether space affords prospects better than the Earth remains open.

Plasmas

Space provides a natural environment for studies of plasma physics that are precluded in laboratories on the Earth. In terrestrial laboratories, the probes employed for measuring the properties of the plasma introduce disturbing effects; these effects are obviated by the much greater scale size of the phenomena produced by nature in space. The turbulent structure in the fluctuating transitional region between the uniform interplanetary magnetic field and the magnetosphere affords unique opportunities in the study of collisionless shock-wave phenomena.

Coordinated measurements of the characteristics of solar plasma beyond the magnetosphere, or of the properties of the ionosphere, could be obtained with manned spacecraft that send out maneuverable subcapsules. The distance between the units could be varied at will, and would be governed by the observations. The detailed topography of the ionospheric structure and airglow phenomena could be investigated with this technique. Transient effects, such as sporadic E, would be susceptible to study by laboratory techniques. The propagation of artificially produced plasma could be observed, and it might be possible to affect naturally occurring plasma with waves produced by nonnuclear explosions, by sparks, or by other means.

The behavior of charged particles in magnetic fields, including particle interactions, is far from understood. Much of the work in the magnetosphere is concerned with this subject. Aside from observation and measurements, actual experimentation is possible, as demostrated by the artificial injection of charged particles; radioactive tracers may also afford a useful technique in this work.

Geophysics

If it were possible to develop a precision microwave altimeter and gravity gradiometer, it would be possible to establish an equipotential reference datum in space that would permit a significant breakthrough in many geophysical studies of the Earth. Moreover, the technique would have a direct bearing on the understanding of similar later studies of the Moon and other planets.

Other Experiments

In the course of considering fundamental questions and major problems or techniques, a variety of other experiments entered the discussion. These generally were of a rather more specialized nature. A few are noted here: the interrelations of solid and liquid and gas phases of materials in a space environment, the collection of cosmic dust and micrometeoroids, and the exposure of emulsion plates for cosmic-ray detection. As these experiments have been reviewed in connection with the manned space program (see "Summary of Biomedical Experiments Presently Planned for the Manned Space Flight Program" in Chapter 10), they were not considered further.

The Lagrangian points were discussed, in part because man might be able to observe whether there is, in fact, an accumulation of planetary material at these equilibrium positions. The general topic, examined three years ago at the Iowa study ("A Review of Space Research," NAS-NRC Publ. 1079, 1962, pp. 4-34—4-45), was reviewed. Further theoretical work since that time has yielded additional information on the nature of the stability; no experimental verification of dust accumulation has been obtained. Photography of the triangular Lagrangian points from Gemini and Apollo flights appears feasible.

Such features of space as high vacuum, gravity-free condition, and low temperature prompted speculation on various possible experiments. Two are noted here: (1) an intermolecular scattering experiment, in which a beam of particles is scattered by a gas sample at very low energies (~1 eV); (2) Rayleigh instabilities, using the classical Bénard cell experiment in the gravity-free environment, where an artificial gravitational field would be applied so as to permit a study of instability from onset.

FINDINGS AND RECOMMENDATIONS

Insofar as space studies of fundamental significance in physics are concerned, the Working Group found that relativity and gravitation are the critical subjects. Some work is under way at present; this merits continued attention and support, and, accordingly, the Working Group adopted the following recommendation:

Recommendation 1. The Working Group considers experiments in relativity and gravitation as the most important fundamental ones in physics and recommends their support and conduct as soon as practicable.

Because a precise height sensor and gradiometer afford prospects for measuring a host of significant physical parameters of the dynamic Earth, which might then be applied effectively to the study of other planets, the group concluded that the development of instrumentation was important. To this end, and in the hope that available equipment will be used to begin the program, the following recommendation was adopted:

Recommendation 2. The Working Group recommends (a) that the development of a suitable, high-precision radar altimeter be tackled vigorously, (b) that existing equipment be used on presently available space vehicles to determine the nature of operational problems inherent in the conduct of measurements with a precision of 1 to 2 cm, (c) that the problem of a suitable gravity gradiometer be examined, and (d) that related studies, now under way, for the more precise determination of orbital constants be continued.

Because the possibility of placing corner reflectors on the Moon opens up prospects for a variety of experiments, a third resolution was adopted by the Working Group:

Recommendation 3. The Working Group recommends that, since a set of corner reflectors on the Moon would facilitate a number of significant scientific investigations, NASA should incorporate the installation of such reflectors on the Moon's surface as part of the lunar landing program.

Moreover, some consideration might well be given to the use of reflectors on long-lived satellites.

In common with other working groups in the Summer Study, the Working Group on Physical Sciences reviewed the findings of the ad hoc Committee on NASA-University Relations (the Simpson Committee). The Group concurred in the findings of this committee and urged favorable consideration of the recommendations by the government. In its own study of the general subject, as stimulated by the Simpson Committee report, the Working Group adopted the following resolution:

Recommendation 4. The Working Group recommends that the support of ground-based experiments, and of balloon and rocket programs, be expanded to provide information which is essential to supplement observations carried out with spacecraft or which cannot be obtained by spacecraft. Proper support of this kind is totally justified by scientific and economic considerations. Moreover, an adequate effort would yield significant by-products: it would, in general, permit continuity of university participation in space research and, in particular, encourage the training of graduate students by affording means for following through a space-related research problem from conception to completion.

Again in common with other working groups, the Working Group on Physical Sciences considered the uses of man in space for scientific purposes. Most of the experiments noted by the Working Group lie in the future, do not depend on a man as a manipulator in space, but do depend on further technological developments. Because of this, no formal recommendation is needed, although the general discussion led to some findings.

First, in general, research in space can be characterized in three ways: (a) most of it can be done by remote-controlled or automatic devices; (b) there are, however, some problems that will eventually require man, although science by itself could not justify the costs of manned effort; but (c) if the national interest continues to call for the development of manned capabilities in space, then the most rewarding by-product of the enterprise probably lies in the pursuit of problems as characterized above in, and relating to, (b). Typical of the kinds of major problems to which a man can contribute, because human discrimination, judgment, and guidance appear crucial, are planetary investigations, emplacement and adjustment of large telescopic systems, and some plasma studies.

But, second, although the large cost of space activity rules out the conduct of what we might call secondary or minor problems, these can merit pursuit as incidental to already planned and otherwise justified manned missions, for they can represent valid effort (a) as experiments of interest and (b) as part of the general acquisition of manned space capability. Thus, there are useful experiments under consideration by NASA for manned flights in Earth orbit: most of these relate to medical and physiological subjects of obvious operational value; some relate to engineering problems useful in space technology; and a few take up limited scientific topics, feasible within the framework of the Earth orbit program.

Third, and last, in space as on Earth, scientific investigations will call upon man and device as necessary to the research goal. Accordingly, the distinction or separation of space research into a manned category and an unmanned one is artificial. This suggests that the unity of the scientific effort be maintained, allowing the nature of the problems and economics to dictate how a given task can best be carried out.

2. POSSIBLE EXPERIMENTS IN SPACE

RELATIVITY AND GRAVITATION

The techniques of space are uniquely suitable for research on gravitation and relativity. The very great weakness of gravitation requires large bodies (astronomical in size) as sources of gravitation. Rapidly moving detectors are also needed to measure it. Space-curvature effects are important only over vast distances, and real research in this area can be produced by the physicist only if he is willing to stop viewing himself as a laboratory-bound creature. Perihelion rotation, light deflection, and precession of a spinning top constitute the complete body of relativistic effects realizable in the near future and capable in principle of distinguishing between (a) Einstein's rather specific Theory of General Relativity and (b) the more general theory of relativity, in which part of the gravitational effects are due to a scalar field.

Perihelion Rotation

One of the two most important experiments is concerned with the relativistic rotation of the line of apsides (perihelion rotation) of a planet or planetoid. It has been pointed out (Dicke, 1964) that, because of an unknown contribution from a possibly oblate Sun, this relativistic effect has an uncertainty in excess of 20 per cent. Inasmuch as the uncertainty in the gravitational deflection of light is just as great, there is not yet a single test of the Theory of General Relativity per se that is more accurate than 20 per cent. This statement should not be misunderstood.

It refers to Einstein's specific theory of gravitation, "General Relativity," and not broadly to all generally covariant relativistic theories.

A slightly flattened Sun having an oblateness of $\Delta R/R = 5 \times 10^{-5}$, would rotate the perihelion of Mercury at a rate that is about ten per cent of Einstein's value. This effect falls off with a higher power of r than does the relativistic effect, and the two contributions would be separable in principle by combining data from two planets.

1. The asteroid Icarus will pass relatively close to the Earth twice a year for the next three to four years. It is an almost ideal object for investigating both the relativistic effect and the solar oblateness. Its eccentricity and inclination are large, making the motion of the node and the perihelion large, and observation easy. The motion of the node provides a good test for the oblate Sun, and the motion of the perihelion is sensitive to both the oblateness and relativistic effect. If the asteroid were tagged with a radar transponder, a precise measure of its orbit could be obtained.

2. An artificial planet moving in an elliptical orbit could also be used to observe the relativistic perihelion rotation. There are two necessary conditions: (a) the accuracy of the range measurements must be sufficiently high and the eccentricity of the orbit sufficiently great that the necessary precision can be obtained in the lifetime of the vehicle; (b) either the average density must be sufficiently great that uncertainties in gas drag and light pressure are of negligible importance, or the vehicle must be equipped with a system of accelerometers and gas jets to cancel out these extraneous forces.

Concerning precision: standard radar-transponder techniques should be adequate. For a planet with a period of about one year and an eccentricity of about 0.2 - 0.5, the line of apsides rotates $\sim 2 \times 10^{-7}$ radian/year because of the relativistic effect. This represents a motion of the aphelion of about 30 km/year, well within the capabilities of present techniques for range measurements. It is evident that a useful life of two to three years might be sufficient for the observation.

The uncertainty in the radiation pressure presents something of a problem but not an extremely difficult one. For a reflecting spherical planetoid of radius r(cm) and density ρ(g/cm^3), the ratio of light pressure to gravitational pull by the Sun is 1.2×10^{-4} $1/\rho r$. This force has essentially zero effect on the motion of the line of apsides as long as the heliocentric aspect of the planetoid and its albedo are not correlated with solar distance. This radiation-induced force should be inverse square to an accuracy of a part in 10^6 for $\rho r \sim 1$. For a large, high-density planetoid ($\rho r \sim 200$) the required accuracy is two parts in 10^4.

The pressure induced by the solar wind on the planetoid is much smaller than light pressure, but unfortunately it is not an inverse-square force and it is variable. This force is less than 10^{-3} of the radiation-induced force, but it may be more difficult to deal with.

It is concluded that a dense, sufficiently large planetoid would yield a measure of the relativistic rotation of the line of apsides. However, a better approach might be to add an accelerometer and set of gas jets to servo-balance these extraneous forces to zero. Such a device, called a "zero-g satellite," is now being designed by R. Cannon and collaborators at Stanford University.

Light Deflection

The accuracy of the well-known light-deflection test of General Relativity is at present very poor because of the necessity, up to the present, of an eclipse of the Sun to make the observations. The uniqueness of the event at a given site precludes the necessary careful study of systematic errors while the eclipse is taking place; thus, little reliance can be placed on these observations.

There appear to be at least two ways in which the gravitational deflection of light (or its equivalent) might be determined using space techniques. First, the Sun, and its surrounding star field might be photographed above the atmosphere without the intervention of a solar eclipse. Second, an artificial planetoid might carry a radar transponder or a precision oscillator behind the Sun to permit an absolute determination of the retardation of electromagnetic waves in passing the Sun. The latter is similar to the suggestion of Shapiro, (1964), to use ground-based radar to monitor an interplanetary distance as Mercury or Venus passes behind the Sun.

1. Photographic or photoelectric means of determining the gravitational deflection of light would consist of a camera with an aperture of 5-10 cm diameter, a photographic plate or film (or preferably an image-storing iconoscope), and a quartz flat that could be interposed in front of the objective to photograph a comparison star field on the same film. Photographs would be made every four hours for two or three days. Each photograph would comprise two separate exposures, with the same comparison star field being used on all photographs. It would be necessary to use a Lyot optical system or some system designed to reduce internal scattering. It would also be necessary to stabilize the camera carefully during the exposures.

2. Light retardation in passing the Sun. The radar method suggested by Shapiro is so eminently reasonable that there may not be a need for a competitive approach. However, in an alternative approach, one would fly a precision atomic clock on an artificial planetoid and follow its apparent frequency, radiated to the Earth, as it moves behind the Sun and reappears. Alternatively, a radar transponder could be interrogated from the Earth. To avoid confusion of the interpretation by the retardation caused by the solar wind, it is desirable to work at short wavelengths (under 6 cm), and there may be advantages of the foregoing approaches over straight radar at these wavelengths.

A Spinning Top

It has been suggested by Schiff (1960), that the relativistic precession of the axis of a spinning top, discussed a half-century ago by de Sitter and others, might be detected by using a spinning artificial Earth satellite and monitoring the motion of the spin axis. The expected precession of about 7 sec of arc per year is composed of two parts: (i) the de Sitter precession due to space curvature, and (ii) the precession, arising from the tendency of a rotating mass (the Earth) to tug inertial coordinate frames around with it. This latter effect is particularly interesting because of its close relation to Mach's principle.

Based on a suggestion of Schiff's, Fairbank and Everitt (Stanford) have been designing an experiment that makes use of a cryogenic gyroscope and readout system, and have been engaging in extensive experimentation. The experiment might also be performed using a brute force approach: flying a large dense artificial satellite and monitoring the orientation of its spin axis from the ground. This approach, first suggested by Knoebel (Illinois), also provides some interesting possibilities, and is being studied at the University of Illinois.

Corner Reflectors on the Moon

By landing one to three optical corner reflectors on the Moon, a large number of observers in many countries would have a tool for a variety of interesting and fundamental investigations of the Moon. These include relativistic factors affecting its motion; factors involving the Moon's interior, its rigidity, and its figure; and the excitation and damping of the Moon's physical libration.

Distance to the Moon. A particularly exciting prospect is the establishment of optical bench marks on the Moon—bench marks of long life whose value would increase with age. It is certain that increasingly more advanced lasers will be developed. Unless we start now, there will be no well-defined fixed points on the Moon to look at with these greatly improved lasers. There appears to be no fundamental reason why a measure of the distance to the Moon accurate to one part in 10^{10} could not ultimately be obtained. However, such precision would be meaningless without at least one well-defined fixed reference point.

Secular and Tidal Accelerations. The long life possible for such bench marks is particularly important when problems involving the lunar orbit are considered. The accuracy with which the secular acceleration of the Moon's longitude can be determined is proportional to the 2.5 power of the observation period. Points on the Moon are so ill-defined that at present 50 years of observation are required to determine its secular acceleration with any precision. The greatly improved accuracy that corner reflectors provide would permit the reduction of this observation period to a few years. Thus, it would become possible to determine the tidal acceleration in a comparatively short interval of time.

Gravitational Constant. A fundamental relativistic question concerns the possible existence of a zero-mass scalar field as part of gravitation. If such a field exists, gravitation should be steadily weakening as a direct result of a secular increase in the magnitude of the scalar field, a cosmological effect of the expanding Universe. This, in turn, would imply a secular slowing of the motion of the planets and the Moon (as measured by an atomic time scale). The classical astronomical determination of the lunar acceleration is based on an ephemeris time scale. Thus, a new determination would permit a comparison of the atomic and ephemeris time scales and hence would expose a secular change of the gravitational constant, if it should exist. The expected change (if it exists at all) is an increase of the lunar period of 2-6 parts in 10^{11} per year.

Geodetic Application. Geodesy provides another important application. The distance of any observation station on the surface of the Earth from the axis of rotation can be determined from the observations.

Libration. The physical libration of the Moon could be deduced from the greatly improved measure provided by the distance to three corners. The physical libration, in turn, would provide information about the principal moments of inertia, rigidity, and internal damping of the Moon.

A better assessment of the value of tracking corner reflectors to obtain improved estimates of the physical librations (and hence the principal moments of inertia) should be possible after improved determination of the Moon's gravitational field is obtained from the Lunar Orbiter Satellite, now planned for 1966. Such a satellite may answer the question of whether the present unreasonably large moment of inertia results from an error in $(C-A)/MR^2$ obtained in part from the Moon's orbit.

Gravitational Waves. One interesting relativistic phenomenon of great importance that might show up in sufficiently accurate range measurements to points on the Moon would be the presence of gravitational waves. They might be of the conventional tensor type, or they could appear in the above-mentioned scalar field. These waves might be detected if they were occasionally substantially stronger than suggested by elementary considerations of average energy density.

The existence of tensor-type gravitational waves and radiation are directly predictable from the General Relativity Theory (Maller, 1952; Forward, 1961) and are currently being studied by Weber at the University of Maryland. It has also been postulated recently that gravitational radiation is quantized (the graviton) and possesses the following characteristics: (i) the radiation is of quadrupole type; (ii) velocity of propagation is the velocity of light; and (iii) gravitational waves result from the acceleration of masses.

Unfortunately these effects are exceedingly small and are quite difficult to generate or detect. For example (Maller, 1952), a one-ton rotating mass quadrupole with a rotational frequency of 10,000 rpm would radiate energy at a total rate of only 4×10^{-33} watt. In addition to the problems of constructing such large radiators, spurious vibrations and background noise must be considered.

Numerous experiments have been conducted recently by Weber in an attempt to detect and measure gravitational radiation. Thus far, there has been no empirical verification. The major problem is mechanical background noise introduced through the supports for the detector. The environmental conditions available in a zero-G laboratory aboard a space vehicle or satellite would greatly eliminate this source of noise and would allow the construction of detectors with a much lower operating frequency than those operated on the Earth.

Because the gravitational radiation expected from astronomical sources is expected to be peaked in the very low-frequency region, it is desirable to build detectors with as low an operating frequency as possible. On Earth, however, the noise spectrum due to ground motion also is highly peaked at low frequencies, and it is almost impossible to eliminate these from the supports by filter techniques now being used by Weber at the higher frequencies (1600 cps).

Corner-Reflector Size. How big should the corner be? An accurate (diffraction-limited) corner cube has an effective area proportional to the fourth power of the length, and for a corner 25 cm on a side its effective area would be enormous ($\sim 400\ \mathrm{km}^2$). The light returned would be much greater than the diffuse reflection from the Moon.

Moon Quakes, Libration. An interesting application of the technique would require two or three somewhat smaller corners placed on the Moon's surface a few meters apart. Interference between the two corners would depend upon tilt. Thus, the return signal would be sensitive to tilt of the Moon and would be responsive to Moon quakes as well as to libration.

Clock Rates in Widely Different Gravitational Potentials

According to the general theory of relativity the interval between two successive beats of a clock located in a gravitational potential $V=\int \rho\, d(\mathrm{vol})/r$ is increased by the factor $1 + V/c^2$ relative to the interval for a similar clock in a local inertial coordinate system for which the gravitational field vanishes. As applied to the frequency or wavelength of lines in the spectrum of a star as observed from well outside the star, this effect is the well-known "gravitational red shift." Experiments to measure this effect by comparing the rates of two accurate atomic clocks, one on the Earth and one in space, were proposed during the first years of the space program. But before the proposals could be carried out, Pound and Rebka (1960) performed their well-known experiment, using a gamma-ray emitter and absorber for which the width of the line was sharpened to about 10^{-12} by the Mössbauer effect. The difference in height between the two elements was about 22 m, corresponding to a frequency shift $V/c^2 = 2.5 \times 10^{-15}$. Although they had to apply corrections for other effects much larger than the shift they were looking for, they obtained a result with a 1-σ uncertainty of only ten per cent which differed from the theoretical value by only five per cent. The precision of their technique was comparable to that estimated for the proposed orbiting clock experiments, and their experiment was of course considerably less expensive. Their result is still the most accurate existing confirmation of this particular prediction of general relativity.

Meanwhile, however, the development of atomic clocks, such as those based on the gases of rubidium, cesium, and thallium, has proceeded so far that a considerably more precise experimental check of the gravitational shift can now be performed, taking advantage of a potential difference much larger than that available to Pound and Rebka. Unlike Mössbauer clocks, atomic clocks can be compared by scaling their frequencies and telemetering the accumulated readings over long distances. Present-day rubidium-vapor clocks are stable to one part in 10^{11} over a period of weeks. The frequency shift of a clock far removed from the Earth compared with one on the ground is given by $V/c^2 = 7 \cdot 10^{-10}$, which is seventy times greater than the relative drift of the clock rates; this fact implies that a greatly improved red-shift measurement could be made with an appropriate satellite as soon as suitable flight equipment can be developed.

Timekeeping Satellite

To look for a gradual weakening of the gravitational interaction (Brans and Dicke, 1961), it would be desirable to have a reasonably short-period, high-density satellite with 12 to 24 precision quartz corner reflectors (say, 5 cm on a side). The satellite might be a 500-kg sintered tungsten ball, and it should fly above the Earth's atmosphere at a gas density of about 20 particles/cm^3. It should be accompanied by another satellite also carrying corner reflectors and moving in almost the same orbit. The companion satellite would be constructed to present the same external appearance but would be, say, only one-third as dense as the principal satellite. Thus, the differences in motion could be used to evaluate the effect of gas drag and light pressure on the principal satellite. The two satellites would be launched together and later pushed apart very gently by a light spring. Alternatively, a single timekeeping satellite could be constructed as a hollow spherical shell of high density, containing a small untethered gold ball at the center, with a capacitance bridge and electrostatic accelerometer to monitor the gas drag and light pressure. Telemetry would be used to transmit the information to the ground.

Eötvös Experiment

The concepts of inertial mass and gravitational mass are very differently defined, but their identity, which is postulated by the general theory of relativity as one aspect of the "principal of equivalence," has been demonstrated by terrestrial Eötvös experiments to within three parts in 10^{10}. The equivalence of gravitational and inertial mass is not subject to test with natural astronomical bodies but could be tested with artificial bodies of known mass.

Imagine two bodies of different materials placed in orbit with identical initial conditions. If the ratio of gravitational mass to inertial mass for body A is identical with the same ratio for body B, difference in their trajectories would be detected; if, however, the ratio differed by $1:10^9$ for the two bodies (still with the same initial conditions), differences of this same order of magnitude would develop between the semimajor axis and periods of the orbits of A and B. On the scale of typical orbits of artificial Earth satellites, there would develop a difference of the order of a centimeter or so between the semimajor axes, and a difference of a few microseconds between the periods.

Although present-day techniques may be inadequate to detect effects as small as these, that situation will not always prevail; furthermore, the idea behind such an experiment is so simple that the possibility of performing it should be continuously re-examined as techniques improve. Even if not detectable absolutely, differences in the orbital constants of the order of magnitude given in the example above might be detected differentially, by measuring the vectorial displacement of one body from the other as a function of the time. In particular, the difference in period would have a cumulative effect, so that one body would move progressively ahead of the other a few centimeters per revolution. One can imagine observations being made on the pair of bodies from nearby orbiting spacecraft, or inside a space laboratory. The most serious difficulties would presumably be those arising from very slight differences in the initial conditions, or from a failure to correct completely for other effects, such as those produced by the gravity-gradient forces caused by presence of the spacecraft and experimenter, electrostatic or other forces, those due to the minute but nonnegligible mutual gravitational attraction of the two test bodies, and those due to electrostatic and other forces of geophysical origin.

Our ability to carry out such an experiment will probably be marginal for some time; compared to it, the following alternative version of the Eötvös experiment was endorsed by the Group as being more nearly feasible.

Eötvös Experiment using an Eötvös Balance in Space. (Roll et al., 1964). This is an experiment in which there is a peculiar advantage in having available the untethered free-fall state. Let a body be constructed such that four spheres of equal mass are placed at the corners of a square, with two of the bodies made of gold or a similar high-Z material and two of light material (say, aluminum), and with diagonally opposite spheres being of different materials. In such a body, placed in a servo-controlled wind shield orbiting about the Earth, any small tendency for the heavy material to have a weight in relation to its inertial mass that is anomalous compared with the light material would cause the balance to rotate so as to move the heavy spheres up (or down) relative to the light spheres.

The advantage of four weights as compared with two is that this arrangement can be made free of a gravitational quadrupole moment. Two weights 20 cm apart would suffer a torque, caused by the gravitational gradient, that would be 3000 times as great as the limit, $1:10^{11}$, that has already been set.

The space experiment should be capable of an improvement by three orders of magnitude, to an accuracy of $1:10^{14}$, as a result of replacing the gravitational acceleration toward the Sun ($0.6\ \mathrm{cm/sec}^2$) by that toward the Earth.

Cavendish Experiment

The currently accepted experimental determination of the gravitational constant is $G = (6.670 \pm 0.015) \times 10^{-8}$ dyne $\mathrm{cm}^2/\mathrm{g}^2$. Numerous speculations have been made regarding the universality and constancy of this number. The implications of possible long-term secular changes in this number, or of variations with local gravitational potential, are of extreme importance in astrophysics and cosmology. As a preliminary step to answering questions of this type, it would seem that a more accurate determination of the constant of gravitation should be made in the near-Earth environment. It is quite conceivable that performing a Cavendish experiment in a laboratory approximating zero-gravity conditions would allow a more precise determination.

As an example of the type of conceptual study that must be carried out to evaluate the possibilities for such an experiment, let us consider an experiment in a zero-gravity laboratory with two solid tungsten spheres of 4-cm radius set into near-circular orbits about their common center of mass under the influence of their mutual gravitation. If the mean separation of this artificial binary system is adjusted to approximately $a = 10$ cm, an appreciable departure from zero eccentricity can be tolerated without having the spheres touch. Since the density of tungsten is 19.3 g/cm^3, the mass of each sphere will be 5.18 kg. The period T will be given by

$$(2\pi/T)^2 a^3 = MG, \qquad (1)$$

where a is the semimajor axis and M is the sum of the masses. Taking $a = 10$ cm, we obtain $T = 7.56 \times 10^3$ sec = 2.10 hours. Such a period does not appear inconvenient for making precise measurements of the relative positions of the spheres. As, for the first time, one would have the possibility of measuring the period of a binary gravitating system for which the masses could be accurately determined by comparing with laboratory mass standards, one could use the measured period and semimajor axis for a precise determination of G. This would reverse the procedure that is customarily applied to deduce the total mass of a binary stellar system from the known value of G.

The method of observing the relative positions of the spheres might depend on their size. For spheres of the size range discussed here, periodic photographs taken with precision optics could be used. The positions on photographic plates of certain fiducial marks provided on the spheres could be measured with an optical comparator if a manned laboratory were available nearby. Simultaneous photography of a star-field background could give a precise measure of the instantaneous orientation of the camera axis. Three cameras with approximately orthogonal axes would probably be used. Optical interferometric measurements of relative position and Mössbauer measurements of relative velocities might be considered if a means to overcome problems associated with rotation of the masses can be suggested. It should be easily possible to measure the direction of the line of separation relative to the fixed stars to an accuracy of 1° after 10,000° of rotation (60 hours), and to measure the separation of the spheres to an accuracy of 0.01 mm. This would give T to one part in 10^4. According to Eq. (1) this would in turn give G to five parts in 10^4, which would represent a substantial improvement in the accuracy with which G is now known. A considerable improvement on the measurement accuracy for angular position and separation should be easily possible using optical systems of long focal length.

GEOPHYSICAL APPLICATIONS BASED ON AN ORBITING MICROWAVE ALTIMETER AND GRADIOMETER

The geocentric radius to the surface of the land and sea is a changing quantity at each point on the Earth. Tidal forces, wind stress, and barometric pressure constantly remold the sea surface. Erosion, tectonic events, glacial accumulations, and internal adjustments remold the surface of the land. These dimensional changes can be progressive, cyclic, or intermittent, but each has an explanation and significance in furthering understanding of physical processes at work within the solid Earth, oceans, and atmosphere.

World-wide surveillance of these effects might be provided by an orbiting microwave altimeter and gradiometer.* Given sufficient experience in the analysis and interpretation of terrestrial events, the technique should also prove valuable in the examination of other planets. With an orbiting, precision height sensor and gradiometer we might hope to measure:

(1) ocean surface waves, including tides and tsunamis;
(2) the patterns and transports of primary ocean currents;
(3) the atmospheric pressure (and winds) at sea level;
(4) redistributions of mass within the lithosphere;
(5) eustatic changes of sea level or land level;
(6) volumes of snow and ice accumulation on land;
(7) density of foliation in forests and grasslands; and
(8) the figure of the Earth and its gravity field.

The dimensions and frequencies of relief events to be expected in each of these physical categories are:

	amplitude (cm)	scale (cm)	frequency(sec^{-1})
(1)	0 to 2×10^2	0 to 10^8	10^2 to 10^{-5}
(2)	0 to 10^2	0 to 10^7	10^{-5} to 3×10^{-7}
(3)	0 to 10^2	10^5 to 10^8	10^{-3} to 10^{-5}
(4)	0 to 10^5	10^7 to 10^9	10^{-5} to 10^{-11}
(5)	0 to 10^4	10^8 to 10^{10}	10^{-7} to 10^{-9}
(6)	0 to 10^5	10^7 to 10^9	10^{-5} to 10^{-7}
(7)	0 to 10^4	10^7 to 10^9	10^{-5} to 10^{-6}
(8)	0 to 10^3	10^7 to 10^9	10^{-4} to 10^{-8}

For dynamical interpretation of items 1 through 5, measurements of relief would be most valuable if made with reference to an equipotential surface at or close to the geoid. In this case, we must redefine the geoid as that equipotential defined by the surface of a motionless ocean, under uniform atmospheric pressure, in which density is either uniform or a function of pressure alone.

To be effective, observations of relief for oceanographic and meteorological purposes would have to be responsive to changes of 1 cm. For example, the hydrostatic response of the sea surface to a change of atmospheric pressure is 1 cm/mb. Correspondingly, the rise of sea level across the width of the Gulf Stream is about 10^2 cm in middle latitudes. Other categories would be easily accommodated if this amplitude sensitivity were to be realized.

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* See note, p. 6-19.

Proposals of this type for a satelliteborne altimeter have sometimes been rejected on the grounds that the satellite orbit is not known with sufficient accuracy. While it is true that the best orbit determinations are currently no better than ±80 m along the track and ±40 m across it, most of this error has a much longer wavelength than the wavelength of the geophysically interesting variations to be measured by the altimeter. For example, the predominant error in height would have an amplitude of about ±40 m and a period equal to the satellite orbital period, with a variation from one revolution to the next of not more than 5m. With a continuous measurement of height and a reasonably careful analysis of the measurements, variations in height with a period shorter than one-third the orbital period (corresponding to wavelengths shorter than 13,000 km) should be distorted by orbital errors by no more than a few centimeters.

Frequency discrimination is most sensitively limited by tsunamis and tides, where the effects of aliasing of time series of surface elevations can be very serious. An orbital period of 90 min would be suitable for tides except that cyclic repetition of measurements at any given place on the Earth would lead to time series at 12- or 24-hour intervals owing to Earth rotation. Sun-stationary orbits would emphasize the lunar tide, while a lunar-stationary orbit would emphasize the solar tide. These simplifications might be useful.

Aliasing through choice of orbit is less of a problem in all other observational categories, for which daily sampling would delineate the progress of change of all processes except step functions such as faulting. Measurements of surface-height changes would be useful to a degree, but would be much more meaningful if they could be referred to some reference geopotential surface.

From the measured tangential velocity of the orbiting altimeter one may find its potential within an arbitrary constant of integration. Having the instantaneous potential and the height of the Earth's surface beneath that place, the potential of the Earth's surface may be found to a linear approximation. Whether a linear approximation is sufficient or not depends on the ratio of the height of the orbit to the local geocentric radius of the Earth and the local deviations of the Earth's gravity field from spherical symmetry. This question needs careful examination in terms of potential theory and of the possible use of a gravity-gradient sensor in addition to the orbiting altimeter. Presumably, measurements of the gravity gradient together with height would lead, after transformation from a fixed inertial to a rotating terrestrial coordinate system, to a good estimate of surface gravity over the oceans. This, with Stokes' theorem, could yield a best fit of the geoid in continental regions that would, in turn, allow proper terrain corrections to be made in reducing gravity on dry land to the geoid. Then, again, Stokes' theorem might be reapplied to a closer approximation of the Earth's gravity field and a better definition of the Earth's figure. This sort of "geodetic bootstrapping" would not only employ detailed ocean surface gravity for the first time but reveal the extent to which isostatic compensation is realized in the crust, and its strength in the face of uncompensated loads.

In the several phases of this orbiting sensor concept, it is to be expected that as much additional information as necessary be incorporated into the interpretation and analysis of height measurements. For example, one would certainly wish to interpret the barometric load on the ocean with reference to synoptic weather maps, Tiros and Nimbus photographs of cloud forms and radiation, and, not least of all, the steric relief of the sea surface due to regional differences of sea-water density and steady-state currents. Similarly, where sea-level barometric pressure systems are clearly established, there should be accompanying geostrophic wind fields and well-developed sea states. It is through this background of related phenomena that the presence and progress of tsunamis might be established, and, again, if tsunamis are present, one would look for sudden changes in terrain owing to faulting or volcanism in the directions from which the tsunamis appear to originate. Given careful study of the orbital perturbations over tectonically active regions, the variations of gravity and relief might possibly serve to anticipate these cataclysmic events.

In the longer term, eustatic changes of sea level have an unknown origin. Sea level is known to rise and fall slowly with respect to the land, but which actually moves? Does the land rise or the sea fall? Simple analysis, assuming that the mass and crustal density of the Earth is conserved, suggests that the mean sea level maintains more nearly constant geopotential than the continental masses. But all this remains an open question.

The virtue of an orbiting altimeter (and gradiometer*) is that it could provide totally new kinds of information about the shape and size of the

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* Note added October 1965: An orbiting gravity gradiometer may be even more useful in application to the Moon than to the Earth, because of the lack of surface gravimetry, the much larger variations anticipated, and the ability to sustain a much lower orbital altitude.

The gradiometer will be quite complementary to the determinations of the gravitational field from orbital perturbations, which are highly smoothed and hence reflect mainly the harmonics of low degree. Since it will be a new device, however, it is highly desirable that the accuracy, drift characteristics, and mode of operation of the gradiometer be such as to make possible a comparison of gradiometer results with those made by orbital perturbation. The mode of operation required would be nearly continuous operation over at least one complete revolution. The accuracy plus drift per revolution would have to be about $\pm 10^{-11}$ gal/cm for such a comparison for an Earth satellite at 1000 km altitude, about $\pm 5 \times 10^{-10}$ gal/cm for a lunar satellite at 100 km altitude. The accuracy currently estimated as attainable for the system proposed by ARMA Division is about $\pm 3 \times 10^{-11}$ gal/cm.

The most active effort toward a satellite gradiometer appears to be that of the NASA/MSS manned orbiters. The argument made in favor of placing the gradio-

Earth, its changes of dimension with time, and the physical events in the air, sea, and solid Earth that are necessarily involved. Moreover, with this new tool at our disposal, and sufficient time to learn how to use it on this planet, a means may well be provided for the physical exploration of other planets as well.

COLLISIONLESS SHOCK-WAVE EXPERIMENT

The possible existence of a collisionless shock wave in interplanetary space was first suggested by T. Gold in 1955 to explain the sudden-commencement phase of a geomagnetic storm. Subsequently, many theoretical studies of the collisionless shock wave have been made, but the physical processes of its formation and many details of its structure are still not understood.

The first successful detection of a collisionless shock wave was made by the first Interplanetary Monitoring Probe, IMP-1, launched on November 27, 1963. The existence of the shock front is evidenced by data showing a transition from a uniform interplanetary magnetic field to the turbulent and rapidly fluctuating magnetic field characterizing the transition region between the shock front and the magnetosphere. Evidence for the presence of a collisionless shock wave propagating in the interplanetary space had also been obtained by C.P. Sonett from Mariner II measurements.

In addition to the magnetic field, measurements of the physical properties of the plasma in the transition region have been made (H.S. Bridge of MIT; I. Strong of Los Alamos; and J.H. Wolf of NASA's Ames Research Center). However, the results are far from complete, and some of them are conflicting. Little is known about the density, temperature, and velocity profiles in the shock front. Whether there are fluctuations in the physical properties of the plasma—in particular in the velocity in the transition region—is not certain at present.

With a properly instrumented manned orbiting laboratory, many interesting experiments can be carried out to measure the turbulent structure in the transition region. As the magnetosphere extends to about 10 Earth radii on the sunlit side of the Earth and as the transition region on this side is 3 or 4 Earth radii thick the orbit of the laboratory should be properly chosen.

Measurements should be carried out for physical quantities such as magnetic-field intensity and velocity and number density of the plasma

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meter in a manned satellite is that it requires frequent calibration and adjustments that are difficult to automate. It should be emphasized, however, that continuous measurement over an entire revolution is desirable, which may place severe restrictions on the astronaut's freedom of motion and other sources of "noise" in the gravity gradient generated within the satellite.

particles in order to obtain both their mean values and fluctuations. The determination of the energy spectrum of the plasma, especially for the electrons, will be highly desirable. A unique advantage of a manned orbiting laboratory is that more accurate determination of these quantities within the response time of the instrument is possible. The reading time and the sampling time for IMP-1 were 4.8 sec and 20.4 sec, respectively, and both are thought to be too high. In addition, the thickness of the shock front obtained by IMP-1 was of the order of 40 km or more. It would be interesting to measure the precise shock-front thickness and to obtain the profiles of the physical quantities throughout the shock front. Sonett has found that the classical Rankine-Hugoniot relations should be modified to have $\gamma = 1.2$ for the collisionless shock wave in interplanetary space. It will be extremely valuable to determine the shock relations for the collisionless shock wave in front of the magnetosphere.

Any experiment on the collisionless shock wave can probably be carried out only in space, since, thus far, all attempts to produce such a shock wave in the laboratory have failed. The collisionless shock wave in front of the magnetosphere is the only readily accessible one in space, and it is logical that every attempt should be made to explore it.

The purpose of this experiment is not only to verify previous results obtained by IMP-1 and by other space probes and satellites. Much-needed and important information about the turbulent nature of the magnetic field and plasma in the transition region behind the collisionless shock wave is to be expected. Useful clues would be obtained for the development of a theoretical model and analysis, and eventually for a better understanding of the phenomena of the collisionless shock wave.

DEEP INFRARED BACKGROUND RADIATION

It has been suggested that a measurement above the Earth's atmosphere of the spectral distribution of general background radiation in the deep infrared (10-100 μ) could provide information of fundamental physical importance. Much of this radiation would have had its origin in the starlight from galaxies when the Universe was young. Radiation originally in the visible and ultraviolet spectral regions from the remotest and hence youngest stars would be strongly shifted to the red by the expansion of the Universe. Because of this extreme red shift, the spectral distribution is strongly affected by the cosmological model, whether open, closed, or flat. It is even more strongly affected by the early revolutionary history of the galaxies. Peebles has estimated the various contributions to this general background radiation. The results are plotted in Figure 1.

The solid curves of Figure 1 give the integrated radiation from the background of remote galaxies for three possible models for the evolution of

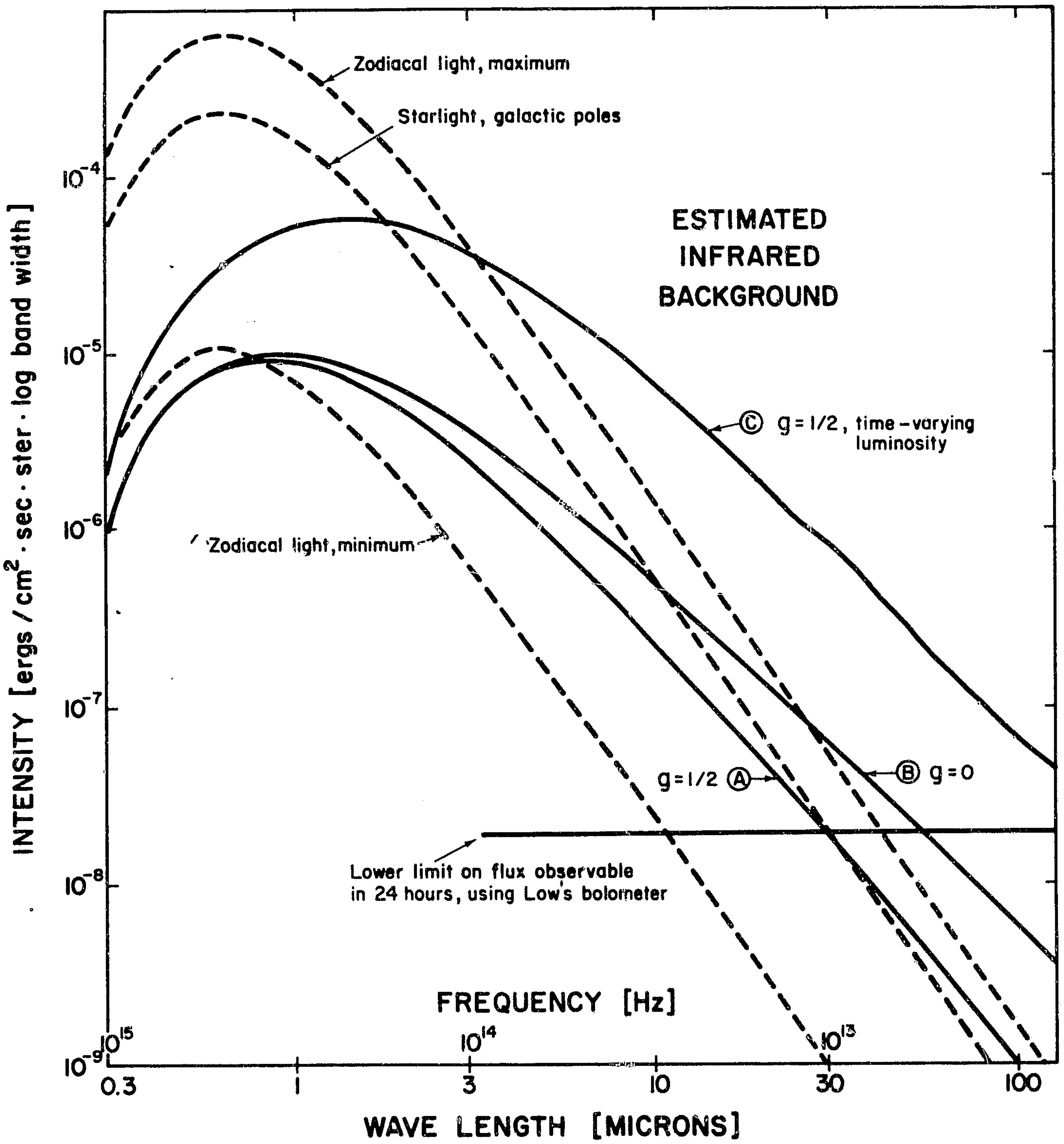

Figure 1. Estimated infrared background

the Universe; the middle dotted curve gives the estimated background from stars in our own Galaxy near the galactic poles (actually foreground when compared to the extragalactic background); and the upper and lower dotted curves give the possible range of the zodiacal light looking normal to the plane of the ecliptic. The vertical scale is the energy flux per unit interval in the logarithm of the bandwidth. For the cosmological models, the reciprocal Hubble constant has been set at $H^{-1} = 1 \times 10^{10}$y. In cosmological models A and B the luminosity of each galaxy is constant, and each galaxy radiates like a blackbody at 6000°K. In model A the acceleration parameter is $g = 1/2$, corresponding to a matter-filled Universe with a mean mass density of 2×10^{-29} g/cm^3. In model B the mass density is substantially less than 10^{-29} g/cm^3. In model C the spectrum of each galaxy remains characteristic of a blackbody at 6000°K, but the luminosity of each galaxy varies with time as the function $L(t) = L(t_f) \exp 4(1-t/t_f)$, where t_f is the present age of the Universe. The factor 4 in the exponent was chosen so that our Galaxy would have converted 30 per cent of its mass to helium with a present mass-luminosity ratio equal to 3 in the solar units. The curves imply that from observations of the spectrum of the infrared background one can hope to gain information on the evolution of galaxies, the effect of the choice of cosmological model being relatively minor.

The curve for the maximum intensity of the zodiacal light background normal to the ecliptic was extrapolated from the visible spectral region, using the solar spectrum. However, from the polarization there is reason to believe that the zodiacal light has been scattered from particles of micron size, so that in the infrared the background may approach that due to scattering from the solar wind only. The curve for the minimum intensity of the zodiacal light is that calculated for a direction toward the ecliptic poles resulting only from scattering by the solar wind with a density assumed to be 15 electron/cm^3 at the orbit of the Earth. The intensity of the starlight is in the direction of the galactic poles. The curve was based on a simple blackbody spectrum at 6000°K. The infrared flux may be larger than that shown if there are appreciable numbers of cooler stars. The horizontal line is the intensity just detectable (unit signal to noise) with the bolometer described by Low (1961), where the bolometer accepts radiation over a bandwidth $\delta\nu/\nu = 1$ and one steradian solid angle, the bolometer temperature is 1°K, and the observing time is 24 hours.

An investigation of infrared background radiation could be carried out either with an automated instrumented spacecraft or with assistance of man in space. The necessity for cooling the infrared radiation detectors with liquid helium would make the participation of a trained space technician useful.

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