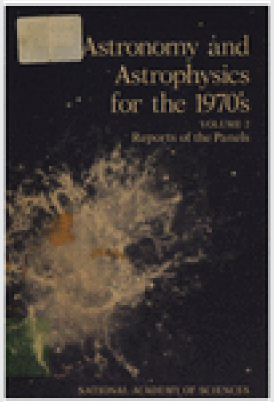

Astronomy and Astrophysics for the 1970s Volume 2: Reports of the Panels

DETAILS

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Astronomy Survey Committee, National Academy of Sciences

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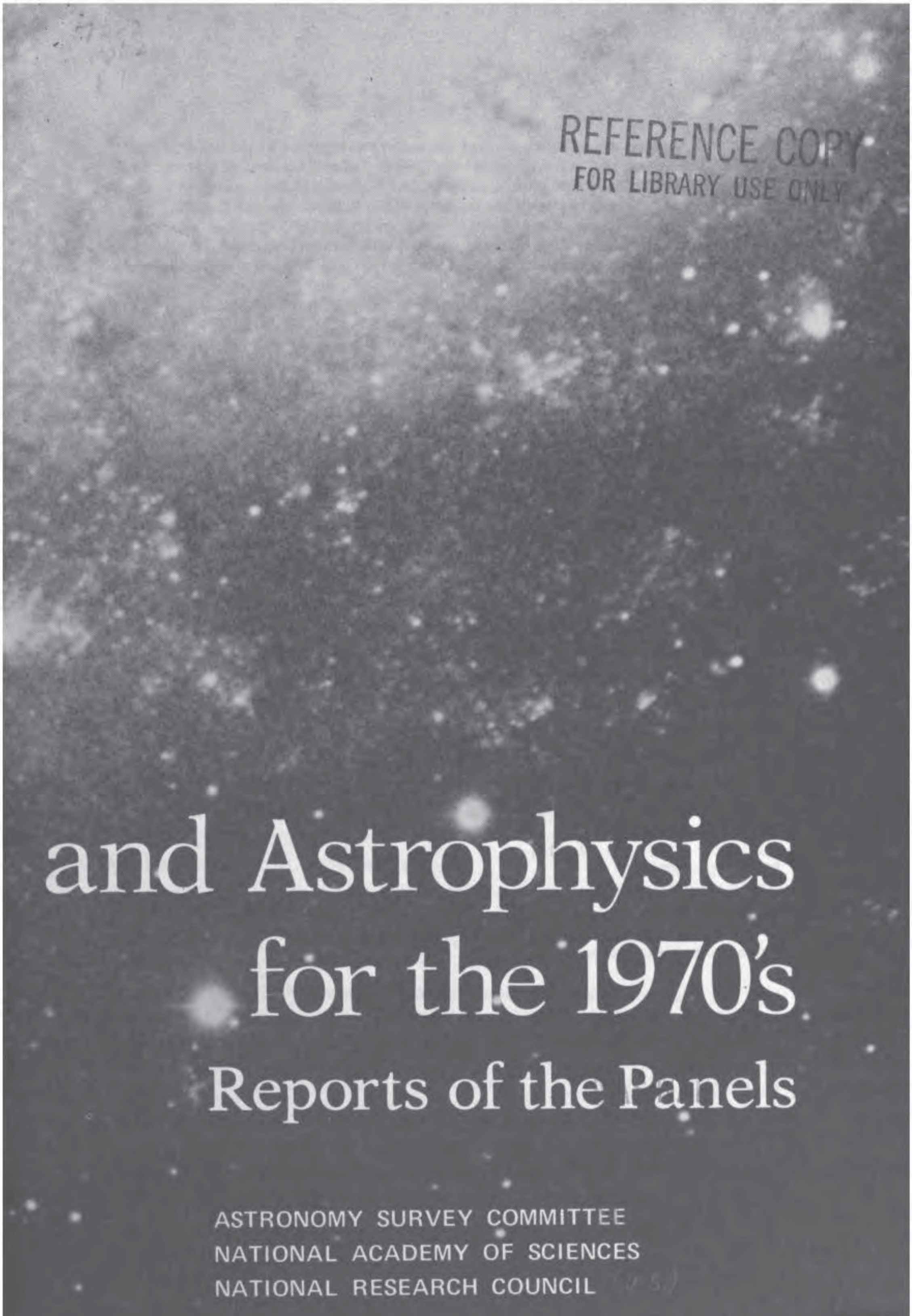

and Astrophysics for the 1970's

Reports of the Panels

ASTRONOMY SURVEY COMMITTEE
NATIONAL ACADEMY OF SCIENCES
NATIONAL RESEARCH COUNCIL

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The members of the committee selected to undertake this project and prepare this report were chosen for recognized scholarly competence and with due consideration for the balance of disciplines appropriate to the project. Responsibility for the detailed aspects of this report rests with that committee.

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Title Page: Spiral galaxy showing stars and groups of stars at the limit of resolution of the 200-in. telescope using direct photography. (*Photo courtesy of Hale Observatories.*)

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Preface

The Astronomy Survey Committee was established in mid-1969, at the request of the Committee on Science and Public Policy of the National Academy of Sciences in response to requests from several federal agencies. Its goal was to outline the present state of astronomy, to identify the most exciting problem areas in that field, and to recommend a program for the United States for the next ten years, including both major new ground-based facilities and major space-science programs. From the beginning, the Survey Committee was also faced with the problem of assessment of priorities within the framework of recent federal funding. In the two years of the Survey, a large number of surprising and important discoveries occurred in astronomy, while growth in support was slowing down and major groups faced retrenchment or loss of funding; for the years from 1968 to 1971 the National Science Foundation funds for basic research grants in astronomy remained unchanged at about $6 million per year, while 400 new PhD's graduated and sought research support.

In 1964, the National Academy of Sciences published a report entitled *Ground-Based Astronomy: A Ten-Year Program,* prepared by a panel headed by A. E. Whitford. The present Survey has a different emphasis. It reviews the present state and future need for facilities, flight programs, and ongoing support of all astronomy, including space science and solar physics; one of its main themes is the rapid progress of the field since the Whitford report. The effectiveness of ground-based facilities has increased extraordinarily as the result of new applications of sophisticated electronics, which have greatly enhanced the effectiveness of existing telescopes and extended their use far into new wavelength regions. The capability provided by the space-astronomy program resulted in observations at essentially all wavelengths unobservable from the ground. These advances led to the discovery of many new objects and phenomena and made it clear that the astronomical universe was in many ways still largely unexplored. New facilities are needed on the ground and in earth orbit to exploit fully the promising opportunities opened by advanced technologies. Furthermore, the new discoveries brought new questions concerning total energy output, high-density matter, and general relativity that touched on central unsolved problems of both physics and cosmology.

The charge to the Astronomy Survey Committee was to consider the broad range of astronomy, excluding lunar and planetary exploration. We do not discuss the moon and meteorites, for example, not because we do not consider them to be important; quite to the contrary, they provide information essential for our understanding of the formation of the sun and the earth. The impressive results of the geological exploration of the moon, the dating of lunar rocks, and the initial heat-flow determinations are major contributions to our picture of the formation of the solar system. The moon and planets, the earth's atmosphere and exosphere, geophysics, and earth resources have not gone unstudied; they have been the topic of extensive reports prepared by the National Academy of Sciences as well as by other groups working directly with concerned government agencies.

Following discussion begun in 1968, a 23-member Survey Committee was appointed in July 1969. Panels were created, each with a Survey Committee member as chairman; later, several working groups and special study committees were created to fill gaps in coverage of subject matter and technique.

This volume contains the reports of those panels.* Each panel report was critically reviewed by the Survey Committee and reworked by the panel after extensive discussions between the Committee and the panel chairmen. The revised final panel reports that appear here are the work of, and reflect the views of, the individual panels.

The discipline panels were concerned with the quality and content of the scientific work in their subdiscipline, existing and hoped-for instrumentation, and scientific expectations for the future. For example, optical telescopes are reasonably familiar, but the auxiliaries for optical telescopes have grown enormously in sophistication and are described

*The Panel on X-Ray and Gamma-Ray Astronomy did not prepare a formal report; its contributions are included in the report of the Space Panel. The Working Group on Planetary Astronomy did not prepare a formal report; its contributions are included in other panel reports and in Volume 1.

in detail with recommendations. The recent expansion of the two National Observatories in providing optical astronomers with two new large telescopes. Radio astronomers, on the other hand, work with an accumulated capital deficit of radio telescopes, which, since the Whitford report, seems almost overwhelming. Nevertheless, the accomplishments of these two major techniques in astronomy have been outstanding. They are described in varying detail in the Optical and Radio Panel reports. Much of the Radio Astronomy Panel report was completed before the exciting discoveries made with the very-long-baseline interferometer became available; nevertheless their recommendations are timely, even in this respect.

The chapter by the Infrared Panel deals with problems of infrared telescope design and the suitability of various sensors and telescopes for use at various altitudes in or above the earth's atmosphere. The Infrared Panel report is one of the first reviews of the technical problems of infrared astronomy to be published; its techniques and problems may be unfamiliar to physicists and engineers who are becoming interested in the field.

The Space Astronomy Panel worked under particularly difficult circumstances; major astronomical launches occurred just before and after most of their work was done. We have attempted to update this report scientifically but perhaps with less success than in other fields. The broad scope of its recommendations remain valid.

Research programs and goals change more slowly in solar astronomy than in some other disciplines. The series of successful Orbiting Solar Observatories has provided us with a clear outline of what may be expected in solar research; in spite of budgetary constraints, the Solar Panel strongly endorses the continuation of this program.

Progress in theoretical astrophysics and dynamical astronomy, made possible in part by the large computer, is an essential theme of those chapters. The application of changing physical theory to model building, as new discoveries are made, forms a continuing challenge to the theoretical astrophysicist and the dynamical astronomer. The problems in theoretical astrophysics are fundamental; costs are relatively small, and theoretical astrophysics can be done in small university groups without major facilities beyond access to a large computer. Although new scientific problems have been posed since these panel reports were written, they require essentially the same scientific techniques.

The Panel on Astrophysics and Relativity was a joint activity with the National Academy of Sciences Physics Survey Committee. A major part of the Panel's report is included in this volume. The membership of the Panel was divided about equally between scientists with astronomical and physical backgrounds. Their recommendations are based on an independent evaluation of the needs for observing facilities on the ground and in space and for theoretical research. It is interesting to look back and see how well their recommendations for new major facilities agree with those of the Optical, Radio, Infrared, and other Panels.

To develop the programs recommended in Volume 1 in the context of total federal and private spending in astronomy and astrophysics, it was essential to have extensive statistical material, some of which was previously unavailable. The Statistical Panel integrated material from published and internal federal agency reports; they surveyed astronomical institutions to assess numbers of astronomers and students, expenditures and sources of funds, and scientific interests. The Panel also surveyed recent PhD's to determine how they were faring in their careers. The Panel's tables and graphs give as complete a description of the profession of astronomy as is now available.

The panel reports formed the basis of the study carried out by the Astronomy Survey Committee, which appears in Volume 1. The Committee is grateful to the nearly 100 participants in this effort. The panel members worked hard, under tight time schedules, to produce technically valid documents that remain modern, in spite of the passage of two years. (Such is the pace of discovery that two years is a long time.) These reports provide excellent guidance as to the major trends to be expected in astronomical and astrophysical research. They also list instrumental facilities, auxiliaries, and programs of theoretical research required for the understanding of the universe in which we live. They make recommendations, of which only a fraction appear in our priority programs in Volume 1. The reports are the responsibilities of the individual panels and their membership; the Steering Committee endorses their contents and validity. Many of their recommendations may prove even more interesting with the passage of time, as astronomy changes.

The Survey was supported by the National Aeronautics and Space Administration and the National Science Foundation. The work was administered by the Division of Physical Sciences of the National Research Council. We are grateful for the government liaison officers to the Survey, to senior members of the staffs of NASA and NSF, who gave us advice in the early stages, and to staff members of congressional committees and the Office of Management and Budget for their assistance. We are also grateful to the entire community of astronomers and astrophysicists, who provided the needed information for the statistical survey, who made special studies, and who gave us extensive advice.

Jesse L. Greenstein, *Chairman*
Astronomy Survey Committee

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CHAPTER ONE

Radio Astronomy

PANEL MEMBERS

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I. INTRODUCTION

A young engineer of the Bell Laboratories, studying the output of data from his radio equipment some 40 years ago, changed the entire course of astronomy. As he puzzled over the accumulated results, Karl Jansky realized that the radio sky was not a dark void but was billions of times brighter than anyone had calculated. His humble gear, an assortment of wires, pipes, old automobile wheels, and simple electronics, showed that nearly all the random radio noise that he received was coming from the Milky Way, the great system of stars to which our sun belongs. Curiously, nearly 20 years were to pass before the remarkable significance of his work was realized by more than a handful. But, the unexpected and deep implications of his and later radio-astronomy results have continued to be the hallmark of the work in this field.

One might say that there is an invisible universe—the radio universe—that appears only when radio telescopes are trained on the sky. This invisible universe is hidden, in part, by the great dust clouds of the galaxy that obscure the vision of the optical astronomer but pose no barrier to radio waves, which penetrate the interstellar spaces as easily as television signals penetrate the smoke and haze of cities. It is hidden, in part, by the vast distances that dim the distant galaxies for the optical astronomer, as he probes the depths of space, until he must count the rare photons one by one as they trickle through the greatest telescopes. The radio signals would similarly be dim, except for a happenstance of nature, which has created radio galaxies more than a million times brighter than our own that shine across the great reaches of space. Finally, there is the universe hidden by time itself—the radio data show the sky to glow dimly everywhere, the faint reflection from the time of the colossal fireball from which all things were born.

But what does this new universe "look" like? For some curious reason, the radio observations often single out the events of tremendous energy. When stars explode, instead of a smoke puff we seem to get a cosmic-ray puff—supernovae are nature's particle accelerators, with magnetic fields, high-energy particles, and tremendous amounts of radio noise. When the nuclei of galaxies erupt, similar events on an enormously larger scale appear, and the resulting radio galaxies are visible even at distances so great that they test the ability of the great 200-in. telescope on Palomar Mountain to see them. Then, there are the mysterious objects: the quasars, which are galaxies, perhaps, or may be the site of entirely new physical phenomena, and pulsars—regularly "ticking" in our galaxy—apparently superdense concentration of matter 10 miles or so across, weighing a billion tons per cubic inch and spinning from one to thirty times per second.

There have been other, quieter, but no less dramatic revelations. For the first time the grand spiral pattern of our galaxy was revealed by radio observation of the 21-cm hydrogen line. In recent years, a new science, the chemistry of space, has been born, as more than 20 kinds of interstellar molecule have been revealed through microwave spectroscopy. Within our own solar system, radio and radar study of each planet has resulted in total surprise. The largest of the planets, Jupiter, possesses a colossal magnetic field and Van Allen belts of energetic particles—a formidable radiation belt making the earth's belt seem trivial—that all space missions must take into account in their planning. Our sister planet, Venus, has been shown to be fiercely hot at the surface, hot enough to melt lead, and to be rotating in the opposite direction of the other planets. Even the study of little Mercury has yielded its surprises, showing every astronomical textbook to be wrong. Mercury does not keep the same face to the sun, as everyone thought, but rotates about its axis exactly 1½ times for each revolution about the sun, unlike the moon which keeps the same face toward the earth.

What of the instruments with which those discoveries were made? Sometimes, they were small and simple indeed, springing from the laboratories of careful and ingenious physicists who still are hard at work laboring patiently to fashion more incisive tests of the radio universe. Some of the discoveries have required the construction of great radio telescopes that tax the abilities of the best engineers yet yield dividends far greater than purely intellectual ones. When instruments were needed to track the Russian Sputniks, instruments of radio astronomers were copied. Visit the great space tracking stations, and you will see that their instruments clearly trace their ancestry to the antennas of the early radio astronomers. The radio astronomers constantly work hand in hand with the most advanced electronic experts of government, industry, and the universities, stressing technology to the utmost and forming a part of the great technological complex that has had such an enormous role in bettering the lot of mankind.

Recently, there have been several striking examples of the close relationship between forefront technology and radio astronomy. Perhaps the most dramatic has been the development of very-long-baseline interferometry, which literally allows us to build radio telescopes as large as the earth. Not so long ago, the fundamental limitation imposed by the wave properties of electromagnetic radiation made the achievement of high angular resolution look most discouraging for the radio astronomer. The optical astronomer, because of the short wavelength of light, can achieve a resolution somewhat better than 1 sec of arc—about equivalent to resolving a pair of automobile headlights at a distance of 200 miles. By contrast, the largest feasible antenna offered the promise of a minute of arc or so resolution, which is equivalent to resolving the same pair of automobile headlights at a distance of only 3 miles. The development of interferometry, in which one uses two or more smaller antennas to build up an effective aperture of much larger size, piece by piece, as it were, has had an important effect on the direction of instrumentation. In its first form, the signals from the individual antennas were combined directly, the received signals being transmitted through cables to the central station. Here the principal demand is that the electrical length of the cables be held constant, to a millimeter or so, and methods have been developed to accomplish this. Ever larger interferometer arrays have been built—the largest is located in The Netherlands. A plan has been developed in the United States, however, to build an array that will equal the resolving power of optical telescopes, by spreading an array of radio telescopes over an area 30 miles on a side. Only two or three dozen elements are needed for this very large array (VLA).

An entirely new development in interferometers, called VLBI (for very-long-baseline interferometry), has been pioneered over the last four years by American radio astronomers. By using atomic clocks at separate stations, and recording the signals on magnetic tape, the need for the connecting wires of the conventional interferometer has been eliminated. The array elements now can be separated by arbitrarily large distances, and angular resolution a thousand times better than that of optical telescopes has been achieved. With the new technique, it has been possible to probe the structure of quasars and to watch them change shape with time. The results are most puzzling, for two of the brightest quasars show changes that apparently require velocities in excess of the speed of light! The interpretation is still a matter of debate; for example, the distances derived for quasars may be wrong, but the inference is clear—the technique has the power to study the details of processes under the strange physical conditions found in quasars.

The future of VLBI, the newest branch of radio astronomy, shows further exciting possibilities, in old classical fields of celestial mechanics, astrometry, time-keeping, and geodesy. Measurements have already been made of distances between radio telescopes with accuracies of a few tens of centimeters, and in the next few years these accuracies will probably improve to the point where precision of a centimeter or so will be achieved—an accuracy high enough to allow direct measurements of continental drift. Comparisons of time between remote stations to an accuracy of better than 10^{-9} sec and comparison of clock rates to an accuracy of a part in 10^{14} are now achievable. The new method will also be applied to improvement of our knowledge of the rotation of the earth and the motions of the stars.

The extension and exploitation of these exciting new ways of studying the universe forms the subject of the Radio Astronomy Panel report.

II. THE UNIVERSE

It is less than 50 years since galaxies were first shown to be aggregates of stars and gas similar to our own Milky Way system but distributed widely throughout space. Following this, in 1929, Slipher, Hubble, and Humason discovered that the universe is expanding, thus confirming a major prediction made from Einstein's general relativity by Friedmann and Lemaitre. The stage had been set for an observational attack on cosmology, and in the period up to the 1950's optical astronomers continued to extend our knowledge of the constituents and the scale of the universe. With the advent of radio astronomy, a new potentially very powerful tool to probe the universe was added.

It was soon found that some very distant galaxies are exceedingly powerful radio sources. They are emitting radio waves through the synchrotron process, which shows that they contain immense numbers of high-energy particles and magnetic fields over vast regions of space.

One of the first sources to be identified with an extragalactic object was Cygnus A, which is the second brightest source in the sky. It is at a distance of about 10^9 light-years from the earth. Because it is such a powerful source at such a great distance, it was immediately clear that it would be possible to detect, at very great distances, radio sources comparable in energy output with Cygnus A. It appeared that they would be much further away than any normal galaxies that could be detected by optical telescopes on the surface of the earth. Thus they could be used for cosmological investigations. In the past 15 years, many surveys of radio sources have been made to lower and lower flux levels, and many thousands of sources have been found. The difficulty associated with the method is that it is not possible to measure the distance of a radio source unless it can be identified with an optical galaxy whose red shift can be measured. However, among the many thousands of radio sources that have been found, only 200 or 300 have been identified with optical objects. There are several reasons for this—one is the difficulty associated with measuring very accurate positions of weak radio sources, and a second is that there is little or no correlation between the intrinsic radio and optical luminosities of sources. Many radio sources have optical counterparts that are so weak that they cannot be detected with optical telescopes.

For this reason, most of the radio-astronomy effort in cosmology has depended on statistical studies of relative numbers of radio sources of various intensity and angular size, based on the reasonable assumption that in general the most distant objects appear fainter and smaller. Unfortunately, the spread in intrinsic luminosity and in linear dimensions of the extragalactic sources is very much greater than the dynamic range of existing instruments, and this has greatly limited progress. Attempts to interpret radio-source counts as a function of apparent luminosity have led to much controversy. The simplest interpretation of the data suggests that there is a large excess of faint, presumably distant radio sources, over what would be expected in a universe uniformly filled with radio sources. This is interpreted by many to mean that there is much greater density of strong radio sources at large distances, or early epochs, than is present today. If this is correct, it is inconsistent with the predictions of the simplest form of the steady-state cosmology, which requires the universe to appear everywhere the same. However, some astronomers challenge the cosmological significance of the source counts, interpreting the data as merely indicating a local deficiency of radio sources rather than a distant excess. The observational result itself is not without criticism. Even accepting an excess density at large distances, a detailed understanding of the situation is not possible without first knowing the radio luminosity function, and this requires knowledge of the distances of a complete sample of sources.

Some attempt has been made to divide the radio sources that are counted into different categories according to optical identification, surface brightness, or radio spectrum in order to determine which, if any, particular class of source is responsible for the observed excess of faint objects. So far the results of this approach have been inconclusive, primarily because these data are only available for a small number of sources and also because of the difficulty of taking into account the effect of observational selection introduced when one tries to classify or identify radio sources.

There is today a growing realization that, before any definitive cosmological conclusions can be drawn from radio studies, it will be necessary to understand better the nature of the radio sources themselves. Moreover, aside from their use as a tool for cosmology, the extragalactic radio sources pose a major problem for theoretical astrophysics. It is now generally accepted that the radiation mechanism is synchrotron emission from relativistic electrons moving in weak magnetic fields. The outstanding question has always been the following: What is the source of energy amounting to 10^{61} ergs or more in a strong galaxy? The release of gravitational energy of single, very massive bodies or of many smaller objects has appeared to be one of the most promising of many mechanisms proposed and has stimulated extensive theoretical research on massive bodies and gravitational collapse. Other suggested energy sources include matter–antimatter annihilations, quarks combining to form baryons and mesons, and force fields of a type previously unknown. If any of these is correct, the implications for fundamental physics are profound.

Equally challenging is understanding the way in which the energy is transformed into relativistic particles and the mechanism of generation of the magnetic fields that, in some cases, appear to be well-aligned over distances of hundreds of thousands of light-years. It is possible that these processes take place through mechanisms that are familiar in laboratory plasma physics or in man-made accelerators. It is equally possible that processes of hitherfore unknown types are involved. The evidence now indicates that the energy is released in repeated violent events, with as much as 10^{52} ergs each, in the nuclei of some galaxies, released in time scales of one year or less and in regions less than one light-year across. These outbursts produce short-lived compact radio sources that have angular dimensions of about $10^{-3\pm 1}$ sec of arc. Although these compact sources are initially strongest at short centimeter and millimeter wavelengths, the radio emission rapidly decays toward longer wavelengths in times of a few months to a few years. More detailed studies of changes in intensity, polarization size, and structure with wavelength and time will give further data on the nature of the compact sources. This will allow us to determine the rate of expansion and the deceleration of a young radio cloud; the magnetic field strength as a function of time; the rate at which the relativistic electrons are produced or accelerated; and the rate at which they lose energy either by expansion of the cloud, by radiation losses, or by Compton collisions. It will then be possible to specify the physical conditions at the very early epochs of such explosions, and this in turn may lead to a better understanding of processes involved in the release of energy.

There are apparently two types of optical object associated with extragalactic radio sources: the radio galaxies like Cygnus A and the quasi-stellar objects—the quasars, which have the optical appearance of stars but very large red shifts in their spectra.

The discovery of quasars was one of the major scientific events of the last several decades. It is now known that radio emission is only one of the characteristics of such sources. They are very powerful sources of nonthermal optical and infrared radiation, generated in exceedingly small volumes—no larger than the size of the solar system in some cases. This discovery led to the conclusion that very large masses are contained in very small volumes in these objects and also in the nuclei of many galaxies, which have similar radiation characteristics. The energy release is due in some way to conversion of gravitational energy to high-energy particles. It is not known for certain where the quasars are—whether their red shifts are due to the expansion of the universe—or what they are—explosions in the nuclei of galaxies or an altogether new type of massive object.

There are clearly great similarities between strong radio galaxies, quasi-stellar objects (QSO's), and the nuclei of comparatively nearby galaxies that are undergoing explosions.

While astronomers in many fields are now studying different aspects of these objects, the basic discoveries have come largely through observations made at radio wavelengths. It may well turn out that radio astronomy, through these discoveries, has given us the key to understanding the universe on the large scale. Some believe that already it has been demonstrated that we live in an evolving universe and that the radio evidence suggests that at an epoch many billions of years ago the rates of galactic formation and evolution were very different than they are at present. Others argue that perhaps the QSO's and related phenomena are direct manifestations of the creation of matter in regions of very high density and that the connection between the laws of the universe and the laws of microscopic physics will be revealed by studies of these events.

III. STARS, SUPERNOVA REMNANTS, AND PULSARS

For most of their lives stars are evolving slowly and unspectacularly, and radio studies are not of great importance at these stages in a star's evolution. However, at the explosive stages of evolution the situation is quite different. Very recently, novae have been discovered to be radio sources. Even more important, the radio investigations have shown the great importance of the supernova phenomenon. Supernovae, which were discovered in the early 1930's by Baade and Zwicky, were predicted by them to be major sources of cosmic rays and also to be the star-shattering explosions in which remnant neutron stars might be produced.

Both of these predictions were shown to be correct through radio astronomical investigations. One of the first radio sources to be identified with an optical object was the Crab nebula, already known to be a remnant of the supernova of A.D. 1054. The discovery that the Crab was radiating both radio and light waves by the synchrotron process showed directly that it is a gigantic cosmic particle accelerator which is able to make cosmic rays at a rate very much greater than any other object known in our galaxy. It was also clear that not all the high-energy particles present could have been generated during the original explosion but that there must be some continuing source of activity among the supernova remnants. The investigations of the Crab have shown the great value of investigations involving optical, radio, and x-ray and gamma-ray astronomy, since it is a powerful source of all of these.

The most powerful radio source in our galaxy is Cassiopeia A, which is also a remnant of a supernova explosion. There is little to be seen optically at the position of this source, except for rapidly moving and changing gas clouds, which is characteristic of supernova remnants, since most of their energy is in the form of high-energy particles and magnetic fields, which can be studied only at radio wavelengths.

The most exciting recent discovery in radio astronomy was the pulsars, which are probably rapidly rotating neutron stars left behind after the collapse of a star gives rise to a supernova explosion. It is apparently the pulsars that are continuing to generate high-energy particles and radiation long after the explosion occurred. They may indeed be a major source of the cosmic rays that pervade the galaxy.

Supernova remnants and pulsars are important for our understanding of the late stages of stellar evolution, cosmic-ray origins, and energy balance in the interstellar medium.

However, apart from this, the discovery of pulsars in 1968 revealed the existence of a new class of extremely exotic objects in which a variety of hitherto unobserved physical phenomena are taking place. Indeed, the study of pulsars offers the possibility of exploring realms of physics, matter, and energy generation that were far beyond our wildest speculations in most cases and that may lead, in the long run, to major practical applications. The pulsars are believed to be rapidly rotating neutron stars in which an amount of material about equal to the total material content of the sun is compressed into a sphere only some 10 miles in diameter. The result of this compression is a new form of matter—bulk nuclear matter or neutron star matter, whose density is about 10^{15} times that of water. If the earth were compressed to such a density, it would be only 100 yards in diameter.

Accompanying the neutron star, which can spin as rapidly as 30 times a second, is an intense magnetic field that, by means not yet clearly understood, converts with very high efficiency the energy of rotation of the star into relativistic particle energies. These particles produce the characteristic brief radio, optical, and x-ray pulses of the pulsar. Some fraction of the particles is eventually injected into the galaxy as cosmic rays. The energy released in this process over the life of a pulsar is as great as the original nuclear-energy resources of the star but is derived from the original gravitational energy of the star. In the radiation-emitting regions of the pulsar, particles interact coherently to create giant pulses of electromagnetic radiation. These superparticles, composed of many particles, give rise to energy fluxes so great that each square inch of the emitting region may emit as much as 10^{12} W, an amount of power equal to the total electrical generating capacity of the earth's electrical plants.

Pulsars have been observed to change their rate of rotation slowly because of the continuous loss of energy and to vary slightly in their pulsing rhythm. The reasons for these phenomena are not yet understood. On several occasions, pulsars have abruptly changed their period of rotation, perhaps due to a seismic event—a starquake in the outer layers of the neutron-star matter with a strength equivalent to 10^9 earthquakes of the magnitude felt in Los Angeles in 1971. In these events, an amount of energy equal to the radiation of the sun over an entire year is released.

The processes taking place in the radiation-emitting region of pulsars are the most extreme we know of in the galaxy and far surpass in energy production any power source yet invented. The understanding of the generating mechanism through further detailed study of the pulsars cannot but help to improve greatly our understanding of physics and could lead to major developments in energy generation here on earth.

The study of pulsars requires painstaking and lengthy observations with very large telescopes that can observe at all wavelengths up to approximately 10 m. The pulsar phenomena can only be studied in detail when extensive opportunities exist for the use of such telescopes.

IV. INTERSTELLAR MATTER IN THE GALAXY

Before the advent of radio astronomy, we had only a fragmentary picture of the distribution and motion of gas and dust in the Milky Way galaxy. Photographs had revealed dozens of H II regions like the Orion nebula, where hot stars ionize the interstellar gas and heat it enough to emit light. It was suspected that the vast areas between the H II regions were not devoid of hydrogen, as they appeared at first sight, but rather contained neutral hydrogen atoms too cold to see.

Optical astronomers were aware of another interstellar component, submicron-sized dust particles that blot out the light from distant stars. The extinction by these particles makes it difficult to study any stars in the disk of the Milky Way that are much more than 3000 light-years from the earth. Study of the variation of interstellar extinction with wavelength indicates that the typical size is near 10^{-5} cm and that the particles are composed of compounds of relatively abundant elements like carbon, oxygen, magnesium, silicon, and iron, with a total mass of approximately 1 percent of that of the gas.

Finally, physicists observed another component of interstellar space—the cosmic rays. Using balloon-borne particle counters, they showed that the earth is being bombarded by an apparently constant flux of fast protons with an admixture of heavier ions with energies in the range $1-10^9$ GeV. Several arguments suggested that these particles are spread quite generally through interstellar space, but their origin was a mystery.

In the 1930's, the first major discovery of radio astronomy, the detection of strong signals at meter wavelengths from the Milky Way, was made by Jansky and Reber in the United States. As more sophisticated radio telescopes came into operation after World War II, primarily in Great Britain and Australia but also in the United States and the Soviet Union, these early studies of the Milky Way were extended, and detailed analyses of the intensity, distribution, and spectrum of the radiation were made. Careful study of the spectrum and polarization of the radiation showed it to be synchrotron emission by relativistic electrons with energies of a few GeV.

From the intensity of the radiation, the number of fast electrons is estimated to be approximately 1 percent of the number of cosmic-ray protons of comparable energy. The radiation mechanism was confirmed when careful balloon experiments succeeded in detecting a flux of electrons of nearly the required intensity, hitting the earth along with the protons discovered much earlier. Radio astronomy, by finding a widespread population of fast electrons, makes it almost certain that the fast protons are widespread also, as suspected earlier. By studying the intensity of the synchrotron emission in various directions, radio astronomers can plot the distribution of the cosmic rays in space, even sensing radiation in the galactic plane that must come from many thousands of light-years distance, where the corresponding light from stars is severely attenuated by interstellar dust. The cosmic rays and magnetic field turn out to be largely confined to a disk approximately 1000 light-years thick and 100,000 light-years in diameter; within this disk, they are particularly prominent within spiral arms.

When radio interferometers with their increased angular resolution went into operation in the 1950's, it was possible to isolate discrete sources of synchrotron radiation. Those near the galactic plane usually turned out to be supernova remnants. The high concentration of relativistic particles in supernova remnants makes it probable that some, and perhaps the majority, of cosmic rays originate in them.

In the process of observing discrete synchrotron sources, radio astronomers also found that the well-known H II regions emit radio waves by another process—free–free or thermal bremsstrahlung, which occurs when the electrons in a hot ionized gas are accelerated by passing ions. Not only were most of the nearby optical H II regions found to emit thermally, but a host of new H II regions were found so distant that their light is obscured by intervening dust.

One of the most interesting areas of radio astronomy in its application to the study of interstellar matter involves narrow radio spectral lines at discrete wavelengths. In 1951, Ewen and Purcell in the United States were the first to find direct proof that H I was generally distributed throughout the galaxy by detecting the 21-cm hyperfine line from interstellar H I atoms.

Because the radiation easily penetrates enormous distances filled with gas and dust, the 21-cm line permits atoms to be observed across the entire galaxy. Because any deviation from the laboratory wavelength is due to the Doppler effect, one can infer the motion of the atoms concerned, and, if one assumes a model for the rotation of the galaxy, their distance can be estimated. The 21-cm line has therefore developed into a major tool for plotting the distribution and motion of interstellar H I in the galaxy. With the advent of large telescopes, 21-cm emission has been detected from many distant galaxies, permitting the correlation of the amount of gas with morphological type of the galaxy.

The gross distribution of the H I turned out to be similar to that of the young hot stars seen optically; the gas (some 4 billion solar masses in all) lies in a thin disk centered on the galactic plane, and within this disk it is concentrated in long spiral-arm features extending around the galaxy. The position of these arms agrees well with that of the young stars, and this is consistent with Baade's idea that the stars, being young, must have formed recently from the gas.

A remarkable outflow of gas has been found in the nucleus of the galaxy. It is not known whether it is a continuous phenomenon; if it is, the amount of interstellar gas in the galactic disk would be increased significantly in a time shorter than the age of the galaxy. On the other hand, the flow could be the result of a short-lived explosion. The latter possibility is suggested by the presence of a compact, intense source of synchrotron radio emission at the center of the nucleus.

With large radio telescopes and interferometers of high angular resolution, it becomes possible to study fainter and finer-scale features in the distribution of neutral atomic hydrogen. The distribution is very irregular on the small scale (light-years), with dense clouds imbedded in a background of lower density. The intensities of the 21-cm line can be used to estimate the gas temperatures. Apparently, it is low in the clouds and high between them. In some regions one sees cloud complexes—vast clouds of gas up to 10,000 or 100,000 solar masses. In these complexes are particularly dense regions that are visible to optical astronomers as dust clouds. Current theory suggests that these are likely places for star formation to take place. In some of these dense clouds there appears to be a surprising deficiency of neutral atomic hydrogen; theory suggested that the hydrogen atoms have combined to form H_2, and this has recently been confirmed by an observation of H_2 made in the ultraviolet from a rocket.

At the outer reaches of the galaxy, matter has been discovered approaching at speeds near 100 km/sec. This entirely unexpected phenomenon may represent accretion of gas from intergalactic space. If so, the total mass of interstellar gas may be increasing, with the result that star formation may be prolonged indefinitely into the future or the high velocity gas may be blown out of the center of our galaxy.

Optical astronomers have found some molecules (CH, CH^+, CN) in interstellar space, but radio astronomers have discovered an unexpected variety of other molecules there. After many tries, the hydroxyl radical, OH, which emits a spectral line at 18 cm, was found by diligent searching. Later, OH was found to be emitting at an anomalously high rate in certain small regions—an interstellar maser had been discovered. In this process, the upper level of the 18-cm transition is constantly repopulated by some pump mechanism, in this case possibly selective collisions with H I atoms. The excited OH then amplifies radiation coming in a certain direction by stimulated emission, and an intense highly localized beam of radiation is generated. The theory of the process is complex, and many features of the data remain to be explained. It is clear, however, that quite unusual conditions must prevail in these regions of stimulated emission.

More recently, three other simple diatomic molecules have been seen by their radio emission: CN, CO, and CS. CN had been seen optically, but the intensity of the radio line will tell us much about the cosmic background radiation at 2.6-mm wavelength, the wavelength of the line. The abundance of CO, where it is seen, is surprisingly large, considering that both elements in the molecule are of low cosmic abundance relative to hydrogen.

Even more surprising, several more complex molecules, including H_2O, NH_3, H_2CO, CH_3OH, HC_3N, and HCOOH (water, ammonia, formaldehyde, methyl alcohol, cyanoacetylene, and formic acid), have been detected. At present, it is not understood how such complex molecules could form under the low-density conditions in interstellar clouds. The water appears to be masing also; in one small region the energy of the water emission is almost equal to that of the sun.

The suspicion is growing that the molecules already discovered are only a few of a large group of molecules of ever-increasing complexity that may exist in space. Particularly fascinating is the connection of these studies with those in which chemists are exploring the environment of the primordial earth, which provided the basis for life. Some of the molecules already discovered are of the type expected in a primitive reducing planetary atmosphere that chemists have shown is capable of producing large organic molecules when exposed to high-energy radiation (ultraviolet light or fast particles). The belief is growing that interstellar clouds, which are known to contain solid dust particles and hydrogen gas with an admixture of other elements, and fast particles may form large organic molecules. Such molecules, which may be as complex as the amino acids and may be of equal importance in our understanding of life, may be discovered by observations made at radio frequencies.

The precise significance of such discoveries for the study of chemical evolution is still unclear. However, a key element in our present ideas about the origin of life is the notion of universality—that the processes that led to life on earth in fact operate throughout the galaxy, if not the entire universe. Finding complex organic molecules in interstellar space would support this idea and would add further evidence to the view that it will be possible to find life beyond the earth.

Another area of interest is the study of radio spectral lines from H II regions. After a hydrogen atom is ionized by the ultraviolet light from a nearby star, it recaptures its electron, often in a highly excited state with a quantum number of 100 or more. As the electron cascades back toward the ground state, it emits radio-frequency lines corresponding to jumps between neighboring high quantum levels. These radio lines can be observed in H II regions and provide a new tool for studying velocity, temperature, and the chemical composition of the gas in the ionized clouds. Abundance determinations are possible because ions of all elements capture electrons in the same way, and the abundance ratios are simply the ratios of line intensities unless certain atoms also are masing. By this method H II regions can be studied across the entire galaxy even if they are completely obscured optically by dust. Measuring velocities, as is done for the neutral hydrogen regions, one can make a map of the distribution of H II regions around the galaxy. It is found that they also lie in spiral arms, as predicted from optical studies of other spiral galaxies.

A recent development in interstellar physics is based on careful observation of the radio pulses from distant pulsars. It is found that the low-frequency component of the pulses arrives systematically later than the high-frequency component. This effect is due to ionized gas (or plasma) along the line of sight, which disperses the radiation. It is interesting that all pulsars show the effect, even though there appears to be predominantly neutral gas and not largely ionized gas along the line of sight. It is clear that more ions are present than we deduced from the earlier studies, and it appears that another source of ionizing radiation must be present in cool regions. Both low-energy cosmic rays and x rays have been suggested and are under discussion. If one postulates that the required ionizing flux is present, one finds an energy input into the interstellar medium that can explain its rather high observed temperature.

The electrons that disperse the pulsar signals, together with the interstellar magnetic field, also constitute a magnetoionic medium that can rotate the plane of polarization of the distant synchrotron source (Faraday effect). If one takes the electron density from the plasma dispersion measurements on pulsars, this permits another estimate of magnetic-field strength to be made. The result agrees roughly with that estimated from the synchrotron theory and that observed from the Zeeman effect and from the optical polarization by dust grains.

V. THE SUN

The sun can be observed at radio frequencies from 100 kHz to as high as 100 GHz. Over this range of 10^6 in frequency, solar phenomena range out from the photosphere into the corona, many tens of solar radii from the sun. With some important exceptions, it is found that at the higher end of this frequency range the emissions exhibit thermal properties of the photosphere, chromosphere, and lower corona, while at the lower end, solar radio emissions demonstrate the essentially plasmalike character of the corona.

In optical wavelengths, the corona is transparent to the photospheric radiation field. Since the corona is extremely hot, and since elsewhere in the sun energy flows in the form of nearly pure blackbody radiation, the corona is often said to be heated mechanically with a possible coupling through the magnetic field. This has been further restricted to imply heating through the agency of oscillations in the photosphere or chromosphere that propagate upward and degenerate into shocks that deposit their energy in the corona.

If this theory is correct, wave motions must occur in the corona. Details of these wave motions will be nearly invisible optically. Since the corona has a characteristic plasma frequency of the order of 100 MHz, it displays extraordinarily complex radio phenomena at metric wavelengths that must in some way be related to the underlying wave motions.

One of the major discoveries of solar astronomy in the past few years has been that of the intricacy of coronal burst structure in narrow frequency bands and short time scales. Two-dimensional radio imaging of the standard burst phenomena is leading to new insights into the nature of particle streams and shock waves in the corona. Fast drift bursts now are shown to occur sympathetically at locations of the sun remote from the originating flare site. Slow drift bursts can cover substantially the entire hemisphere around the flare locus, and the geometry of these shocks revealed by their radio emissions is beginning to be mapped. The postflare synchrotron continua appear to be intimately connected with the prior shock waves, propagating to enormous distances above the sun.

Observations of the kind described are helping us to understand the heating of the corona, the generation of fast particles, and the emission mechanisms. These studies are important for radio astronomy in general, and they may help us to understand the physics of flare stars, radio galaxies, and quasi-stellar objects.

Solar radiation at microwave frequencies is generated mainly through thermal processes in the hot coronal magnetic plasma. The entire photosphere, chromosphere, and inner corona can be detected because they are hot gases with significant continuous opacity starting as low as 20 to 30 MHz but becoming very important above 1000 MHz. It is clear that even with the highest resolutions used for solar radio astronomy—30 sec of arc—structures may still remain unresolved. Most of these highest resolution solar radio telescopes are transit instruments that provide images of the sun integrated over relatively long time scales; very little solar imaging has been accomplished on solar bursts in the microwave region, where emission mechanisms are well understood.

About 10 years ago, it was shown that flare-associated radio bursts of synchrotron origin and hard x-ray bursts are identical to one another in shape and in timing. The locations of hard x-ray sources produced during flares have apparently been observed on only one occasion, on June 8, 1968. This event is startling in the complex pattern of the emission, which resembles in many details the much more familiar pattern of hydrogen emission viewed in the easily accessible visual range. An x-ray telescope in space is needed to observe the x-ray flare phenomena.

A synoptic program is required in order to obtain data for the study of a stochastic phenomenon such as solar flares. This need will be satisfied, to a certain extent, by x-ray space experiments that are already planned. To complement this, a comparable program of study of microwave activity on the sun is required. This will support the x-ray evidence on the sources and dynamics of energetic electrons in the solar atmosphere.

To obtain such information, it will be necessary to observe the sun at microwave frequencies in real time. To do this at high resolution, <5 sec of arc, would eventually require the development of new facilities especially designed for this problem.

VI. THE PLANETS

The penetrating power of the longer radio wavelengths has literally opened a new window on the solar system. When radio telescopes were first trained on Venus in 1956, astronomers were surprised to find apparent disk temperatures of over 550 K—much higher than at the surface of the earth. Even allowing for the proximity of the sun, these were inexplicable. Similarly, Jupiter was found to be a potent emitter at decimetric wavelengths, with a highly nonthermal spectrum. Only later did polarized interferometry reveal the true nature of this radiation for Jupiter: synchrotron emission from Jupiter radiation belts, providing not only the first evidence of a distant planetary magnetic field but also of extraordinary high-energy processes in the outer parts of the solar system.

The last decade has seen an accelerating shift to even shorter wavelengths, so that much work is now being done at millimeter wavelengths. Interferometer techniques working at centimetric and decimetric wavelengths have been used to resolve small planetary disks. In this way the subsurface temperature variations of Venus from pole to equator have been studied, despite the thick overburden of absorbing atmosphere. Polarized interferometry has been used to study the subtle changes in emission from planetary surfaces as the plane of the observed linear polarization is varied. From these observations, the dielectric properties of the surface layer may be deduced.

Much important information about the planets has come from the development of sensitive radar systems. These allow the experimenter to use a highly coherent and known source of illumination to make sophisticated observations. In addition to the determinations of precise distances and surface radii, radar has made substantial and often unique contributions to our knowledge of planetary topography, rotation, surface structure, and surface and atmospheric composition.

By finding unexpected resonances in the rotations of Mercury and Venus, the radar observations have supported inferences concerning the magnitude of gravitational harmonics and, in the case of Venus, the possible existence of a liquid core. Radar systems will soon be able to map the electrical surface scattering properties of the terrestrial planets with resolutions far exceeding the ground-based optical capability.

Only three facilities in the world are now capable of making radar observations of the terrestrial planets with sufficient precision to be significant in the improvement of planetary orbits, in the determination of planetary topography and surface characteristics, and in the testing of theories of gravitation. These facilities are Arecibo Observatory (70 cm), Puerto Rico, operated by Cornell University; Goldstone Deep Space Facility (12.5 cm), California, operated by California Institute of Technology's Jet Propulsion Laboratory; and Haystack Observatory (3.8 cm), Massachusetts, operated by Massachusetts Institute of Technology. The large wavelength spectrum covered by these facilities makes their results in part complementary, especially if near simultaneous observation can be arranged.

VII. FACILITY RECOMMENDATIONS

The basic instrumental needs of radio astronomy—greater collecting area and resolution—have been known for a number of years and are well documented in earlier studies, referred to below. In the late 1950's and early 1960's, astronomers began to develop the designs for a next generation of instruments that would fill these needs. Out of a variety of ideas and designs there have emerged proposals for four major new instruments—the resurfaced Arecibo telescope, the 440-ft fully steerable radome-enclosed telescope, the Owens Valley Array (OVA), and the

Very Large Array (VLA). All of these instruments are consistent with the 10-year program recommended in 1964 by the Whitford panel. The design of each is now well advanced. Both the need for these instruments and the individual proposals have been exhaustively reviewed, separately or collectively, by a number of individuals, panels, and committes, including reviewers for the NSF Astronomy Program, the Panel on Astronomical Facilities (Whitford panel, 1964), the Panel on Planetary Astronomy of the Space Science Board (1968), the radio and radar astronomers meeting convened by the Smithsonian Institution in November 1968, the Astronomy Missions Board of NASA (1969), and the NSF *ad hoc* Advisory Panel for Large Radio Astronomy Facilities (the Dicke Committee in two meetings in 1967 and 1969). These reviews found the proposed instruments scientifically justified and technically feasible. Most recently, the Dicke Committee in June 1969 conducted an intensive review of all the proposed instruments, including the scientific needs, technical feasibility, and present status of design and engineering. Their report strongly recommended construction of all the proposed instruments. However, only one of the proposed programs—the resurfacing of Arecibo—has yet been authorized for construction, and in fact there have been no major new instruments for U.S. radio astronomy in recent years.

The Radio Astronomy Panel also reviewed and endorsed the above proposals; the present report does not discuss specific proposals but instead recommends three major types of instrument:

1. We *recommend* construction of a large-aperture synthesis array. This instrument is needed primarily for studies of galactic and extragalactic sources but will also be useful for planetary and solar investigations and for spectroscopy. The array should have the highest resolution, sensitivity, and speed consistent with present state-of-the-art and economic limitations.

One of the most active areas of radio astronomy is the study of nonthermal sources, including quasars and radio galaxies. Observations of these sources deal with large fluxes of relativistic particles and point to high-energy phenomena in the universe. The sources have roughly equal intensities at all wavelengths, emit relatively small amounts of energy in each spectral band, and are often located at vast distances outside the galaxy, where the numbers of such sources are very large. As a result, it is difficult to distinguish neighboring sources in the sky, so that all present work is limited by confusion of the source under study with the large number of faint nearby sources. This fundamental limitation of the sensitivity of all existing systems is at present severely impeding further progress in understanding nonthermal sources. This limitation can only be reduced by increased resolution. Thus, the prime need in this area is for a very small antenna beam that can isolate sources for study even at the limits of the universe and that can study the details of nearby sources. These requirements can best be met with a large aperture synthesis array.

2. We *recommend* construction of a large fully steerable parabola for centimeter-wavelength observations. This instrument is needed primarily for spectroscopic and radar studies but, again, is useful in other areas as well. The telescope should be as large as present state-of-the-art and economic limitations allow.

Fully steerable parabolas have been the workhorses of U.S. radio astronomy for the past 15–20 years. For spectroscopic and radar studies, large collecting area and the ability to change wavelengths readily over a very wide bandwidth are generally more important than high resolution, and for these studies the steerable parabola is an ideal instrument. The United States has long been the leader in these fields, which today are more active and fruitful than ever before, but U.S. instrumentation is now falling behind what is needed to continue the present pace of development and also falling behind what is available in other countries. A new, fully steerable parabola is needed, operating at centimeter wavelengths and larger than any instrument now available.

3. We *recommend* construction of a large telescope for millimeter-wavelength observations. This instrument is needed for both spectral studies and studies of nonthermal continuum sources in a region of the spectrum that is only now beginning to be exploited but that has already produced important new results.

The recent detection of both diatomic and, even more interestingly, complex molecules in interstellar space has opened an entire new field. Maser effects seem to play an important role in the emission by some of these molecules; it is not understood how complex molecules form under the low-density conditions in interstellar clouds. Finally, it now appears that molecules long considered to be among the building blocks of life are available all through the universe. Thus, the study of molecules, whose radiation in the form of spectral lines at short wavelengths (centimeter and millimeter waves), has become, and promises to remain for a long time, one of the most fundamental areas of endeavor in astronomy.

Similarly, and almost equally important, is the realization that variations in the intensity of quasars and their spectra at centimeter and millimeter wavelengths may eventually give the clues for the understanding of the almost unbelievably high efficiency and output of the emission mechanisms of these objects.

In view of the importance of these forefront areas of research, overlapping astronomy, physics, chemistry, and even the life sciences, the Panel puts a high priority on the construction of as large as possible a millimeter-wave instrument to study them. The large collecting area is required because most of these studies are severely hampered by the very small amount of radiation reaching the earth.

Each of the above instruments should be a national facility, available to all qualified scientists. Each would be unique and of sufficient power to provide major new capability. From the earliest days of radio astronomy, it has been recognized that single large dishes and extended arrays of smaller antennas have complementary functions, neither of which can be dispensed with. Our recommendation of both a powerful array and unique new single dishes recognizes this fact. Construction of these key facilities would greatly strengthen the main lines of effort in U.S. radio astronomy.

VIII. UNIVERSITIES AND NATIONAL FACILITIES

Radio astronomy, like almost all branches of science, had its origin in the universities. In the 1950's, federal agencies, especially the Department of Defense, were investing considerable sums of money in several university-based installations. At the same time, the need was felt for more generally available equipment, and the pressure for new major instruments started growing. This resulted in the foundation of the National Radio Astronomy Observatory (NRAO). The large instruments were considered to involve capital investments and backup groups too large to be handled by a single university. Moreover, to be truly available to all astronomers on an equal basis, a national rather than university-based organization seemed appropriate. This situation is still true. Although in radio astronomy the instruments available in some universities equal in capability any single instrument at the NRAO, neither the combined facilities at the NRAO nor the planned very large telescopes would be appropriate for use by a single institution, both in terms of manpower and capital and operating costs.

Since the inception of the NRAO, though, a second important task has been assumed by the National Observatory; this is the task of providing facilities of typical university-type size (i.e., not the largest or most expensive) for those astronomers who do not have such equipment available at their own institutions. Thus, the national observatories play a dual role as providers, both of the highest-level and of standard-type equipment.

If all of this can be provided at national observatories, why should there be any university-based radio observatories? The reason is that the roots of the U.S. radio-astronomical effort lie in the universities, and it is clear that we are restricting future development if we give insufficient support to the existing and developing radio-astronomical centers at universities. It is principally from these centers that come the scientific leaders at the professional and graduate-student levels who produce the scientific work that has resulted in so many developments in the total radio-astronomical effort in the nation. Instrumentation at large national facilities, because it has to be versatile enough to enable astronomers with widely varying background and interests to use it, must in general be planned long in advance and in great detail, and there is little time to experiment. But only by allowing experimentation can we continue developing the new ideas that in the past have played such an important role in the development of new equipment and the discovery of new phenomena. It is this experimentation, in which graduate students can take part, that gives students in radio astronomy such an unusually broad background in physics and engineering. The radio-astronomy PhD is effective in many areas of modern technology; indeed, we expect that in the future an increasing fraction of radio-astronomy PhD's will not become professional astronomers but will go into industry.

New ideas and new initiatives traditionally come out of the universities with in-house programs. The university radio astronomer is the one who advances technology—and not only in his own field—and who guides the national observatories along the way to excellence, versatility, and forefront equipment. The objective at the university facilities will be to stay competitive in particular areas of research. To this end there will have to be continual programs of modernization, and so only those groups that have a capability in equipment development, especially electronics, will be able to survive. On the other hand, it is imperative that these groups, which have the interest and capability, be properly funded, so that their telescopes can be used to best advantage. Only healthy operating budgets at the university facilities will make possible a fruitful interplay between them and the national observatories and thereby guarantee the success of radio-astronomical research in the nation as a whole. Unfortunately, the severe shortage of funds during the late 1960's and early 1970's threatens to distort this balance. The funding of new and major instrumentation at national observatories must go hand in hand with considerable increases in the budgets of existing university facilities; only in that way can we make the new instruments fully efficient.

The Panel *recommends* that:

1. Construction of new instruments at university facilities should continue, in order to encourage new research ideas and provide stimulation for the most creative use of national facilities. In some cases, where outstanding competence exists, major new university instruments should be provided.
2. Support for new operations, new state-of-the-art equipment, and maintenance of existing university facilities must be maintained at a level that will allow effective research.

IX. OTHER RECOMMENDATIONS

A. RECOMMENDATION FOR SOLAR RADIO ASTRONOMY

Solar radio astronomy is represented in the United States by only a few active groups. Nevertheless, its contributions to astronomy are many, and its potential for the future appears to be rich. We therefore *recommend* additional encouragement and funding in this area.

Pencil-beam antennas now exist that form images of solar bursts in the vhf range. There is a critical need for this kind of observation over the range of wavelengths from hectometer down to millimeters. There is also a need for several equipments of this type even at a single frequency, since the observations should be studied synoptically.

Fast narrow-band spectroscopy of solar bursts at metric and longer wavelengths is also critical. Time constants here may usefully be as short as milliseconds, and bandwidth as narrow as a few kilohertz.

Finally, the development of solar radars has been virtually halted, after the initial investigations of the total spectrum of the returns. The need here is for additional spatial resolution of the returns, that is, in their angle of arrival. Equipment that can distinguish returns from different parts of the corona is necessary in order to untangle these complex echoes. It promises to provide many details of the nature of wave structure in the corona at heights of half a solar radius and higher that may be otherwise unobservable.

B. RECOMMENDATION FOR THE DEVELOPMENT OF MILLIMETER-WAVE INTERFEROMETRY

Realizing that the use of interferometry in astronomy is the only way to increase resolution manyfold, and recognizing the problems associated with studying very small objects at millimeter and centimeter wavelengths, the Panel *recommends* that the techniques of millimeter-wave interferometry be developed. Effort in this area by qualified groups should be given adequate support. In addition, after the large millimeter-wave telescope has proven its usefulness, consideration should be given to construction of a second, movable large millimeter-wave telescope to complement and extend the millimeter-wave facility.

C. RECOMMENDATION FOR LONG-WAVELENGTH RADIO ASTRONOMY

Long-wavelength radio astronomy ($\lambda > 1$ m) was originally the main area of activity for radio astronomers. The continuing concerns at these wavelengths reflect interest in astrophysical plasmas, such as the solar atmosphere, the solar wind, Jupiter's atmosphere, and the nonthermal galactic and extragalactic sources. The discovery of pulsars has brought much new interest to the wavelength region because pulsars have very steep spectra and usually are seen best at long wavelengths.

The Panel notes with approval that U.S. radio astronomers now have or soon will have available several of the world's largest and most versatile long-wavelength telescopes. The Panel *recommends* that long-wavelength radio astronomy be encouraged, and that the existing long-wavelength facilities be funded at a high enough level that they can operate with maximum efficiency.

D. RECOMMENDATION REGARDING NASA TELESCOPES

The NASA telescopes at Goldstone and other places are among the most sensitive radio detection systems in the world. Five percent of the time at these telescopes has been devoted to scientific use. They have been used by a small but growing number of radio and radar astronomers from the United States and other countries and have given important scientific information. For example, observations have been successfully carried out on pulsars, interplanetary scintillations, and very-long-baseline interferometry of compact extragalactic objects, while Mercury and Venus have been observed with bistatic radar at 3.8-cm wavelength. The Panel notes with approval that a band around 3.8 cm has recently been made available at the 210-ft telescope, in addition to the 13-cm band; and that development for a 2-cm band is nearing operation. The Panel *recommends strongly* that NASA add generally usable spectroscopic facilities to the 210-ft telescope. The Panel also *strongly recommends* that the time allotment for scientific use be increased substantially. The two new 210-ft telescopes abroad should also be available to radio astronomers.

CHAPTER TWO

Optical Astronomy

PANEL MEMBERS

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I. SCIENTIFIC OBJECTIVES

During the past decade, many entirely new and unexpected objects have been found in our universe, such as quasars, pulsars, x-ray stars, and infrared galaxies. Although most of these discoveries have come from exploring the sky in the regions of the electromagnetic spectrum that have become accessible through radio telescopes and space vehicles, our understanding of these recent discoveries usually depended on a close collaboration with optical astronomy. This classical field, with its unexcelled capacity to record images, its good detector sensitivity and frequency resolution, and its long-time baseline of observations, continues to provide information essential for the identification and interpretation of observations at other wavelengths. Consider how different would be our picture of the quasars if the optical emission lines had never been discovered and interpreted as red shifts.

Of course, optical astronomy on its own has also found new kinds of objects, such as stars with unusual or apparently variable surface chemical composition, circularly polarized and variable degenerate dwarfs, and slowly rotating stars with very large surface magnetic fields.

Nevertheless, the importance of ground-based optical astronomy goes far beyond its contribution to the more exotic objects new to astronomy in the last ten years. In this relatively mature field, observations are sufficiently precise and theoretical understanding sufficiently advanced to allow detailed comparisons between observations and conceptual models. Thus, the well-developed theories of stellar atmospheres and interiors now permit us to interpret our observations with confidence and to begin to answer some of the interesting scientific questions that underlie all astronomical investigations. For example, what are the chemical compositions of stars, how were they formed, and what are their probable futures? In the last decade, optical astronomy has begun to provide the data pertinent to answering these questions. With the instrumentation planned for the next decade we hope to answer similar questions about the origin and evolution of normal galaxies.

However, because of the current interest in the fascinating new phenomena discovered by radio, infrared, and space astronomy, we shall discuss first the importance of optical observations to these new fields and then return to the investigations unique to optical astronomy.

A. PARTICULAR CONTRIBUTIONS OF OPTICAL OBSERVATIONS TO OTHER FIELDS OF ASTRONOMY

Optical measurements are fundamental to our understanding of objects discovered in other spectral regions for many reasons:

1. The high angular resolution of 1 sec of arc or better available at optical wavelengths gives a two-dimensional picture with an amount of information not obtainable at other photon energies, unless the source has a very simple structure.
2. Optical spectra give the key information about temperatures, luminosities, densities, compositions, and radial motions.
3. Distance estimates normally depend on optical data through relating the new object to familiar objects of known absolute brightness.
4. At visual wavelengths there is a vast body of understanding already available to help interpret the new object, and there often is historical information on light variability and proper motion.

In fact, data at visual wavelengths are so necessary that whenever a new object is located in another energy band, the discoverer immediately contacts a major optical facility to arrange for observations.

B. EXAMPLES OF THE IMPORTANCE OF OPTICAL OBSERVATIONS

1. *Quasi-stellar Objects*

The study of quasi-stellar radio sources provides one striking example of this interplay between the fields of astronomy. The development of sensitive radio receivers and the instrumentation to determine accurate radio positions enabled many radio sources to be identified with objects having a stellar appearance and thereby disclosed the existence of an entirely unsuspected constituent of the universe—the QSO's or quasars. The subsequent discovery of the very large red shifts associated with some of these objects, now amounting to $\Delta\lambda/\lambda_0 = 2.88$ in the largest case, greatly increased the interest in these objects and is a contribution that so far is uniquely within the province of optical astronomy. A by-product of the discovery of QSO's with large red shifts has been to open up the ultraviolet spectral region longward of 800 Å for spectroscopic study from the ground.

The nature of the absorption-line spectra in these objects is still a complete enigma. We expect that the lines will provide clues not only about the QSO's themselves but also about possible matter between the galaxies. Studies of these exceedingly faint objects tax to the limit the capabilities of the largest optical telescope. While spectral features are not unique to optical astronomy, the ability of optical spectroscopy to detect minute amounts of material is impressive. Typically 10^{20} hydrogen atoms per square centimeter in the line of sight are required to produce an optical depth of unity in the 21-cm line, but 10^{-7} of this amount of gas will produce the same effect in the red-shifted Lyman-α absorption line in a QSO.

2. *Galactic Nuclei*

The discovery of the quasi-stellar objects has spurred intense interest in the related area of the nuclei of galaxies. The various techniques available to the optical astronomer, including spectroscopic analysis, photometry, polarimetry, and detailed two-dimensional filter photography, have now made it clear that violent activity in galactic nuclei is a much more frequent occurrence than we had suspected when the first optical identifications of radio sources were made. The subsequent remarkable observations of infrared radiation from these nuclei have only served to deepen the problem associated with the energies in galactic nuclei. Optical observations are essential to show the general arrangement of the stars, which provide much if not most of the gravitating mass and probably play an important role in the origin of these complex phenomena in the nucleus. High-resolution pictures of large numbers of galaxies cannot be obtained in any other way at this time.

3. *X-Ray Sources*

X-ray astronomy provides another example of the importance of the optical observations. Some of the earliest x-ray detectors were directed toward the Crab nebula, which was known to be a very unusual object even in the days before radio and space astronomy. While numerous discrete x-ray sources have since been discovered, our ideas of their nature and physical condition have been derived mainly from the handful of optically identified sources, notably the Crab nebula and Sco X-1. The same is true of our ideas concerning possible extragalactic x-ray emitters. A few abrupt breakthroughs may result from nonoptical studies, but it seems certain that continuing progress in understanding the nature of the x-ray sources will require an increased ability to study these objects optically. It is, therefore, discouraging that so few have been identified; they are apparently very faint optically, and it is clear that increased light-gathering power is necessary if we are to keep pace with the wealth of data now being acquired by x-ray satellites. The need will be specially urgent following the launch of the first High Energy Astronomical Observatory (HEAO) planned for 1975.

4. *Pulsars*

Optical astronomy also has been of crucial importance in our understanding of phenomena whose connection with the optical spectrum might at first sight seem remote. The radio pulsar that has received the most attention and discussion has been the Crab pulsar for the very good reason that its optical identification with the Crab remnant significantly aids us in its interpretation. From the purely technical point of view, the ability to time optical pulses very accurately and to study their shapes free from the distorting influence of the interstellar medium has been of considerable value.

5. *Cosmic Background Radiation*

An additional similar example involves the 3 K background radiation where optical data on the strengths of molecular absorption lines have provided a firm measure of this background in a crucially important spectral region inaccessible with ground-based telescopes and have given limits at somewhat shorter wavelengths, setting severe constraints on the nature of this radiation. These optical observations, though simple in principle, have demanded a large amount of observing time at the coudé spectrographs of the largest telescopes.

6. *Cosmology*

It now seems clear there will be no rapid solution to the long-standing question of the overall structure of the universe. In the usual approach, the observer seeks to determine the parameters describing the geometry and curvature of the universe by means of the red-shift-magnitude relation for the normal galaxies whose properties are thought to be reasonably well understood and whose intrinsic luminosities exhibit a relatively small scatter. Because of observational errors and the difficulty of making accurate evolutionary corrections, small cosmological effects are difficult to detect. It will be necessary to push to much larger red shifts and considerably fainter magnitudes before the differences between cosmological models lead to definitive observational tests. This will be an extremely time-consuming operation on the very largest telescopes.

A second important question arises from studies of numbers of radio galaxies and quasi-stellar objects at various apparent flux levels that indicate that there is a rapid change in the number of sources per unit volume with increasing distance. However, without the red shifts determined by optical telescopes we cannot tell whether the change is due to evolution in the objects or a cosmological variation in their distribution. Furthermore, there are still only a small number of optically identified sources when compared with the vast number of sources now appearing in the most recent radio catalogs. To pursue either a continued program of optical identification or a systematic study of the statistics of quasars selected solely by their optical properties will again severely tax the capabilities of the very largest existing optical instruments. For significant progress on these problems in the next decade, light-gathering power equivalent to several additional telescopes of the 150- to 200-in. class will be required.

C. RESEARCH AREAS UNIQUE TO OPTICAL ASTRONOMY

In addition to these recent spectacular advances in which progress has been dependent upon the fruitful interplay between traditional ground-based optical observations and the newer branches of astronomy, there are several investigations where important progress has been made, and probably can continue to be made, only through optical techniques. One example is the further study of the history and evolution of the chemical elements within our galaxy and, for that matter, in external galaxies. Here the wealth of spectroscopic information on this problem has only begun to be fully exploited. We still do not understand at all clearly the manner in which nucleosynthesis has proceeded either as a function of time or as a function of position in our own galaxy. Obtaining high-dispersion spectrograms of unevolved stars in our halo, or objects in the Magellanic Clouds and other nearby galaxies, is still not feasible for even the most powerful optical telescopes. Nevertheless, many unexpected and striking discoveries continue to be made by spectroscopic techniques. Technetium, whose longest-lived isotope has a radioactive half-life of 3×10^6 yr, has been found in some stellar atmospheres; and one star may contain promethium, which has no isotope with a half-life longer than 18 yr.

The origin and apparent persistence of the spiral structure in galaxies has long been a great puzzle, but recent theoretical work indicates that the pattern may be a density wave passing through the rotating disk of gas and stars, with star formation occurring at the peaks. Kinematical studies of the motion of stars will play a crucial role in testing the validity of these theories, while the interplay of optical and radio astronomy will be essential for studying the motions of the gas flow. As a final example of a recent optical discovery, whose implications for theoretical astrophysics are still far from clear, we may mention the discovery of a high degree of circular polarization in several white-dwarf stars giving evidence for atmospheric magnetic fields a million times the strength of the earth's surface field.

When we consider the spectacular growth in radio, infrared, and space instrumentation, it is remarkable that optical astronomy has been able to make such significant contributions, enhancing the new fields and at the same time continuing all the old ones. Many optical facilities are not so modern as one might wish, and, more seriously, several of them are located in sites that are deteriorating rapidly due to the pollution of the night sky from the lights accompanying growing urban areas. For all these reasons there has been a very rapid growth in the demand for observing time at large telescopes. The full utilization of the radio, infrared, and space measurements depends on such optical observations, but the total collecting area in dark sites has not grown at the pace necessary to meet the requirements of these new fields or even to serve the current needs of traditional ground-based optical astronomy.

II. NEW FACILITIES AND INSTRUMENTAL DEVELOPMENTS NEEDED FOR OPTICAL ASTRONOMY

What are the immediate needs of optical astronomy? First is an increased rate of gathering photons in order to exploit fully the recent discoveries and maintain a balanced pace with the sister disciplines. For example, a recent measurement at high dispersion was made of the absorption lines in a small portion of the optical spectrum of one of the relatively bright quasi-stellar sources with very high red shift. Even with the largest telescope in the world and one of the most sophisticated modern detecting systems, this effort required an integration time of half a night. Evidently, to extend similar studies to a large sample of objects two or three magnitudes fainter would be prohibitive. Similarly, obtaining the red shift of even a single 22nd magnitude galaxy with 10 Å resolution to pursue the conventional cosmological problem would require observing for two nights at the 200-in. telescope.

As a further example, it has been rather discouraging, but at the same time fascinating and puzzling, that no additional optical pulsars have been found. It is clear from the limits that have already been set that very large photon-gathering rates are required. Aside from providing information on the coherent radiating processes in a spectral region of several decades higher energy than radio frequencies, the setting of the lowest possible optical limits on pulsars is of interest in discussing the thermal properties of the presumed neutron stars responsible for the phenomena. We have already alluded to the desirability of optically identifying x-ray and radio sources, and here again it is clear that increased light-gathering power is necessary in order to keep pace with the copious data now being acquired. An operating x-ray Explorer is now in orbit, and one major institution has assigned 200 nights a year on various instruments to follow up this NASA mission. *Uhuru* has found a wide variety of x-ray sources, from quasars to eclipsing x-ray variables.

Fortunately for the optical astronomer, there are developments in technology that promise to improve detector efficiency and increase collecting area at a rather low cost compared with some of the large-scale facilities required by some other branches of our science.

A. ELECTROOPTICAL DETECTORS

It is now possible to build one- and two-dimensional image detectors with quantum efficiencies 10 to 30 times greater than photographic plates. The image intensifier tubes now in use at most large observatories are the forerunners of these arrays. In the next generation of detectors we want to try to combine the best features of the photographic plate and the photomultiplier such as (a) large size, up to several inches in diameter, to accomodate wide-field telescopes; (b) resolution down to the order of 10 μm; (c) very low dark-count rates; (d) high quantum efficiencies for visual and infrared wavelengths; (e) no threshold; (f) linearity over a very wide range of intensity or the ability to count individual photoelectron events; (g) unlimited storage capacity; (h) convenience in operation; (i) low cost; and (j) the ability to monitor the observations as they are being acquired.

It is clear that we are still a long way from having all these features in a single device. However, there are several promising electrooptical schemes that will greatly benefit observational astronomy and should be developed and implemented immediately.

1. Photographic Image Intensifiers

Improved versions of the original image intensifier continue to be developed with increased sensitivity and resolution and reduced noise. Many observatories will want to replace their present models with the new ones while awaiting perfection of some of the more sophisticated items described below. The estimated cost, with accessories for each improved intensifier, is $15,000.

2. Integrating Television

An integrating TV system has been developed at Princeton which is capable of exposures up to 10 h with high effective quantum efficiency and is quantum-noise limited over most of its dynamic range of 2000 photoelectrons per picture element. New scientific results have been obtained with the system on several different telescopes, including the 200-in., and similar systems are now in use at two other institutions. This detector is a significant improvement over previously available techniques for many applications and would be useful at all major observatories. Now that the development is essentially complete, funds should be provided for several observatories to obtain integrating TV systems. The cost of such a system is approximately $75,000 to $100,000.

3. Photoelectron Counting by Television

Another approach uses a TV sensor to count discrete photoelectrons in a one- or two-dimensional image. In one scheme, now being perfected by the British, a four-stage image intensifier is viewed by an SEC vidicon. In the United States, the NSF is supporting the development of a simpler sensor with the potential for higher performance. This system will have a single intensifier in the same envelope as a TV tube with a silicon target to give a total gain of 100,000 electrons per photon compared with an rms noise of 2000 electrons. Presently available sensors appear capable of a limiting resolution of about 15 μm. The high gain results in a dynamic range of about 10, so that it is necessary to scan the target frequently enough to detect single events and assemble the image numerically in a digital memory. This approach has the great advantage that the growth of the image can be monitored during the exposure. The estimated cost per system after development is approximately $150,000. A TV system that can record discrete photoelectrons in a two-dimensional picture would be specially useful for the measurement of weak signals that are limited by their own quantum noise rather than sky background, as for example in high-dispersion spectroscopy. Also, since no strong charge pattern is built up between readouts, the observation of a faint galaxy associated with a bright nucleus should not be disturbed by any bending of the readout beam. The unlimited dynamic range would permit the level of the night sky to be measured with sufficient accuracy that signals of stars, nebulae, and galactic bridges lying only a few percent above the sky should be observable.

4. *Intensifier with Image Dissector*

A third approach, which has been applied successfully at the Lick Observatory, uses multistage intensifier tubes and an image dissector tube to read out the final phosphor. The cost is about $20,000, excluding data storage; this is less than many other systems, but the effective quantum efficiency is lower and the approach appears to be limited to one-dimensional data such as spectra or a two-dimensional array with some 10^4 elements.

5. *Intensifier with Silicon Diodes*

A fourth approach, developed at the University of California, San Diego, and elsewhere, involves a row of silicon diodes at the anode of a single-stage image intensifier. Single photoelectrons are accelerated by 20 to 30 kV and generate a detectable pulse upon striking a silicon diode, each of which is connected to a separate amplifier–discriminator. The present 40-channel device costs approximately $15,000. A 200-diode sensor is under development, and two-dimensional arrays of up to 1000 elements are possible, but the extension to 10^4 or more diodes would be very expensive.

6. *Electronographic Camera*

The electronographic camera developed by Lallemand has now been used successfully by several astronomers. Its resolution, quantum efficiency, linearity, dynamic range, information storage capacity, dark count, and gain are excellent, and the photocathode apertures of 2.5 cm may be increased to 10 cm in the future. Current efforts should be encouraged to reduce the complexity of this system and the special attention needed for its operation. A major requirement is the development of a sophisticated microdensitometer to measure the fine-grain plates. Once perfected, the improved camera with large cathode and easier operation should cost approximately $40,000, not including the densitometer.

In most cases, the very expensive development costs for a new type of detector will have to come from government agencies and industrial laboratories interested in the space, military, or commercial applications, but the major observatories still must assemble competent staffs to experiment with the new devices and adapt them to telescopes and spectrographs. When one considers the factor of 10 or more gain in the rate of gathering information, along with improved accuracy, and that such gains are applicable not only to the generations of telescopes yet to be built but also to all existing large instruments, it is clear that strong and vigorous support of such a detector program is essential. Once we have detectors that will count photons with high efficiency, a factor of 10 increase in data acquisition corresponds to a tenfold increase in the available telescope area for many types of measurements.

B. TELESCOPE ACCESSORIES

Other modern equipment can make significant improvements in the efficiency with which a large telescope is used. Examples are automatic setting controls to reduce the time spent between objects, television cameras to view the star field to permit setting and guiding on faint objects, automatic guiding systems to give sharp pictures and uniform spectra, and observatory computer reduction of data while it is being collected so that optimum exposures can be obtained.

C. LARGE TELESCOPES AND ARRAYS OF MIRRORS

Even when we have detectors that can count 20 percent or more of all photons reaching a mirror, research on the frontiers will be limited by the very long integration times necessary to study the faintest objects. A measurement requiring one night at the 200-in. telescope is simply not practical spread over 10 nights at a 60-in. telescope. More telescopes of the 200-in. class and larger will be needed to investigate the large-scale structure of the universe and the puzzling objects that it contains. For example, obtaining high-resolution spectra of the absorption lines in the fainter QSO's will not be feasible unless the collecting area equivalent to several 200-in. telescopes can be devoted to the problem.

It now seems likely that any significant increase beyond the light-gathering power of a 200-in. telescope will require radical departures from the conventional design. Recent studies have considered alternatives such as an array of moderate-sized mirrors with superposed focal planes all on a single alt-azimuth mount or a system of individually mounted telescopes whose light can be brought to a common fixed focus. The preliminary investigations suggest that such an instrument could be made with collecting area and image quality comparable with those of the largest conventional telescopes and at less cost. For even larger areas, an array may be the only practical system. However, these concepts must be proven in actual observing situations.

Therefore, we *recommend* further design studies and prototype development, followed by construction of an array with an effective aperture in the 200-in. class at a dark site that could then be used as an observatory. It is important that special consideration be given to the matching of modern detectors to the beams produced by such arrays. Any comparison with conventional reflectors must not neglect the extra maintenance and operating costs that may be required for an array, as well as the additional detecting systems necessary if the images are not superposed optically.

If the experiment is successful, we would have the urgently needed additional light-gathering power and the basis for planning much larger systems. On the other hand, if we find that an array does not have the capability of producing a seeing-limited image at low cost, we must proceed directly with another single-mirror telescope of the 200-in. class.

For the full utilization of telescopes with apertures exceeding 200 in., the development of recording devices with negligible detector threshold and noise is essential so that images need not be concentrated into areas as small as 15 μm square. For seeing of 1 sec of arc at the 200-in. telescope, an $f/0.6$ camera must be used to image a star or element of a slitless spectrum within a resolution element of a photographic plate or photographic image tube, and this camera speed must increase in proportion to the telescope diameter unless sites with significantly better average seeing are found.

D. INTERMEDIATE-SIZED TELESCOPES

The deterioration of the night sky at many existing observatories because of lights from expanding urban areas constitutes a serious problem for large groups of optical astronomers who are rapidly being deprived of the ability to pursue research on the faintest objects. In effect, merely to maintain our present capabilities, let alone increase or improve them, it is necessary to build new telescopes at dark-sky sites. Furthermore, competent groups of astronomers have been brought together at institutions where there is no access to instruments at favorable observing sites, except for the heavily taxed facilities of the Kitt Peak National Observatory and the few other institutions with large telescopes that provide small amounts of time for individual guest investigators.

These situations create the demand for construction of at least four new optical telescopes 90 in. or more in diameter during the next decade. These instruments will cost from $3 million to $5 million each with an extra $1.5 million or $2 million if site development is also required. The alternative of moving a threatened large telescope to a darker site should be considered, but preliminary studies indicate that relocation would amount to between one half and two thirds of the cost of a new telescope, excluding any site development. It therefore seems preferable to build the new instrument, because the existing instrument can still continue to be useful and in great demand for observing brighter objects both spectroscopically and in the near infrared.

The new auxiliaries such as image intensifiers and rapid-data-handling devices have increased the power and usefulness of this size of telescope in a good location so that it can remain a valuable research tool for many decades. Furthermore, if the design gives serious consideration to the requirements of infrared astronomers, including location at a dry site, the telescope can be used during the twilight and many of the daylight hours too.

The first of these intermediate-sized instruments is urgently needed and should be started immediately. Possible locations might be Kitt Peak in Arizona, Mauna Kea in Hawaii, Mt. Locke in Texas, or the undeveloped site on Junnipero Serra Peak in California, all of which will be safe from city lights long enough to make the investment worthwhile and to relieve some of the present demands for dark-sky light-gathering power.

E. DARK-SKY SITES

At the same time as we plan for new construction, the rapidly increasing night sky brightness at all major observatories in the United States, except for Mt. Locke and Mauna Kea, requires that we continue to search for new first-class dark-sky sites, particularly in the northern hemisphere, that have good seeing and will be safe from lights for several decades. One location deserving immediate consideration is the new Mexican National Observatory in Baja California. If observing experience there confirms the expectations for good seeing, clear weather, and dark sky, the possibility of cooperative projects between Mexican and American astronomers should be investigated.

F. INTERFEROMETERS

Optical interferometers can provide fundamental measurements of angular dimensions smaller than the usual limits of 0.5 to 1 sec of arc set by atmospheric seeing. Around 1920, Pease used a Michelson interferometer on the 100-in. telescope to measure the angular diameters of several red supergiants, but variations in the visibility of the fringes made the results very uncertain. Some recent experiments using modern techniques have shown that additional angular information can be obtained from the images formed by large optical telescopes, either from examination of the Fourier components in the entire seeing disk or by measurement of the fringes formed between two small areas of the same mirror, but these methods cannot be pushed beyond the 10^{-2} sec of arc limit of the 200-in. telescope. For the last eight years, Australian astronomers have successfully used an intensity interferometer to measure diameters of stars hotter than the sun and as small as 4×10^{-4} sec of arc. Experiments are now under way to test the possibility of operating an infrared interferometer on the heterodyne principle of beating a laser against starlight at 10 μm, where our atmosphere may not disrupt the phase correlation so badly. Lunar occultations also can provide angular measurements. but they are limited to 10^{-3} sec of arc on a few cool stars.

1. The Intensity Interferometer

The Australian astronomers are now considering a new larger installation with mosaic mirrors totaling 160 m^2 of collecting area on tracks permitting effective baselines up to 1800 m. Stars of all types could be measured to visual magnitude 7.5, and if red-sensitive detectors are used, some fainter M dwarfs would be accessible. The estimated cost is $4 million.

Such an instrument would be the source of much valuable fundamental data including: (a) reliable angular diameters and hence radii, surface fluxes, and effective temperatures for stars of almost all spectral and luminosity types, including many peculiar ones; (b) the angular sizes of envelopes around Wolf-Rayet, Of, Be, and other emission line stars; (c) the distortion of the brighter rotating stars, including a critical test of whether they are rotating nonuniformly or as solid bodies, through comparison with the known rotational velocities; (d) limb darkening of the brightest stars, especially giants and supergiants; (e) the relation between angular size of the brightest Cepheids and their variations in light and velocity, to check models and obtain geometrical distance estimates; (f) masses and geometrical distances of many double-line spectroscopic binaries through combination of measurements of angular separation and radial velocity; (g) the detection of new double stars too closely spaced for visual observation; and (h) the total interstellar absorption affecting reddened stars, through comparison of the angular diameters of stars expected to have similar absolute surface fluxes. The intensity interferometer is a proven system, and no major technological developments are needed to build a larger one except for the high-efficiency infrared photomultipliers required to reach main-sequence stars cooler than M3.

2. *The Michelson Interferometer*

In principle the Michelson interferometer can be applied to the same set of problems with the advantage of reaching fainter objects in the same observing time. For example, if a dependable method of fringe detection can be developed, it should be just possible to obtain 2×10^{-2} sec of arc resolution on the 12th-magnitude nucleus of the Seyfert galaxy NGC 4151 with two 10-cm apertures at the edges of the 200-in. mirror. Then we might learn whether the stars that may be in the nucleus are close enough together to collide with each other. In order to reach resolutions of 10^{-3} sec of arc, to study possible changes in the size of the Seyfert nucleus that might accompany the light variations, or 10^{-4} sec of arc to compete with the intensity interferometer for many types of star, the collecting mirrors must be separated by 100 or 1000 m, respectively. Then the areas of the apertures must be increased to compensate for the decrease in the optical bandwidth necessary to relieve the tolerance on the equality of the two arms. If we permit a maximum of 200-μm difference in the two light paths, the bandpass at 5500 Å must be limited to 1 Å, and then the brightest quasar, 3C273 at $V = 12.8$, would require 75-cm-diameter apertures.

The feasibility of such Michelson interferometers still must be demonstrated, and even then a major amount of development will be required before many significant astronomical measurements are possible. The principal problems to solve are the following: (a) A dependable, objective technique for fringe detection must be perfected under real observing conditions with apertures not exceeding 10-cm in diameter, the limit of transverse phase coherence in our atmosphere. If the technique is to be applicable to baselines of 10 m or more, the two mirrors must have independent mounts. (b) The use of larger mirrors must be investigated to see whether the fringe visibility can be derived by special sampling and analysis of the pattern at a number of points in each aperture. (c) Practical schemes must be devised to equalize the light paths as the instrument tracks a star across the sky. One plan would mount the arms parallel to the earth's rotation axis, but that would limit the interferometer to measurements only in declination. These are challenging problems but worth pursuing because modern developments such as photomultipliers, television cameras, star trackers, and lasers could provide major improvements over the visual observations of Pease.

The Optical Panel concludes that at present the intensity interferometer is the only demonstrated way to obtain fundamental data on a wide variety of objects with angular sizes of less than 10^{-2} sec of arc, and therefore construction of a large instrument is worthy of support. The Australians, who are planning such an instrument, would welcome the collaboration of the United States; this would provide notable opportunity for international cooperation in astronomical research. We *recommend* that the United States Government explore with the Australian Government the possibility of cooperative funding in the construction of a large intensity interferometer, which is estimated to cost a total of $4 million. The variety of stars available for study is nearly the same in each hemisphere, so that the actual location would depend more on the frequency of clear skies and proximity to necessary technical facilities.

Although feasibility of the Michelson interferometer has not yet been proven, it has the potential for measurement of fainter objects than possible with an intensity interferometer. The development will be a challenging problem that deserves modest support for preliminary investigations at this time. Funding up to $200,000 would be appropriate for experiments in fringe detection with real-star images and for design studies of methods for tracking a star and equalization of the optical paths. These studies and similar ones now being pursued by the British and French should then be used for critical assessment of the ultimate potential of the Michelson in comparison with the large-intensity interferometer. If the Michelson can be shown to be technically possible with distinct scientific advantages, support should be given for the construction of a working model with a baseline in the 100-m range.

G. LARGE SOUTHERN SCHMIDT

Experience on Palomar Mountain has shown that a Schmidt telescope is an indispensible auxiliary tool for a large reflector. The wide field of view of the Schmidt permits it to photograph a region of the sky where a new radio, infrared, or x-ray object has been discovered to help to single out possible candidates for detailed investigation with the large telescope. A photographic survey of the whole southern sky is planned for a European Schmidt to be located in Chile, and the British are building a 48-in. Schmidt in Australia. However, these instruments will not be available to study specific areas in support of the two large reflectors that the United States will soon build in Chile. Consequently, it is important that the United States also install a large Schmidt telescope there at the earliest possible opportunity. The new instrument should have an objective prism and a plate scale 50 percent larger than the Palomar one. Such a Schmidt telescope would cost $4 million.

H. SUPPORT OF A GRATING LABORATORY

Spectroscopy at all wavelengths depends on continued developments in diffraction-grating technology to increase efficiency, ruled area, and number of rulings per millimeter. For example, an essential feature in the design of a high-dispersion coudé spectrograph is the size of the grating. For a given linear dispersion, a larger diameter collimated beam will permit a larger collimator focal length and hence a wider entrance slit so that less light in the seeing disk will be wasted. Alternatively, many of the new electrooptical detectors are best matched with the two-dimensional echelle pattern produced by a spectrograph containing a pair of gratings whose dispersions are at right angles. Consequently, there is much interest in developing holographic manufacturing techniques that could incorporate both dispersion and cross-dispersion rulings and even optical power into a single element. These advanced methods, as well as the ruling of gratings by interferometric control or lasers, should be pursued in a specialized grating laboratory for the entire astronomical community. The initial expenditure plus maintenance for the decade would be $3 million.

I. SUPPORT OF RESEARCH ON THE PHOTOGRAPHIC PROCESS

Even with major advances in electrooptical systems, direct photography will continue to be important for astronomy, particularly for astrometric and wide-field observations. The expensive efforts required to improve emulsions are best left to the photographic industry, but support should be provided for astronomers to investigate the techniques of sensitization and developing. The Panel *recommends* the funding of at least two existing laboratories for the study of improved photographic techniques, up to a total of $0.5 million for the decade.

J. ASTROMETRIC INSTRUMENTS

Astronomers continue to depend in many ways on the data provided by astrometric programs. Absolute positions of optical objects are required to high accuracy for comparison with the positions obtained from radio interferometry. Absolute proper motions are necessary for the determination of galactic rotation, the cluster parallax of the Hyades, secular parallaxes of the RR Lyrae stars, and the zero point of the Cepheid period–

luminosity relation, as well as the precession of the perihelion of Mercury. Therefore installation of additional automatic transit circles, astrolabes, and photographic zenith tubes, particularly in the southern hemisphere, along with support of programs for measuring proper motions relative to galaxies in both hemispheres, are required for further improvement in the fundamental coordinate systems. For the analysis of parallax and proper-motion plates, more automatic measuring engines are needed because they are capable of decreasing the measuring errors to a fraction of a micrometer and at the same time enormously increasing the speed with which the plates can be reduced. Finally, there are several existing refractors particularly suitable for astrometric observations that could be more effectively used after a modest amount of modernization and the installation of automatic guiding cameras. These instruments are further described in the report of the Working Group on Dynamical Astronomy, where a total budget of $6.4 million is indicated for instrumentation. The Optical Panel supports this program and in addition believes that experiments should be encouraged to develop photoelectric techniques for measuring relative star positions directly at the telescope focus. Such methods have the potential of defining the center of a star image much more accurately than a photograph and thus could give significant improvements in parallaxes, relative proper motions, and double-star separations.

A special-purpose 61-in. astrometric reflector is in operation near Flagstaff, Arizona. The first list of 100 parallaxes has been published, and further parallax work is in progress. Given the accuracy of this first series along with discussions of the accuracy obtained with properly used reflectors, we conclude that it is important for a wide range of problems in astrophysics, galactic structure, and stellar dynamics that sufficient observational information be obtained in the southern hemisphere as well. It is not obvious that the Flagstaff design should be copied, since it is both expensive and somewhat wasteful of light with a large flat secondary. Instead, we *recommend* that special precautions be taken in the mechanical and optical design of the next southern hemisphere reflector to be built in the 60- to 90-in. class. Essentially a Ritchey-Chrétien design will provide adequate field at the Cassegrain position, and the accuracy of collimation can be established by proper imaging collimation devices. Since a large gain in accuracy arises from the use of automated measuring engines, a program of southern proper motions and parallaxes based on a sturdily build general-purpose reflector with a modern automatic measuring engine becomes an important part of southern hemisphere research. It should be realized that an extensive parallax program involves a large commitment of time and bears fruit only slowly.

III. PRIORITIES FOR OPTICAL ASTRONOMY

A. FIRST-PRIORITY PROJECTS

Among all the important projects just described, certain ones stand out with the highest priority for immediate funding. These are the facilities and developments in optical astronomy essential for continued progress toward understanding our universe, both by comparison with observations in other energy ranges and by studies unique to visual wavelengths. The Optical Panel has concluded that the following items constitute a minimum program for ground-based optical astronomy in the next decade:

1. (a) Continued development, following a variety of approaches at several institutions, of various electrooptical devices to provide more efficient detectors than unaided photographic plates. Funding must also be provided to permit the best of these devices to be purchased and applied to existing and planned optical facilities. The Panel estimates, on the basis of the number of groups actively involved in such development and the observatories that will want to obtain working devices, that at least $10 million will be required over the next 10 years for this purpose.

(b) As a necessary adjunct to such sophisticated detectors, and in order to make full use of their power, it is necessary to have high-speed data-handling equipment and other devices to make telescopes more efficient, for example, television cameras and automatic controls to assist in setting and guiding. We estimate the cost of introducing such systems into existing major U.S. observatories to be approximately $5 million.

2. Construction at the earliest opportunity of a large optical telescope in the 200-in. class at a safe dark-sky site with good seeing. Initial efforts should concentrate on detailed engineering design studies for an array or multiple telescope system along with construction of model prototypes that together demonstrate whether the technical problems intrinsic to these approaches can be solved. Actual construction of such a telescope equivalent to a 150- or 200-in. in collecting area should then proceed. An operating telescope of that size would cost about $5 million without auxiliaries or major site development. If the array concept is successful, the design and construction of a much larger system in the 400- to 600-in. class should be pursued. However, if this concept does not fulfill expectations, the Panel advocates the construction of a conventional single-mirror reflector of the 200-in. class, which would cost $22 million on a developed site.

3. Immediate construction of at least one new telescope 90 in. or larger at a reasonably dark site with good seeing. The cost would be \$3 million to \$5 million plus another \$1.5 million to \$2 million if site development is needed.

4. Site surveys, particularly in the northern hemisphere, for new locations with first-class seeing, long periods of clear weather, and night skies that are likely to remain dark for several decades. Total cost of the surveys would be \$1 million.

5. Experiments in methods of fringe detection suitable for a Michelson interferometer and design studies of methods for tracking stars and equalization of the interferometer arms. Funding up to \$0.2 million would be appropriate for these preliminary investigations.

B. SECOND-PRIORITY ITEMS

We list here the remaining major facilities that deserve funding as money becomes available after the completion of the first-priority items.

6. Construction of a working Michelson interferometer, if the preliminary studies show that it is feasible and competitive with the proposed large-intensity interferometer (see Item D below). No cost estimate for an operating Michelson can be given until the studies are complete.

7. Construction of a large Schmidt telescope in the southern hemisphere at a cost of \$4 million.

8. Construction of three additional reflectors of 90-in. diameter or larger in dark-sky sites with good seeing, since it is clear that the single new instrument given highest priority will not fully satisfy the needs described above. Each telescope would cost \$3 million to \$5 million plus another \$1.5 million or \$2 million for each site requiring development.

C. CONTINUING DEVELOPMENTS AND SMALLER PROJECTS

In this list we include several smaller items that need not compete with the main capital investment, so that gradual funding should be possible.

9. Support of a laboratory for ruling better diffraction gratings and for developing new manufacturing techniques. A total expenditure of \$3 million would be appropriate.

10. Support of investigations to improve the photographic process up to \$0.5 million for the decade.

11. Construction of new astrometric instruments, including automatic transit circles, photographic zenith tubes, astrolabes, and automatic

measuring engines; the modernization of some astrometric refractors along with the installation of automatic guiding cameras on others; and the development of new techniques for direct photoelectric measurement of relative star positions. The total cost would be about \$6.4 million.

D. INTERNATIONAL COOPERATION

Under this last heading we list the large-intensity interferometer with a total aperture of 160 m^2 and effective baselines up to 1800 m now being studied by the Australians. We *recommend* that the United States Government explore with the Australian Government the possibility of joint funding in the construction of this unique instrument. The total cost to both countries would be approximately \$4 million.

CHAPTER THREE

Infrared Astronomy

PANEL MEMBERS

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I. INTRODUCTION

Within the past decade, infrared astronomy, long a technically difficult field pursued by only a few hardy pioneers, has become an important contributor to the mainstream of astronomy and offers great promise for future development. It now appears that many common objects, as well as some exotic ones, emit strongly in the infrared, and indeed certain unusual objects may prove understandable only through understanding the physical origin of their infrared radiation.

The infrared range of the electromagnetic spectrum begins at the long-wavelength end of the visible spectrum at a wavelength of about 1 μm and spans a range of more than ten octaves to about 1 mm, where it overlaps the short-wavelength end of the radio spectrum. Within this range, a significant part of which is accessible to ground-based instruments, lies a wealth of spectral information barely touched until now. For example:

1. The characteristic blackbody radiation of the moon, the planets, and several kinds of very cool galactic objects lies mainly in this range, as does the cosmic background radiation of the expanding universe.
2. Many objects, notably certain unusual types of galaxies and quasi-stellar sources, emit strong, nonthermal continuum radiation in the infrared.
3. The vibrational and rotational line spectra of cosmically important molecules fall in this wavelength region.

Thus the infrared is replete with opportunity for significant photometric and spectrometric measurements. New techniques and instruments now being used, and others yet to be invented, will surely produce an accelerating flow of important new results.

Infrared observations are not without their difficulties. The earth, its atmosphere, the telescope and its optical parts, the detector—all are significant and troublesome sources of infrared radiation, particularly in the 8–13-μm wavelength range where a 300 K blackbody has its spectral maximum. To avoid or minimize the effects of such radiation, all modern infrared detectors are cooled, sometimes to temperatures as low as 2 K; the field of view of the detector is matched to that of the telescope optical train by apertures, field lenses, and baffles, also cooled; uncooled structures within the field of view, such as support spiders, are eliminated if at all possible. Even with such precautions, the radiation emitted by the optical surfaces and by the sky seriously limits infrared measurements from uncooled, ground-based telescopes. Effects of the earth's atmosphere, which arise mostly from its water-vapor content, are minimized by choosing as a base of operation a high, dry site such as a suitable mountain, an airplane flying above the tropopause, a high-altitude balloon, or a sounding rocket. The ultimate in sensitivity will be attained only by using a cooled detector *and telescope* situated above the earth's atmosphere in a spacecraft. However, one need not wait for the day when such an instrument will be available, for many significant observations can very well be made from the ground and from other platforms less expensive than spacecraft, such as aircraft, balloons, and rockets.

The list of significant results in infrared astronomy is already long and growing rapidly. In Section II of this chapter, the history, current status, and future prospects of infrared astronomy are discussed.

Sections III and IV discuss the current status of infrared telescopes, technology and techniques, and the developments needed to reach the goals set in Section II. It should be remembered, however, that the most exciting developments are likely to be those that we do not foresee. Section V therefore describes a recommended program of systematic surveys of the sky designed to pick up unusual and unexpected objects as well as to gather statistics on those sources that we already know to exist. Section VI summarizes our recommendations for a program of development over the next decade.

II. INFRARED ASTRONOMY–SCIENCE

A. THE SUN

Infrared astronomy might be said to have begun with Sir William Herschel's discovery 170 years ago that a thermometer was heated by solar radiation lying beyond the red limit of the prismatic spectrum. Infrared measurements made decades ago entered into the determination of the solar constant, a quantity of fundamental importance for stellar astronomy, as well as for the study of solar-terrestrial relations. It is only in recent years, however, that infrared research has started to assume a major role in the study of the sun itself. This is because of the recent development of sensitive detectors, the appearance of a new generation of spectrometers (both grating and multiplex types), and the use of high-altitude aircraft to obtain observations from above the absorbing atmosphere.

One of the advantages of the infrared for solar research results from the large variations of continuous opacity with wavelength. At 1.65 μm, the photosphere is more transparent than anywhere else in the spectrum, allowing study of thermal fluctuations in the granulation and supergranulation at deeper levels than is possible at visible wavelengths. At longer wavelengths, the opacity increases as λ^2, so that beyond 300 μm most of the radiation comes from the low chromosphere above the temperature minimum. Accurate radiometry in the infrared is probably the most direct way to determine the temperature structure of the low chromosphere.

The vibration–rotation bands of most diatomic molecules lie in the near infrared (1–20 μm). They are relatively easy to interpret, for local thermodynamic equilibrium is generally a better assumption than it is for atomic lines, and the excitation potentials and transition probabilities vary in a known and smooth fashion for the many lines within a band. Molecular bands are therefore valuable temperature and pressure indicators, useful both for the study of the upper photosphere and of the intrinsically cooler regions above sunspots. Conversely, the sun and sunspots are perhaps the best stable source of gas at elevated temperatures, and measurements of high excitation lines in the solar spectrum can provide valuable information for theoretical studies of molecular structure.

Many elemental abundances are best determined from molecular lines. For example, fluorine is best seen in the form of HF, and the C/O ratio may best be determined by measuring the strengths of CH and OH lines in sunspot spectra. Isotope shifts are much larger for vibration–rotation bands of molecules than for atomic lines, so that determination of isotope ratios (such as the $^{13}C/^{12}C$ ratio from CO) is relatively easy in the infrared, as it is in the radio region. Zeeman splitting of atomic lines depends on λ, and therefore the most sensitive magnetic lines lie in the infrared. Development of an infrared magnetograph based on lines such as the λ22083 line of Na can in principle yield sensitive determinations of the fields in granulation, supergranulation, and plages. For the chromosphere, concurrent observations of magnetic splitting and intensity in the λ10830 line of He I should yield important relationships between field strength and heating mechanisms. Measurement of magnetically sensitive lines may also be important in testing models of flare activity. Coronal magnetic fields can be uniquely studied by means of the resonance polarization effect in the λ10747 line of Fe XIII, the strongest coronal emission line thus far observed in the visible or infrared spectrum.

In contrast to the rest of infrared astronomy, solar infrared research is not limited by telescope size but more by the need for sophisticated auxiliary instrumentation. The one exception involves high angular resolution. Because of improved seeing in the infrared, resolution considerably better than a second of arc can be expected, but even 0.5-sec resolution at 10 μm requires an aperture of 160 in., more than twice as large as any existing solar telescope. New infrared telescopes of large aperture and resolution should therefore be designed as far as possible with the needs of solar work in mind.

High-altitude spectroscopy of the sun is necessary for complete mapping of the solar spectrum and for such problems as the determination of the temperature structure of the chromosphere, but because of the general desire for high angular resolution, ground-based observations have the higher potential for most other areas of solar physics.

B. THE SOLAR SYSTEM

Scientific objectives in solar-system studies are the understanding of the origin and evolution of the planetary system and of the nature and the role of comets and of the zodiacal dust. Additionally, the properties of our solar system, since it is the only such system that can be studied in detail, will be useful in the broader study of the origin and evolution of stars and planetary systems in general.

Analysis of infrared spectra offers a prolific source of information concerning the identification and abundance of molecular species and their environmental conditions, whether they be constituents of a planetary atmosphere, isolated radicals in space, or bound in surface rock or dust formations. Thermal measurements provide direct evidence on the heat balance of solid bodies in the solar system and of their atmospheres. Such information is essential as data for physical theories of the origin and evolution of the solar system. A large number of infrared studies need to be pursued, either as an extension of existing programs or as entirely new efforts.

In the realm of broadband ($\Delta\lambda/\lambda \geqslant 0.2$) measurements of thermal emission, measured brightness temperatures of the planets range from approximately 600 K for the bright side of Mercury to 55 K for Uranus; almost all the thermal radiation from bodies in the solar system falls in the infrared. Brightness temperature measurements of Jupiter and Saturn are of particular interest because measurements at 10 μm and 20 μm from the ground and over the range 2–100 μm from an airplane have shown that these planets are radiating considerably more energy than can be accounted for as coming from the sun, indicating the existence of internal energy sources.

Temperature measurements also provide information on surface structure. Thermal inertia of surface layers has been derived from observations of the dark-side temperature of Mercury, variation of temperature with longitude on Mars, and cooling of the moon during eclipse. The study of thermal anomalies on the dark side of the moon has shown the presence of regions with widely differing thermal inertia.

For those objects with extensive atmospheres, brightness temperature and limb darkening measurements have provided information on the structure of the atmosphere that are particularly valuable as aids in defining relevant model atmospheres.

Broadband thermal observations of zodiacal dust near the sun during a solar eclipse have possibly detected burn-out zones, within which the dust is vaporized. Further observations of this type would provide information on the composition of the zodiacal dust through its vaporization temperature. Thermal observation of the zodiacal dust away from the sun is hampered by the diffuse nature of the source and will require the use of cooled optics on rockets or satellites. However, such observations will yield valuable data on the spatial distribution and temperature of the dust, which may also prove very useful in the study of dust in the vicinity of other stars.

Future broadband thermal observations should extend observations of the types described above to more solar-system objects, extend the wavelength coverage (particularly for objects beyond Mars, whose thermal radiation is predominantly beyond 25 μm), and increase the spatial resolution of the measurements. The latter will require use of very large telescopes to keep the diffraction image small.

Infrared spectral measurements are extremely valuable for determining the composition and structure of atmospheres and the composition of surfaces and particles. Vibration–rotation overtone bands in the range 1–3 μm are generally stronger than similar bands at wavelengths less than 1 μm. Thus high-resolution ($\Delta\lambda/\lambda \sim 10^{-4}$) spectra in the near infrared are well suited for detection of minor atmospheric constituents. Reflection spectra of solids have characteristic features even when the surface is granulated, so medium-resolution spectroscopy is useful for determination of surface composition.

The study of the composition and structure of planetary atmospheres has advanced enormously by means of recently developed interferometric techniques. For example, the most reliable quantitative estimate of the amount of water vapor in the Venus atmosphere was obtained with an interferometric spectrometer carried in a high-altitude jet rather than by direct sampling of the Venus atmosphere by the Venera spacecraft. The superb planetary spectra recorded by the Conneses provided a wealth of information on the isotopic and trace-element constituents of some planetary atmospheres and also upper limits to the abundance of certain key molecules whose presence or absence is significant to the origin and evolution of the planets and their atmospheres. The presence of deuterium in the outer planets is important for theories of nucleosynthesis. A particularly intriguing question is the relation of atmospheres to the existence of life forms. While space missions may actively seek life on planets close to the earth, infrared spectroscopy is a tool by which organic and prebiological molecules might be detected on the more distant planets. Further efforts in obtaining high-resolution infrared spectra from a superior site is thus an immediate, technically feasible goal of planetary spectroscopy.

Spatial resolution in planetary spectroscopy is an important goal of the future. For example, the intriguing cloud patterns on Jupiter suggest the presence of unusual hydrodynamic phenomena. Spectroscopic observations of the molecular species within these markings should lead to a better understanding of the underlying atmospheric physics. Compositional differences in the cloud formations may permit observations deeper into the cloud layers.

Temporal studies of planetary spectra are important for the study of transport mechanisms in planetary atmospheres. The well-known variation of the CO_2 content of the Venus atmosphere and the migration of the Martian polar caps are among the problems that should be investigated spectroscopically. Rare planetary orbiter opportunities must be supplemented by observations from the earth.

Infrared studies of comets may provide further clues concerning the origin and evolution of the solar system. The rarity of bright comets and their unpredictable appearance makes cometary studies very difficult. Only within the last two decades have high-resolution spectra of even the optical region been obtained. Almost no observational material exists in the infrared, although this region should prove highly rewarding because here exist the vibration–rotation spectra of such key molecules as CO^+, CO, NH_3, CO_2, H_2O, CH, CH^+, CH_2, CH_3, OH, OH^+, CN, CN^+, NO, NH, N_2O, HCN, C_2H_2, and CH_4.

The temperature structure of a planetary atmosphere can be determined by measurements made within an absorption band of a major atmospheric constituent. For example, the 15-μm CO_2 band could be used to determine the temperature profile in the atmospheres of Mars and Venus, and the 7.7-μm band of CH_4 could be used similarly for the atmospheres of Jupiter and Saturn. These measurements require observations from aircraft and balloons as these spectral regions are otherwise obscured by telluric absorptions.

Where solid particles are individually and collectively optically thin (i.e., zodiacal dust cloud, comets, most circumstellar dust clouds) medium-resolution spectral observations can yield important information. For example, observations of Comet Bennett (1969i) have shown that the solid particle component in this comet is similar in composition to the dust clouds surrounding some late-type stars and is probably mostly silicates.

Infrared techniques should be considered for the study of such interesting phenomena as the airglow, for the high infrared activity of the earth's upper atmosphere is known to be associated with geomagnetic disturbances. The effectiveness of infrared spectroscopy has already been demonstrated for such weak, extended sources.

C. GALACTIC STUDIES

Infrared studies relate to galactic astronomy—stars and interstellar matter and their aggregation and evolution as parts of the galaxy—in two distinct ways. Studies of the energy released by different objects are needed to understand evolutionary processes. Infrared observations of energy release are needed because thermal emission from cool objects between 4000 K and 3 K is concentrated in the infrared; a substantial group of objects have surface temperatures that fall in this range. Second, the interstellar medium is relatively transparent to the long wavelengths of infrared and the shorter radio wavelengths. Studies in these two spectral regions can probe through our entire galaxy. Further, in studies of galactic structure we find that among the bolometrically most luminous stars of both old and young populations are the cool supergiants radiating mainly in the infrared; such bright objects may be observed at wavelengths where the galaxy is transparent.

One of the first major achievements of infrared astronomy was the measurement of the bolometric luminosity of stars. This was used in the determination of the mass–luminosity relationship for stars and was an important ingredient in our present understanding of their internal structure and evolution. The study of bolometric luminosities is, however, far from complete. There is still almost no information available on the emission of normal stars in the 25–700-μm region.

Infrared studies of galactic structure have barely begun. Observations of stars and the galactic center are beginning to provide the factors needed to correct for interstellar absorption. A survey at 2.2 μm has provided a list of the bright cool supergiants in the northern hemisphere within a sphere of 2–3 kpc. The survey should be extended to the southern hemisphere, to longer wavelengths, and to much fainter limiting magnitudes. Some studies of individual stars have been made, but much more needs to be done. No information has yet been obtained on the distribution of interstellar dust clouds or cool dense gas clouds. Thus galactic structure studies belong mainly to the future.

Apart from general arguments for applying infrared studies, there are certain arguments relating to particular processes and materials. Studies of the mass balance between stars and interstellar matter, composition of interstellar solids, mechanisms of star formation, the origin of planetary systems, and the significance of nonthermal infrared processes are all topics of current research. They form a tightly interwoven mesh of topics in which the composition, formation, size, amount, and emission of solid particles play major roles. One of the exciting results of infrared surveys has been the discovery of a variety of types of circumstellar emission, often related to the formation of solid particles. Some are old stars with circumstellar blankets, others are prestellar clouds in regions where star formation appears active, still others are not yet understood.

These infrared studies appear destined to provide a fruitful attack on the problem of the origin of planetary systems. The astrochemical studies of molecules and condensates in space are necessary steps in understanding the solid bodies of the solar system. The infrared radiation from stars believed to be forming planetary systems (often accompanied by radio-frequency molecules and masers) provides important information on the distribution of particulate matter through these systems. The studies of interstellar solids show which solid materials are introduced into forming planetary systems, while the studies of dying stars show which materials may condense in the ejecta from different types of central stars.

The study of cool stellar atmospheres in the infrared provides some of the greatest challenges to those specializing in radiative transfer processes. The extraordinary variation in the opacity in the infrared produces stellar continua with bumps and dips quite unlike those in other spectral regions. Proper identification of the sources of opacity is a prime prerequisite to the construction of realistic model atmospheres for cool stars, which in turn are needed for understanding of the mass-loss problems.

The nonthermal processes of the infrared are currently being disentangled from thermal processes. The source at the center of the galaxy provides the closest view of the strange processes seen in other galaxies.

It is hoped that further examples will be found when the first sky surveys of faint objects are conducted in the far infrared.

Radio masers involve nonthermal processes that often seem to be associated with infrared continuum sources. Many of the quantitative details required to understand these objects must come from infrared studies, which can give information about the radiative and collisional environment where these spectral lines are produced.

In summary, the important studies of bolometric luminosities and galactic structure are ones where the infrared is known *a priori* to provide crucial information. Recent observations of a wide variety of galactic objects have, however, opened up new vistas that were not predicted. They involve solids and molecules at temperatures not far removed from those of our own environment and seem destined to provide a link between the earth, its origins and processes, and phenomena occurring thousands of light-years away. They also link processes occurring in our own galaxy to some of the most energetic phenomena in the universe.

D. EXTRAGALACTIC ASTRONOMY

In the past decade, many significant and surprising infrared observations of extragalactic objects and of the galactic center have opened up exciting new scientific vistas. Without doubt the great contribution of infrared observations to extragalactic astronomy has been the discovery that a large fraction of the energy emitted from the center of our galaxy and from the nuclei of some other galaxies appears as infrared radiation. Further, the total infrared luminosity in some active galaxies is extremely large; energy comparable to the rest mass of a galactic nucleus is transformed into infrared radition in $\sim 10^8$ yr.

Infrared observations of the galactic center have given us one of the best opportunities to study galactic nuclei. In 1966, the central 100 pc of the galaxy was mapped in the 2-μm region, and most of the radiation was explained in terms of a very large density of stars. In 1968, a strong source of 10-μm radiation 1 pc in diameter was found at the very center of the galaxy. That same year, balloon observations at 100 μm showed a large extended source of radiation approximately 400 $\times$ 1000 pc in size, with a luminosity $\sim 10^9$ solar luminosities or 3 percent of the total luminosity of the galaxy. Recent airplane observations at 100 μm show that a significant fraction of the radiation comes from discrete regions, one of which is coincident with the radio source Sgr A. The full significance of these results is far from understood.

Observations of extragalactic objects have been mainly in two areas: (a) nuclei of galaxies and (b) quasi-stellar sources. Observations of Seyfert galaxies, the exploding galaxy M82, and several other galaxies have been made out to 10 and 20 μm, resulting in the surprising discovery that most of the observed energy from those galaxies is radiated in the infrared.

In one of the Seyfert galaxies that has been observed from an airplane (NGC 1068), the luminosity between 50 and 300 μm is 10 to 100 times larger than the combined radiation observed in the radio, optical, and near-infrared regions. The energy distribution of NGC 1068 between 3 and 100 μm closely resembles that of the central region of our galaxy.

Measurements of the variability of some infrared sources indicate that the radiation comes from a region less than 1 light-year in size; the source of radiation cannot then be thermal. However, a very recent observation has shown that the region emitting 10-μm radiation in M82 is at least 100×400 pc in size. If all these observations are correct, then there must be a wide range in the physical size and surface brightness of these sources. Observations of quasars in the infrared have been mainly limited to the wavelength region between 1 and 4 μm. The energy distributions vary widely, but they are all quite unlike that of an ordinary star.

Variations in the brightness of some quasars at 2.2 and 3.5 μm have been established. The time scale and the magnitude of the variations are similar to the variations at visual wavelengths. Further, variations of a factor of 3 in one year's time appear to rule out a thermal source of radiation.

Although active extragalactic nuclei sources are also sources of optical, radio, and x-ray radiation, an understanding of these objects will require many more observations at infrared wavelengths, where most of the energy is radiated. The basic goals are to determine the mechanism that produces the infrared radiation, the energy source of the radiation, and the relationship between, and evolutionary histories of, various extragalactic objects. Some of the obvious observational programs leading toward this goal can be outlined briefly.

Surveys should be made to discover new sources of extragalactic infrared radiation, both unbiased surveys at all wavelengths and measurements of a larger sample of known extragalactic objects. These surveys will (1) increase the population of known sources from which interesting objects can be selected for detailed study; (2) help to better correlate infrared radiation with the known optical, radio, and high-energy properties of extragalactic sources; and (3) perhaps discover many new kinds of infrared source. The last is particularly important, for these sources could be fundamentally new objects in the universe, or they could be a missing portion of an evolutionary sequence of extragalactic objects.

More detailed studies of individual objects are also needed to determine the fundamental physical properties of extragalactic sources. Measurements of the energy distribution, especially in the region between 30 and 700 μm, are needed to give the shape of the emitted spectrum and the total luminosity of the sources.

The extragalactic sources should be monitored for variability at all infrared wavelengths, particularly those objects in which eruptions have been observed at millimeter and centimeter wavelengths. These measurements may provide clues to the size of the source and to the mechanism of radiation.

Observations of the spatial structure of the galactic center and nearby galaxies will also help to elucidate the problem of source size. Very large telescopes and interferometric techniques will be needed to obtain the highest possible resolution.

Finally, higher wavelength resolution studies should be made with the hope of detecting infrared emission or absorption lines in extragalactic objects. Although these measurements will require the ultimate in technical capabilities, they are vitally important, for the detection and measurement of absorption and emission lines has always been basic to the understanding of astronomical sources of radiation.

E. COSMOLOGY

The basic questions in cosmology are concerned with the nature, origin, and evolution of the universe. It is observed that the light from distant galaxies is shifted toward the infrared. Thus important conclusions regarding fundamental cosmology can be reached by measurement of the integrated brightness of the night sky at infrared wavelengths. As long ago as 1956, satellite experiments were proposed to perform such measurements, but it is only recently that they have become technically feasible.

The bulk of measurements made to date were stimulated by the discovery in 1965 of isotropic microwave background radiation which closely fits the spectral distribution of blackbody radiation at a temperature of 2.7 K. The most likely interpretation is that we are viewing the cosmologically red-shifted radiation field emitted during the initial phases of expansion of the universe. However, alternative mechanisms have been suggested, and until the shape of the spectral curve from 1 mm to 100 μm is measured, identification of the radiation with that predicted from the initial expansion will be in doubt. Indeed, recent measurements of the sky radiance in the submillimeter spectral region obtained with rocketborne and balloonborne infrared telescopes indicate that the spectrum of the isotropic background is not completely thermal. The current observations can be represented by a 3 K blackbody curve on which is superposed a strong line in the 800-μm to 1000-μm region, but further observations are needed to confirm this interpretation.

If the background radiation is indeed cosmological, its measurement can give us much information about the early history of the universe. For example, it may contain information on the more distant and hence younger galaxies, relating directly to their early evolution. The short-wavelength tail of the spectrum (75–200 μm) will also be affected markedly by the recombination processes that took place at the time of decoupling of matter and radiation in the early expansion. The details of this process, particularly the degree and nature of the irregularities remaining at its completion, are important for the understanding of galaxy formation. Thus measurements of the spatial structure as well as of the spectrum of the background radiation will be of great value.

Measurements of the large-scale isotropy of the background are necessary in order to separate the cosmological component from the emission from the earth's upper atmosphere, the zodiacal dust cloud, interstellar grains, and the background starlight of the galactic halo. Any remaining large-scale anistropy is of interest in that it may tell us the velocity of the earth with respect to the rest frame defined by the background radiation itself.

The mean density of the universe is a basic datum for all cosmological models. Determination of this datum requires cataloging the contents of the universe; our present information is limited to what is observable at optical and radio frequencies. Many types of new object could be discovered by surveys at infrared wavelengths. For example, the existence of highly evolved cool dead galaxies has been proposed; or much of the mass may be in the form of solids. These objects would have a high mass-to-light ratio and might contribute significantly to the mean density, but not to the galaxy counts. In addition, precise measurement of the background at all wavelengths is needed to define the equivalent mass density of the radiation.

Most of the problems discussed above require measurements at wavelengths inaccessible from the ground. Hence, contributions of infrared astronomy to cosmology will of necessity involve measurements from airborne and space platforms.

III. INFRARED TELESCOPES

A. GROUND-BASED TELESCOPES

At the present time, there exist in the United States two ground-based telescopes with apertures near 60-in. diameter that are used mainly for infrared work. In addition, there are about six telescopes in the 60- to 120-in. diameter range at which infrared observations account for between 5 and 25 percent of the observing time. Infrared observations on the 200-in. have been restricted to under 5 percent in the past but are growing with the use of twilight hours.

In order to carry out the future programs outlined in this report, more infrared observing time is needed. Expansion of the observing time on existing optical telescopes is not a satisfactory way of accomplishing this, for several reasons:

1. Existing telescopes are already fully subscribed for valuable optical programs.
2. Existing telescopes are not located at optimum sites for infrared observations.
3. Existing telescopes are not designed for minimum background radiation, and they cannot cope with the special techniques of beam switching required to handle the background problem.
4. Many telescopes do not have sufficiently accurate setting or guiding controls to enable observation of infrared objects with weak or no visible counterparts, particularly under conditions (moonlight or daytime) that are otherwise perfectly adequate for infrared work.
5. Since most optical telescopes are designed partially for direct photography, they are overdesigned and hence more expensive than necessary for photometric and spectroscopic work on infrared objects. The cost of the present 1.5-m infrared telescopes is about one third to one fourth as much as good optical telescopes of the same size.

The "light bucket" approach, a large aperture with poor image quality, is inadequate for infrared astronomy. For most infrared problems the limiting noise source is background noise, which varies as the product of the telescope aperture D and the angular size of the image θ. To make most efficient use of the telescope all radiation should be brought to a focus in an area smaller than the average seeing or diffraction disk. Thus to optimize the signal-to-noise ratio, large infrared telescopes must have good image quality. This requirement, coupled with the need for sophisticated acquisition and guidance devices, will eventually make very large infrared telescopes comparable in cost with optical telescopes of the same size.

In determining the size of new telescopes, consideration needs to be given to cost efficiency, scientific efficiency, and scientific need. These points are discussed in detail below, and the conclusion reached is that for some infrared problems large (3–5 m) telescopes are needed, while for other problems smaller telescopes (~1.5 m) are desirable.

Cost efficiency, that is, cost per measured photon, regardless of the usefulness of the photons, can be discussed by considering the time required to obtain an observation at a given flux level with a specified signal-to-noise ratio. This problem has been considered recently both by Johnson and Richards[1] and by Stein and Woolf.[2] Since the expense of a major telescope is primarily interest and depreciation, the cost per observation is proportional to the investment cost I times observation time t. For background-limited detectors, if the image size is determined by seeing, $t \propto D^{-2}$, whereas if diffraction limits the image size, $t \propto D^{-4}$. In the near infrared, seeing is the limiting factor, and the cost per observation will be less for larger telescopes only to the point where I starts increasing more rapidly than D^2. The cost figures of Johnson and Richards indicate that this point is reached at about $D = 1.5$ m.

However, for wavelengths of 6 μm and beyond, the diffraction image of a 1.5-m aperture is larger than 2 sec of arc (which we take as a conservative estimate of the seeing disk), and at 20 μm it does not reach this limit except for apertures larger than 5 m. For observations at these longer wavelengths, the cost per observation will continue to decrease with aperture to the point where the investment cost I starts rising more rapidly than D^4, a point that is probably not reached until well beyond the 5-m size. Thus there exists a good economic case for larger aperture infrared telescopes for use at long wavelengths.

The above discussion assumed that the source of noise is the background radiation from the telescope and sky. Two situations exist at present in which this is not the case, namely, high-resolution spectroscopy using narrow bandpass filters (e.g., $\Delta\lambda/\lambda < 0.01$ at 10 μm) and all observations at $\lambda < 3$ μm. In this situation, with appropriate optics, the smallest practical detector of 0.1 mm can be used on any size telescope smaller than 10 m. The noise will then be independent of aperture, the observation time for a given signal-to-noise ratio will be proportional to D^{-4}, and the cost per observation will decrease with aperture until I rises more rapidly than D^4, as for the diffraction-limited case discussed above. Improvements in detector sensitivities will reduce the range where this consideration applies, but we can expect that there will always be some resolution or spectral range where (regardless of the other considerations discussed below) telescopes of several-meter diameter will be more efficient that smaller ones.

In determining scientific efficiency and scientific need for a given size telescope, a major factor to be considered is the practical limit on the integration time that can be employed and the number of objects that can be observed in that time. Although in principle long integration times on small telescopes are equivalent to short integration times on large telescopes, there are several reasons why this equivalence is rarely realized in practice. Changes in atmospheric transmission and emissivity and systematic effects in instrumental systems combine to make observations difficult if they require more than a few hours. Similar difficulties stand in the way of combining observations from several small telescopes, and in addition there are serious problems of organization and of data handling. Thus, as has proved true in the history of optical astronomy, it is likely that most infrared observations undertaken will be those that can be accomplished in a few hours' integration time on a single telescope.

At the shorter infrared wavelengths, numerous objects can be detected with small-aperture instruments. (The Caltech Two-Micron Survey detected 20,000 objects with integration times of a few seconds using a 1.5-m telescope.) Photometric studies and low-resolution spectroscopy of stars at short wavelengths ($\lambda < 5$ μm) are easy with moderate-size telescopes. In fact, it would probably be less efficient with a large telescope, for then setting time would become an appreciable fraction of total observing time.

High-resolution spectroscopy of these same stars with small-aperture instruments is almost impossible. Even at 2 μm, with the best spectrometers operating with a moderate resolution of $\lambda/\Delta\lambda = 500$, observation times of about an hour are needed to obtain spectra of stars that can be measured photometrically in seconds. To observe more than the brightest of such objects, to obtain higher resolving power, or to extend the observations to longer wavelengths will require larger-aperture instruments.

The situation is also inadequate for making infrared measurements of known optical, radio, and x-ray sources such as galaxies, quasars, and pulsars. For example, it would have taken six full photometric nights on a 1.5-m telescope to reproduce the recent 2.2-μm measurement of the pulsar that was measured in one half-hour twilight session on the 200-in. telescope.[3] Even though some galaxies have been shown to be bright at infrared wavelengths, integrations longer than 1 h are needed on a 1.5-m telescope just to detect many of the sources.

Angular resolution is also an important question, particularly for understanding the nature of galactic nuclei, diffuse galactic sources, circumstellar clouds, and protostars. Some attempts are under way, and should be encouraged, to exploit to the fullest the capabilities of existing telescopes for angular size measurements. At the shorter infrared wavelengths, it should be possible to measure angular diameters well under 1 sec of arc if, as expected, atmospheric seeing is not so important as it is in the visible part of the spectrum. In the longest wavelength bands observable from the ground, however, large apertures are needed for even moderate resolution. As mentioned previously, a telescope of at least 5-m diameter is required to obtain resolution of 2 sec of arc at 20 μm. In the submillimeter region (e.g., 350–450 μm), where ground-based observations are again possible, a 20-m telescope would be needed to obtain resolution of even 10 sec of arc. Clearly, very large apertures are needed to obtain adequate resolution in this part of the infrared spectrum.

It is clear that ultra-high resolution in the infrared will not be possible with single-dish instruments. Recently, long-baseline interferometers operating in the microwave part of the spectrum have yielded the highest angular resolutions obtained on astronomical sources. Such interferometric techniques should also be developed and exploited in the infrared, for if they are successful, the shorter wavelength involved promises even a higher resolution than possible with a radio interferometer. It appears practical even now to construct an infrared interferometer operating at 10-μm wavelength that can give angular resolution of 0.01 sec of arc and be capable of detecting many of the brighter infrared sources. A particular instrument now being proposed uses heterodyne detection with a CO_2 laser as the local oscillator. The signals from two detectors mounted on different telescopes are combined at radio frequencies, with phase coherence being maintained by transmission of the laser radiation as a beam between the two telescopes. Development of such an instrument of modest aperture is recommended, not only because of the importance of the scientific measurements to be made but also as a means of discovering any possible limitation to very-long-baseline infrared interferometry due to atmospheric seeing or even to motions of the earth's crust.

In addition to very large instruments, construction of a large number of moderate-size telescopes is warranted. Infrared astronomy is a rapidly developing science, with new types of instrumentation being continuously invented and improved. It is difficult to carry on such developmental programs far away from one's home institution or in the short amount of time likely to be available on a large expensive instrument, particularly for those astronomers who must spend most of their time in teaching. Therefore, moderate-size telescopes easily available and near to university communities are needed for such development.

In summary, telescopes of 1.5-m diameter are both economic and capable of reaching large numbers of objects in reasonable times when used for photometry and low-resolution spectroscopy. Such instruments are also extremely valuable in developmental programs. For high-resolution spectroscopy, for work on many optical, radio, and x-ray sources that are faint in the infrared, and for work at the longer wavelengths, particularly when high angular resolution is desired, larger-aperture telescopes are both necessary for extending the limits of current observations and in many situations are economically justified. In addition, for the ultimate in angular resolution, interferometric instruments need to be developed.

In the light of the preceding considerations, we make the following recommendations:

1. The immediate construction of a 3- to 4-m infrared telescope on an existing northern hemisphere site. This telescope would probably be a scaled-up version of the existing 1.5-m infrared telescopes.
2. The immediate construction of a 1.5-m infrared telescope at Cerro Tololo, Chile.
3. The immediate construction of two additional 1.5-m infrared telescopes at northern hemisphere sites.
4. In the second half of the decade the construction of a 3- to 4-m telescope in the southern hemisphere.
5. The funding of 0.5- to 0.75-m telescopes for individual groups who show an interest in developing infrared instrumentation and who intend to continue infrared observations on larger telescopes.
6. Planning for a giant telescope ($\geqslant$10 m) used mainly for spectroscopy. This would include support of design studies and new design concepts, in particular that of the multielement array.

Recent measurements at Mauna Kea, Hawaii, indicate that some observations at submillimeter wavelengths (350 and 450 μm) are possible. If further measurements on that and other sites confirm this, then there are several reasons why a larger (5–10-m) telescope should be constructed for these wavelengths. First, diffraction will always determine the background independent of telescope size, so that from the arguments given above a large telescope will be more cost efficient. Second, to obtain high angular resolution at these wavelengths a large telescope is needed. The site of the telescope is of utmost importance. Surveys to determine the best possible site must be made.

The cost of a 5-m telescope for these wavelengths should be considerably less than that of a normal infrared telescope because both the image quality and the pointing accuracy can be an order of magnitude lower.

B. STRATOSPHERIC TELESCOPES

Observations in the wavelength interval 25 to 300 μm are not possible using ground-based telescopes, because of the large absorption in this spectral region by atmospheric water vapor. Fortunately, water vapor in the earth's atmosphere is strongly concentrated in the lower troposphere, so that observations throughout this spectral interval are possible from the stratosphere or above. Observations have been made of a dozen or more galactic sources over this entire wavelength interval using a 30-cm-diameter telescope mounted in a small jet aircraft operated at altitudes of from 12,000 to 15,000 m. Two extragalactic sources have also been detected. The planets Venus, Mars, Jupiter, and Saturn have been observed in several spectral bands. In addition to the aircraft work, a similar telescope has been operated at balloon altitudes and has yielded comparable results at 100 μm. There have been several attempts to make observations above the stratosphere using sounding rockets, but the results have so far been chiefly concerned with the absolute flux emitted by the sky. Until sources of known intensity have been observed, it is difficult to assess the performance of the rocketborne instruments.

In addition to the small far-infrared telescopes operated at low cost in small jet aircraft ($200/h) and in balloons, NASA has begun construction of a 91-cm high-precision telescope that will be operated at altitudes up to 15,000 m in a C-141 jet transport. This instrument will be of optical quality and have offset guidance accurate to at least a few seconds of arc. Extended observing periods will be possible, permitting long integration time on individual sources necessary for many spectroscopic applications. At 100 μm, diffraction will limit the resolution to about 30 sec of arc.

Although quantitative data are still lacking concerning the attenuation and emission of the atmosphere at these wavelengths as a function of altitude, latitude, and season, a few general outlines have been established. At 50 μm and at northern latitudes, there appears to be no measurable attenuation as a function of altitude from 13,700 to

15,000 m at 20° above the horizon. In the tropics, where the tropopause is at 16,800 m rather than at 12,000 m, the same experiment showed a small but strongly altitude-dependent attenuation. The implication of these results is that at arctic latitudes, altitudes of only 9000 m may be sufficient to obtain almost complete transparency in the far infrared. Obviously, systematic measurements of these properties of the earth's atmosphere are badly needed.

It is difficult to assess all the operational and economic factors concerning aircraft and free-floating balloons as platforms for stratospheric infrared telescopes. Undoubtedly, both types of vehicle will continue to be used. The airplane is certainly a noisier and inherently less stable platform than the balloon; however, the advantage of manned operation combined with versatility and reliability offset these disadvantages.

In comparing the results obtained by stratospheric instruments with the result from rocket flights, there is not yet sufficient data to determine what gain in performance is achievable because of the higher altitude. However, long observation times and repeatability strongly favor the stratospheric platforms. Economy also favors the stratospheric platforms, as does reliability. The principal advantage of rocketborne instruments is the extremely low background levels that are theoretically possible by cooling the entire telescope to liquid helium temperatures. This results in a high instantaneous signal-to-noise ratio. However, small cooled telescopes are also possible in the stratosphere, and until these instruments are tested their performance remains unknown. Thus, technological improvements in cooled telescope techniques and in ultra-low-background far-infrared detectors will determine the productivity of these various small telescopes.

We turn now to the scientific goals of the large far-infrared telescopes of the future. The most powerful discrete sources of electromagnetic radiation in the universe and in our own galaxy emit the bulk of their energy in the wavelength interval under discussion. Already we have learned something about the luminosities of these sources, as well as about their spectra, their sizes, and their locations with respect to optical and radio counterparts. However, we have been frustrated in our studies of many of these sources by the lack of angular resolution. For example, in the galactic nucleus we now know that the central source, when observed at 10 μm, is composed of many small sources less than a few seconds of arc in diameter. We know that this central cluster of sources is immersed in a volume filled with H II regions that are emitting intensely in the far infrared. In order to measure the far-infrared spectrum of the nucleus of our galaxy, we must have an angular resolution better than 30 sec of arc. In order to measure the brightest of the individual components in the galactic nucleus, we must have a resolution of 5 sec of arc. When we turn to the Orion nebula or to the Carina nebula, a similar situation exists. Powerful extended sources obscure or dominate one or more objects or regions of special importance. Turning to the extragalactic sources, the two galaxies that have been observed are NGC 1068 and M82. Observations at 10 μm have shown that NGC 1068 has an infrared nucleus that is extremely small, but that M82 has an extended nucleus. We must have sufficient angular resolution at 70 μm, near the peak of the spectral energy distribution, to measure these differences. The list of objects that must be studied at higher angular resolution will be enlarged. It should now be clear that high signal-to-noise ratio without sufficient angular resolution will not help in solving many of the principal problems that already face us in the far infrared. It is for this reason that we *strongly recommend* the development of a large diffraction-limited stratospheric telescope with angular resolution no worse than 10 sec of arc. Such an instrument, by virtue of its large aperture (3-m diameter) would attain extremely high sensitivity and would permit high spectral resolution, as well as high angular resolution.

The construction of a telescope meeting these scientific requirements would be a relatively simple, straightforward task, of modest cost, except for the problem of the stratospheric platform. The Infrared Panel has not investigated all the possible stratospheric platforms and their relative merits. Here, we list the principal suggested alternatives, describing them briefly.

1. Free-floating helium-filled balloons are fully capable of lifting a large instrument of this type to the required altitude of 15,000–24,400 m. Recovery problems, reliability, and cost must be investigated.
2. Large jet aircraft exist, which are capable of carrying a 3-m-diameter telescope; the main problems are structural re-engineering and cost. Given sufficient funds, there should be no fundamental problem, and reliability and observation time are high.
3. A glider, towed to altitude and released, might be considerably cheaper to construct than a powered aircraft of the same load capability and would have a better vibrational environment. Observation time would be less than in a powered aircraft, but the recovery problems would be considerably less than with a balloon.
4. As suggested by the Infrared Panel of the Astronomy Missions Board, it should be possible to develop a dirigible capable of operating in the stratosphere for long periods of time. This seems an attractive but as yet unexplored avenue.
5. Experiments with tethered balloons and kites have shown that

large payloads can be lifted to altitudes of 10,700 m or above. Here, there are a host of technological problems; but if they are soluble by straightforward means, this alternative would probably be the most economical.

In summary, present programs consisting of small 30-cm-diameter telescopes operated in aircraft and free-floating balloons will continue to yield valuable information in the spectral range that cannot be observed from the ground. The 91-cm-high precision telescope that will be flown in a C-141 jet transport by NASA will greatly augment work now in progress; however, for the future, we must have higher angular resolution and, hence, much larger apertures. This can be accomplished within the existing state of the art in the design and construction of the telescope itself, but it requires considerable study of the several stratospheric platforms that are possible. We would strongly urge that these studies be started as soon as possible, so that a working instrument could be available well before the end of the decade.

C. SATELLITE TELESCOPES

The previous sections indicate that much of the current need for infrared observation of individual objects can be met from ground-based and stratospheric platforms. However, the progress of science will inevitably lead to a desire for observations of fainter objects, in which case the environmental radiation will become the limiting factor. As discussed in Section V, the atmosphere sets the limiting sensitivity in the intermediate infrared, even with cooled telescopes, so that a large reduction in limiting flux level could be achieved with a cooled space telescope even now. In the far infrared, detector development has not yet reached the stage at which full advantage could be taken of the lowered background in space, but we anticipate that such improvements will be made in the near future. Thus we must look forward to the need for a space telescope, perhaps of the Orbiting Astronomical Observatory class (1-m diameter with fairly accurate pointing capability), designed for observation of infrared objects. There is little point in launching such an instrument if it cannot be cooled, so we would think it unwise to try to combine such a telescope with instrumentation for observing other parts of the spectrum. A pointed telescope in space, particularly one whose purpose is the extension of observations achieved from a stratospheric platform, should be designed to have a long lifetime. It will therefore likely require an active cooling system or the capability of replacement of cryogenics. Development of the technology for both these alternatives should therefore be initiated soon. The large space telescope (LST) is likely to have only limited, near-infrared capability. Cryogenic problems limit satellite observations but may be lessened in the space shuttle mode of observation.

Surveys of the sky for detection of extended sources are discussed in Section V, where it is pointed out that the expected flux levels are so low that few will likely be detected except from a space platform. Detailed mapping and spectral analysis of sources detected in these surveys will also require a pointed space telescope, but, since the surveys are not yet made, this will be a later development.

IV. TECHNIQUES AND INSTRUMENTATION

A. DETECTORS AND FILTERS

Great strides have been made in detector and filter technology in the last decade. In fact, the development of the germanium bolometer and improvements in photoconductive detectors, coupled with the development of interference filters for the infrared have been directly responsible for the rapid rise in infrared astronomy. There is, however, a clear need for further development in this area.

Considerable work is needed in order to provide filters that isolate wide or narrow spectral bands beyond 30 μm. Present-day filters in this spectral range consist of transmission filters using crystals imbedded in polyethylene and fine wire mesh and metal spot filters that act as long- and short-wavelength pass filters. It would be extremely valuable to infrared astronomy to have a wide variety of cut-on, cut-off, and narrow-band filters available across the entire 30–1000-μm range.

In the 3–30-μm spectral range, detectors are presently available that operate near the photon noise limit for some applications. There are, however, at least three areas where more sensitive detectors are required:

1. For all observations at wavelengths <3 μm, where the background thermal radiation becomes very small;
2. For $\lambda > 3$ μm, in high spectral resolution devices—in this case the background flux is reduced as a consequence of the narrow spectral bandpass received by the detector;
3. For use in the cooled optical systems of rockets and satellite instruments, where again the background is significantly reduced. Highly sensitive detectors are especially important, for these optical systems are relatively small and observing times are quite limited.

In addition to the development of broadband detectors for this spectral range, there is the possibility of developing intrinsically very narrow-band detectors such as the infrared up-converter. Such devices would be extremely valuable for numerous applications, including the study of interstellar molecular absorption features, stellar absorption and emission features, and fine-structure atomic emission lines in gaseous nebulae.

For wavelengths beyond 30 μm, there are few cases for which present detectors operate near the background photon noise limit. Hence a large developmental effort for detectors in this range should yield high scientific returns.

Several types of detector can be used in the far-infrared or submillimeter region, including germanium bolometers; doped-germanium photoconductors; gallium arsenide photoconductors; and Rollin, Putley, and Josephson detectors. The characteristics of currently available examples are summarized in Table 3.1. Germanium bolometers should be capable of much lower NEP's if made to operate at very low temperatures ($T <$ 0.1 K). The germanium photoconductors should be capable of achieving an NEP of 10^{-15} W or less. This has been done in the 10–25-μm band, and there is no reason to believe that it cannot be accomplished for the longer wavelengths.

In addition to these broadband detectors, there are several types of extremely narrow-band detector, including heterodyne detectors and ac Josephson junction devices. At this time, the most sensitive detector at 3 mm is a heterodyne detector, while the best performance at 1 mm is given by a germanium bolometer, but this situation can be expected to change as submillimeter heterodyne techniques are improved. There have already been several reports of submillimeter heterodyne detectors using a CN laser as the local oscillator and an InSb Rollin detector as the mixing element. These detectors have an NEP $\sim 10^{-17}$ W over a bandwidth of several hundred megahertz.

Narrow-band Josephson junction detectors have been constructed that yield NEP $\leqslant 10^{-17}$ W over a bandwidth of 300 MHz. However, these detectors have only been used at wavelengths of $\sim$ 2 mm. It is hoped that future developments will result in a tunable submillimeter detector with very high sensitivity.

There are theoretical reasons to believe that these narrow-band detectors can be improved by several orders of magnitude. They will be useful for studies of the submillimeter emission lines from gaseous nebulae and the rotational transitions of interstellar molecules.

B. SPECTROSCOPIC TECHNIQUES

Infrared spectroscopy is fundamentally more difficult to do than optical spectroscopy because of the inherent weakness of infrared sources and

TABLE 3.1 Characteristics of Currently Available Detectors

Type	Spectral Response	Size $(mm)^2$	NEP (W sec$^{1/2}$)	Time Constant (msec)	Operating Temperature (K)	Material
Germanium bolometer	Flat from 2 to 300 μm	½–10	2×10^{-14}	~15	0.8–1.8	Sb- or Ga-doped Ge
Germanium photoconductor	Increasing sensitivity from 2 μm to cutoff at ~120 μm	1–30	1×10^{-13}	~1	4.2	Ga-, In-, or B-doped Ge
Rollin	Submillimeter region to a few millimeters	2–50	1×10^{-13}	~0.1	1.5–4.2	InSb
Putley	Narrow band, 100 μm–1 mm, tunable with kG fields	2–50	10^{-10}–10^{-13}	~0.1	1.5–4.2	InSb
Josephson	Narrow band, millimeter region	1	1×10^{-14}	>0.01	4.2	Nb

the low sensitivity and high noise of infrared detectors. This situation is compounded by atmospheric transmission phenomena, thermal background radiation, and occasional hostile observing conditions for both the apparatus and personnel, all of which must ultimately influence the design of infrared instrumentation. The fine spectroscopic instruments developed over many decades for optical spectroscopy become exasperatingly unproductive for astronomy in the infrared for lack of detectors of suitable types or with sufficient sensitivities. The photographic plate is an obvious example of a detector whose exact analogue does not exist in the infrared. In fact, the photographic limit at $\sim 1.2\ \mu m$ is often taken as the point that divides infrared and optical spectroscopy. Optical spectroscopy also employs highly sensitive photoemissive devices which when properly operated can count individual photons and thereby approach the fundamental limitation to any observational technique. For all parts of the infrared, best current detectors have too much intrinsic noise to reach the photon counting regime. Even more important, most detectors have been operated in a radiation environment that would saturate any photon-counting device. Thus in some spectral regions detectors are limited by their intrinsic noise, and in other regions the detector performance is limited by the radiation environment.

Spectroscopy, as opposed to color photometry and the mere detection of astronomical objects, is concerned with the more or less high-resolution examination of the spectral detail in the incident radiation. The instruments are classified as either spectrometers or spectrographs according to the nature of the detector used, and they employ either dispersive or interferometric means for the physical analysis of the light. Many forms are possible, some of general utility and some very specialized, but all can be characterized by a few simple parameters such as resolving power, throughput, and number of spectral elements. The throughput expresses an instrument's ability to make most efficient use of the incident light flux, and the number of spectral elements indicates the total information capacity of a single record produced by the apparatus. The development of instruments that excel in these two characteristics is especially important since the maximum spectral content of infrared sources is sought in scans normally held to a few hours' duration. To these general characteristics should be added several practical considerations for infrared astronomy such as portability, the ability to operate reliably in hostile environments, and the effect of the radiation falling on the detector in the absence of a signal.

Instrumental development in recent years has taken several distinct directions; the most favored has been to improve detector technology to the point that some of the methods of classical optical spectroscopy can regain their effectiveness in the infrared. Any increase in detector sensitivity or decrease in noise level has the effect of extending spectroscopic techniques to increasingly weaker sources or to higher resolving power. Advances in optical and thermal engineering applied not only to the detectors but to the apparatus as a whole, telescope included, can also increase the number of objects subject to spectroscopic examination. The incentive for making such innovations obviously rests with those groups already active in infrared observations, for these astronomers sense keenly the substantial increase in information content that would accompany gain factors of 2 or 3 realized in components of the overall system performance. Such activity, in connection with active observing programs, should be recognized as a meaningful endeavor and encouraged by appropriate support where necessary. While even small gains would be welcome, the gains already achieved for some types of observation have been as high as 10^{10} in observing time.

A recent detector development, up-conversion, permits conversion of infrared photons to optical photons that can then be detected with the far more sensitive optical detectors. This device has special significance in that it can be tuned and so act simultaneously as a spectrometer. Although this technique is still in an experimental stage, development should be vigorously pursued and adequately supported to learn its capabilities and limitations for possible applications to infrared astronomy.

These attempts to bring infrared spectroscopy closer to the achievements of optical spectroscopy are directed toward making infrared spectroscopic equipment operate like optical equipment, which requires, among other things, circumventing background radiation problems. Progress along this line is apt to be a slow evolution that capitalizes on each improvement in detector technology. An approach that can yield the much desired increase by orders of magnitude in observing capabilities may require radically new instrument designs that effectively operate in high background radiation environments and make maximum use of current detector performance. The field of infrared multiplex spectroscopy has quickly and firmly established itself as a spectroscopic technique far superior to classical methods for many studies, particularly in the near infrared. As an example of the rewards, P. Connes demonstrates that a conventional grating spectrometer could have produced Venus spectra equivalent to those from his Fourier spectrometer (resolution limit 0.08 cm^{-1}, $S/N \approx 100$), but only by increasing the observing time by a factor of 10^{10}! Multiplexing occurs naturally within interferometric devices, although the techniques can also be incorporated with effectiveness into other forms of spectroscopic apparatus such as grating instruments. Innovations such as these cannot be fully predicted over the long term. Therefore, the Panel urges that support for infrared instrumentation be sufficiently flexible to accommodate the development of any promising technique.

For infrared observations currently requiring long integration times merely for detection, spectroscopy is a luxury that cannot yet be afforded. The possibility always exists, however, that some combination of larger telescope, improved or novel instrumentation, better detectors or superior site, including extraterrestial ones, will eventually allow access to the high information content of the infrared spectra of these sources through spectroscopic techniques. The exploitation of instrumental techniques by infrared astronomy in the immediate future should emphasize high-resolution studies of any object for which enough light flux can be collected and low-resolution studies of those weak objects for which no spectra yet exist. No single instrument can possibly serve the whole of infrared astronomy. The complete lack until recently of productive techniques for infrared spectroscopy requires that instrumental development proceed in several directions to provide astronomers with the choice of instruments necessary to exploit optimally a particular observing facility for a specific astronomical problem. This will also provide flexibility for adapting to an expected evolution in detector technology. Innovations in instrumentation will most likely arise from those groups actively seeking solutions to particular problems, and the value of such efforts should be recognized and supported.

The development of infrared spectrometers will affect advantageously other areas of spectroscopy that can equally well exploit these techniques. For example, some characteristics of Fourier spectrometers that are of secondary importance to infrared astronomy are of great interest for laboratory studies of atomic and molecular structure. These include the unlimited resolving power available with the Connes interferometer; the precision of the wavenumber scale automatically present in the spectrum; and the narrow, predictable, instrumental line profiles. Connes has predicted that the Fourier spectrometer will eventually replace all other types of scanning spectrometer for high-precision wavenumber measurements. Certainly, experimental evidence supports this claim. For equal resolving power, as measured by the half-width of the instrumental function, the experimental instrumental profile of a Fourier spectrometer is sharper than the theoretical profiles of grating and Fabry-Perot instruments. The highest precision obtained in near-infrared laboratory measurements of line positions with conventional spectrometers is 2.0×10^{-3} cm^{-1}; Connes's spectrometer has achieved rms errors of 1.4×10^{-4} cm^{-1}. Such performance is of interest to infrared astronomy. Several objects, including the sun and the bright planets, provide enough flux to utilize such performance. Also, laboratory absorption spectra are frequently used as aids in analyzing planetary spectra, and, as a rule, they must be recorded specifically for such comparisons, for lack of appropriate published data.

C. SITES FOR GROUND-BASED OBSERVATORIES

The problem of choosing a site for an infrared telescope is in some respects similar to the problem of locating a general-purpose telescope in that the following criteria must be considered in both cases: (a) cloud cover; (b) seeing; (c) ease of access, use, and maintainence; (d) geographic location; (e) cost; and (f) politics. A dark sky, quite essential for many optical problems, is not nearly so important for infrared work. A dark sky is helpful for locating and guiding on infrared sources with weak visible counterparts, but television guiders with extended red response, or direct infrared acquisition devices, will largely eliminate this problem in the future.

There are two special criteria for selecting a good infrared site, however, that make most optical observatories poor places to conduct infrared observations. These are (g) atmospheric water vapor and (h) sky noise. The importance of low water vapor is obvious for the conduct of observations through the infrared windows that are only partially transparent (5 and 20 μm) and near the edges of the other windows. Even in the relatively transparent regions at 2, 3.5, and 10 μm, however, there remain numerous weak lines that may not seriously affect broadband measurements but that add confusion to high-resolution spectra and make them difficult to interpret. The interferometric spectra of Connes suffer badly from this effect. If ground-based observations in the submillimeter region of the spectrum are to be done, then a *very* dry site is required.

The problem of sky noise—fluctuations in background emission above the photon statistics—is still not completely understood. Progress has been made in the design of modulation devices (notably use of a rocking secondary mirror) such that spurious noise introduced by the instrumental system and sky fluctuations can be greatly reduced. However, under certain conditions noise is observed above the photon noise of the background, which depends on the telescope size and site but which does not appear to be directly correlated with seeing, water vapor, or other meteorological phenomena. All studies of infrared sites should include measurements of infrared noise.

The general characterics that produce a site with low water vapor are cold temperatures, high latitude, and high elevation. These conditions are unfortunately in direct conflict with the desirability for a site with ease of access, comfortable working conditions, and low cost. A combination of facilities will probably produce the greatest scientific progress: (a) development of a few good sites (with water vapor of the order of 1 mm or less much of the year) of easy year-round access and possessing a variety of instrumentation; (b) development of one site with extremely low water vapor, accessible perhaps only a small fraction of the year and equipped with special-purpose instrumentation for obtaining those measurements not possible from the general-purpose site.

The search for good infrared sites should continue, but it is clear even now that three sites already under development for American astronomy are also reasonably good for infrared work. These are Mt. Lemmon in Arizona, Mauna Kea (and possibly Mt. Haleakela) in Hawaii, and Cerro Tololo in Chile. More statistics are needed about the variation in water vapor and sky noise over the year at these sites, which might influence the location of a very large instrument, but we believe that they have already proved good enough that their development with instrumentation designed especially for infrared astronomy would contribute greatly to the progress in this field, providing both good seasonal coverage and access to both the northern and southern skies.

This recommendation should not be taken as precluding construction of moderate-sized instruments on other sites. Several types of infrared astronomy can be done under only moderately good conditions. The advantages of ease of access, particularly when instrumental development programs are involved, may override the advantage in environmental conditions.

Finally, the scientific value of an ultra-low water-vapor site is unquestioned. At present, data are lacking to choose from among the various possibilities. In addition to mountain sites, the search for a truly exceptional infrared site must consider as special cases the South Pole region and airborne observatories. A preliminary reconnaissance of mountain sites has been made by Kuiper,[4,5] and a number of promising possibilities listed. A recent report of the National Academy of Sciences[6] points out several advantages of the South Pole region. The NASA high-altitude observing program with jet aircraft has already produced definitive scientific results that fully justify expansion of this program regardless of any other decision on high-altitude sites. The aircraft's mobility and the ultra-low water-vapor content achieved with this ultimate extension of a ground-based observatory are essential for many infrared observing programs. Nevertheless, a superior gound-based site will also be required. Even though the water-vapor content will be more than with the aircraft, such a site will accommodate short-term observing requirements that cannot be scheduled on an aircraft or extensive programs requiring more flight time than could be made available. It is likely that such a site will be provided with a larger-aperture instrument than could be airborne; the site would be used for any instrumentation too bulky or sensitive for airborne operation. To choose from among the possible ground sites requires a program to gather the relevant data on water vapor, seeing, sky noise, and construction and operating costs.

V. SKY SURVEYS

During the initial development of infrared astronomy, many interesting objects have been studied and discussed, but their discovery has come about largely by chance. Clearly an unbiased survey of the sky is necessary to determine the degree to which such objects are unique and to ascertain what other kinds of infrared sources exist. Furthermore, since the infrared spectrum encompasses over ten octaves in frequency, several surveys in different wavelength bands will be needed in order to delineate fully the character of the infrared sky. We feel that surveys of the sky are important for the new science of infrared astronomy and so devote this special section to their discussion.

A. THE INTERMEDIATE INFRARED

Sky surveys in the intermediate infrared region (1–50 μm) should be particularly rewarding for the galactic studies discussed in Section I. C of this chapter. For example, highly reddened supergiants can be detected best in the 3–6-μm region; a survey of their distribution may give us some knowledge of spiral structure on the far side of the galaxy. Also relevant to studies of galactic structure is the distribution of H_2 emission, both the 28-μm line and the higher-level lines that may be excited by interstellar shock waves. H II regions and planetary nebulae should be strong sources of atomic line emission such as that from ionized Ne and Mg. H I clouds may be excited to emit other lines, such as that of Fe^+ at 26 μm. Although cool dust clouds of the "dirty ice" variety would emit primarily in the far infrared, those of other compositions, particularly if heated by a nearby star, might show peak emission as short as 30 μm.

Aside from their importance for galactic studies, a sky survey in the intermediate infrared will tell us about the distribution of the interplanetary dust, and the location of burn-out zones will give information about its composition. The survey would also detect significant numbers of infrared galaxies; although such objects will emit more energy at 100 μm, the spectral distribution at shorter wavelengths is important for understanding the physical processes responsible for their emission.

Sky surveys in the intermediate infrared can be made from several different platforms: from the ground, from airplanes, from balloons, from rockets, and from spacecraft. Ground-based surveys are restricted to the natural atmospheric windows, seven narrow bands between 1 and 25 μm (see Table 3.2). Even within the windows, the sensitivity of a survey is limited by the sky brightness. Elevating a survey telescope above most of the atmosphere greatly reduces the sky brightness, as well as removes the restriction to atmospheric windows. Here the sensitivity is limited by the background radiation produced by the telescope optics, which can be cooled, but only to the frost point (200 K). In the space environment reached by rockets and satellites, the background can be further reduced, and the sensitivity increased, by cooling the telescope to cryogenic temperatures. The aim of a survey will determine the platform chosen for its conduct.

1. Ground-Based Surveys

Much of what we know today concerning infrared radiation from celestial objects has been obtained with ground-based instruments. The Caltech Two-Micron Survey was the first systematic survey of the northern sky at infrared wavelengths. The catalogue lists 5600 sources and is complete to magnitude $K = 3.0$. Analysis of these data should result in a significant contribution to our understanding of the structure of the galaxy. Although most of the sources detected prove to be ordinary cool

TABLE 3.2 Standard Spectral Bands for Ground-Based Photometry

Spectral Band	λ_{eff} (μm)	Flux for 0^m (W cm^{-2} μm^{-1})	Limiting Magnitude[a] (61-in. Telescope)
J	1.25	3.4×10^{-13}	–
H	1.60	1.28×10^{-13}	11.0
K	2.2	3.9×10^{-14}	10.0
L	3.4	8.1×10^{-15}	9.0
M	5.0	2.2×10^{-15}	7.5
N	10.2	1.23×10^{-16}	6.0
Q	22.0	6.0×10^{-18}	3.0

[a] For an integration time of 1 h and a 6-sec of arc diameter field of view. Included as a best estimate of the present state of the art with a pointed telescope.

stars, the survey also uncovered a class of objects with color temperatures of ~ 1000 K, some of which have proved to be interesting astrophysically. It is *recommended* that a high priority be given to the extension of the Two-Micron Survey to the southern hemisphere.

Surveys at longer wavelengths should detect cooler stars and perhaps new types of infrared objects of both galactic and extragalactic origin; they are more difficult to perform, however. At wavelengths beyond 5 μm, the detectivity of an infrared system is ultimately limited by photon noise of the sky background. (Even to reach this limit, great care must be taken to eliminate radiation from telescope structures and noise generated by poor modulation techniques. At some times and at some sites, true sky noise, i.e., changes in background level, may also be limiting.) Sky background can be reduced by limiting the field of view, but this is at odds with performing a survey efficiently–ideally the field should be chosen just small enough to avoid confusion of sources at the survey's limiting flux level. Ground-based surveys should be done but will be limited therefore to the first reconnaissance, at relatively high flux levels. Preliminary efforts to perform surveys at 5, 10, and 20 μm are indicative of the present state of the art. Using a 71-cm, *f*/16 telescope and a germanium bolometer subtending a solid angle of 4 min of arc, it is possible to survey the sky in less than a year to a limiting magnitude of $M = 1.5$, $N = -1$, or $Q = -3.5$.

2. *Airplane and Balloon Surveys*

Although use of an airborne telescope allows observation at wavelengths between the windows, the main advantage for survey work in the intermediate infrared is the reduction in the sky brightness. Table 3.3 summarizes recent measurements of this quantity. It is clear that raising the telescope to an aircraft operating altitude of 13,700 m will result in a marked reduction in sky background flux, but here another factor is also important. Table 3.4 shows that the radiance from the telescope optics will be higher than the sky background at temperatures above 200 K. This temperature is the frost point at 13,700 m and represents

TABLE 3.3 Radiance of the Sky at 10 μm

Altitude (m)	Radiance (W cm^{-2} sr^{-1} μm^{-1})
3,000	10^{-6}
13,700	10^{-7}
30,000	10^{-8}

TABLE 3.4 Mirror Radiance

Temperature (K)	Radiance (W cm^{-2} sr^{-1} μm^{-1})
300	1×10^{-5}
200	9×10^{-7}
77	1×10^{-11}

the limit to which airborne telescopes can be cooled, unless new techniques of preventing frost deposition on large apertures are developed. The frost point does not vary appreciably between 13,700 and 30,000 m, so no increase in sensitivity would be achieved by lifting the telescope to balloon altitudes.

An airborne or balloonborne 61-cm telescope cooled to 200 K using an array of detectors should be capable of a high-resolution survey (4 min of arc) of the upper hemisphere in about 8 h to magnitude $N = 0$. A balloon would be favored for such a survey, for it allows a large range of viewing angles, whereas aircraft structures generally restrict the viewing angles available at one time. A survey to $N = 3$ would require about 2000 h of observation. In this case, an aircraft would be preferred, for recovering and refurbishing balloon payloads require long intervals between flights. This latter type of survey is preferable scientifically, for it represents a major reduction in limiting flux level over that achievable from the ground. As discussed below, the economics may even make a rocket survey to this flux level competitive with one conducted from an airplane.

3. Rocket and Satellite Surveys

In the space environment, the frosting problem does not exist, and the background flux can be drastically reduced by cooling the telescope to cryogenic temperatures (see Table 3.3). If detectors designed for low background conditions are used, even the small telescopes carried by rockets can become competitive for survey purposes. One can show that using the best available detector and a 16-cm telescope, a survey of 25 percent of the sky to a level $N = 3$ can be achieved in one 300-sec rocket flight. Allowing for overlap and failures, about seven flights (at a cost of approximately $2 million) will be needed to cover the entire sky. This program is already under way.

The results of a survey to $N = 3$ can be readily anticipated. For normal stars, the N magnitude is not much different from K, so one would expect to pick up at least as many objects as in the Caltech Two-Micron

Survey (several thousand) and be able to determine, for example, the stars with circumstellar shells showing a 10-μm excess. For extragalactic objects, one can estimate that if all galaxies emitted as much radiation at 10 μm as does NGC 1068, there would be 8000 brighter than $N = 3$. This is, of course, not the case, but it indicates that the frequency of the infrared nucleus phenomenon can be well determined at this flux level.

The next significant step in reducing the limiting flux level in a survey, say to $N = 5$, would require large apertures or long observation times, neither of which could be achieved with rockets. A satellite is clearly indicated for this purpose. Such a telescope must also be cooled, and the development of space-qualified cryogenic cooling systems should be a long-range goal.

4. *Surveys for Diffuse Sources*

The above discussion has dealt with the problem of detecting sources of small angular extent, such as stars and galaxies. Many kinds of interesting objects are much larger than the typical 4-min of arc beam used to survey for point sources. Table 3.5 lists the predicted radiance for several such diffuse sources at 10 μm. Even the brightest of these is 10^{-7} as bright as the sky radiance at the ground and 10^{-5} of the radiance of a 200 K mirror. A sky survey for diffuse objects must therefore be conducted from a space platform.

B. THE FAR INFRARED

The far infrared, as defined here, encompasses the wavelength range 30–1000 μm. Mapping of the sky in three different wavelength bands, at 100, 350, and 1000 μm, is of interest for the following reasons:

1. Recent discoveries have shown that some galaxies are extremely bright at 100 μm;

TABLE 3.5 Sources of Diffuse Radiance

Source	Radiance (W cm^{-2} sr^{-1} μm^{-1})
Interplanetary dust	8×10^{-12}
Zodiacal light and stars	2×10^{-12}
Cosmic background	1×10^{-12}
Cosmic Ne^+ at 12.8 μm	1×10^{-11}

2. The emission from interstellar dust clouds is expected to peak near 350 μm;

3. The nature of the cosmic background can best be understood by a careful study of both the large- and small-scale anisotropy near 1000 μm (1 mm).

1. The 100-μm Survey

(a) A Survey for Discrete Sources The discovery that many galaxies, including our own, are strong infrared sources at 70 μm was one of the most unexpected results of far-infrared astronomy. There are indications that even our galaxy emits more radiation in a band centered at wavelengths near 100 μm than at all other wavelengths combined. To understand the source of this radiation, it is important for us to determine the extent of the infrared galaxy phenomenon. The atmospheric opacity in the 100-μm region is very high. Therefore it is essential that a 100-μm survey be made from high in the atmosphere, from an airplane, balloon, or sounding rocket.

As discussed in the previous section, the background radiation from an airborne or balloonborne telescope with a warm mirror is roughly equivalent to the radiation from a 200 K black source, corresponding to a brightness at 100 μm, for a bandwidth of 50 μm, of 10^{-4} W cm^{-2} sr^{-1}. Measurements at sounding-rocket altitudes indicate a maximum background of $\sim 10^{-10}$ W cm^{-2} sr^{-1}, less by a factor of 10^6. Since the integration time required to reach a given limiting magnitude is directly proportional to the background power, a rocketborne survey telescope is in principle 10^6 times faster than its warm-mirror counterpart carried by a balloon or airplane. Some preliminary studies indicate there exists a compromise between the all-cooled rocket telescope and the warm balloon or airplane instruments. This compromise would consist of a telescope with cooled optics and a warm low emissivity ($\epsilon \approx 1$ percent) window carried to balloon altitudes. By this technique, it is expected that an instrumental background of approximately 10^{-6} W cm^{-2} sr^{-1} could be achieved in practice. Since there is still residual emission from the atmosphere at balloon altitudes amounting to 10^{-6} W cm^{-2} sr^{-1}, there is no particular advantage in reducing the window emissivity below 1 percent (the value used above).

Table 3.6 lists the performance and operating characteristics of such a hybrid system as well as the warm-optics balloon instrument and the all-cooled rocket telescope. The relative observing time shown in line 4 is based on photon-noise-limited systems having the same geometry. The advantage of the low background at sounding rocket altitudes is

TABLE 3.6 Characteristics of 100-μm Survey Telescopes

	Warm Optics	Cold Optics, Warm Window	Cold Optics
1. Instrumental background (W cm^{-2} sr^{-1})	10^{-4}	10^{-6}	$<10^{-10}$
2. Atmospheric background (W cm^{-2} sr^{-1})	10^{-6} (~1% emissivity at 225 K)	10^{-6}	$<10^{-10}$
3. Total background (W cm^{-2} sr^{-1})	10^{-4}	2×10^{-6}	$<10^{-10}$
4. Relative observing time[a]	10^6	2×10^4	1
5. $(A\Omega)_{max}$ (NEP = 10^{-14} W/Hz$^{1/2}$)	3×10^{-4}	1.5×10^{-2}	2[b]
6. $A_{typical}$ (cm^2)	4×10^2	2×10^2	2×10^2
7. Time to cover 4π sr to 5×10^{-17} W/cm^2 once	4×10^6 sec	1.6×10^5 sec	1.3×10^3 sec
8. Number of flights required	130	5½	4½

[a] For identical geometries in the photon-noise-limited case.
[b] $A\Omega = 3 \times 10^2$ would be needed to match background and detector noise, but the largest practical $A\Omega$ without multiplexing is approximately 2 with present systems.

clearly shown. Line 5 gives the maximum throughput that can be used in each system consistent with a detector NEP of 10^{-14} W/Hz$^{1/2}$. Here we see that present detectors are unable to exploit the full advantage of the low backgrounds above 120 km. (Photoconductive detectors have already been made with NEP $< 10^{-15}$ W/Hz$^{1/2}$ at 20 μm. There is reason to believe 10^{-15} W/Hz$^{1/2}$ or better detectors will soon be available at 100 μm.) By using multiplex techniques to increase the usable area in the focal plane, one can offset some of the mismatch of present detectors and the background. Line 8 gives the number of flights required by the various platforms to reach a limiting flux level of 5×10^{-16} W/cm^2 (with an rms signal-to-rms noise ratio of 10:1). An NEP of 10^{-14} W/Hz$^{1/2}$ has been assumed, and no multiplex advantage has been put in for the rocketborne system.

On the basis of this analysis, we *recommend* that a preliminary sky survey be conducted soon using a balloonborne warm-mirror telescope. This survey would employ approximately ten flights and reach a limiting flux level of approximately 5×10^{-15} W/cm^2. We also propose a sounding-rocket or hybrid telescope survey to a deeper level for later in the decade. The choice between the two modes will be determined by the degree of technological development occurring during the next few years.

(b) A Survey for Extended Sources and Background Radiation The hybrid balloon telescope described in the previous section cannot be used to detect low-level background radiation or for mapping large extended sources of radiation. The balloonborne telescope must use beam-switching techniques to subtract out atmospheric background radiation. This same switching technique therefore also subtracts out the celestial background signal. This procedure also decreases the telescope's sensitivity to extended sources (e.g., zodiacal light, interstellar dust clouds).

To detect and measure extended sources, one must employ absolute chopping techniques and therefore a very low background environment. Measurements of this type require the use of sounding-rocket instruments. We therefore *recommend* the continuation of the present sounding-rocket programs directed toward the measurement of backgrounds and large extended sources. This program is distinct from the low-level sounding-rocket sky survey proposed for later in the decade.

2. *The 350-μm Survey*

The situation is somewhat different in the case of the 350-μm survey. Although the atmospheric water-vapor problem is less severe than at 100 μm, recent observations from Mauna Kea indicate that the 350-μm atmospheric window is still only about 25 percent transparent. Therefore, it is unlikely that a survey at 350 μm could be conducted from a mountain top.

One must also use a larger-aperture optical system to reach the same spatial resolution limit as compared to a 100-μm system (4-min of arc resolution at 350 μm requires a 0.5-m telescope). The increased aperture eliminates the possibility of using a sounding-rocket telescope for a detailed survey at 350 μm. It must be done using an airplaneborne instrument.

3. *The 1000-μm Survey*

In the case of a 1000-μm survey, the decision between a ground-based and airborne instrument is more difficult. Since the atmosphere is relatively transparent at 1 mm, it may be possible to use a ground-based telescope (antenna) to determine the small-scale isotropy of the background and detect continuum sources. The best available low water-vapor site is required for this purpose. However, a survey at even a nearby frequency (the 18.5 cm^{-1} waterline, for example) will require a high-altitude platform, most likely a balloon.

In a study of large-scale anisotropy, one must measure the absolute flux level, and therefore one cannot use differential chopping techniques. For this problem, high angular resolution is not required, and cooled sounding-rocket telescopes are adequate.

C. SATELLITE SURVEYS–A DEVELOPMENT PROGRAM

The short observing time available with sounding rockets, balloons, and airplane telescopes severely limits the capability of such systems. This short time will preclude spectroscopic studies of many sources or surveys to faint flux levels. It is therefore important to recognize that a satellite will be essential for many infrared observations.

Figure 3.1 gives the outline of a plan to design and construct an infrared Small Astronomical Satellite (SAS) during a two-year period. The preliminary design stages, Steps 1 and 2, must be followed by sounding-rocket studies to develop techniques for coping with the atmospheric airglow emissions and the radiation from particulate contamination carried by the payload. The problem of dust contamination is particularly severe since a micron-sized dust particle 10 to 100 m from the telescope will register as a strong source. One possible solution is to use two radiometers separated by a meter or more to identify dust particles by their parallax. It is important to recognize that this developmental sounding-rocket program is associated with the design and construction of a satellite and may not result in direct astronomical observations. It must be considered apart from the purely astronomical sounding-rocket programs such as the 100-μm survey.

Step 3b provides for studies aimed at providing the longest useful lifetime for the satellite that is consistant with the SAS system. This would include studies of various passive and active cooling schemes.

After completion of steps 3a and 3b, it may be necessary to make major changes in the original design. If so, further testing via rockets would be warranted. The fine details of the package would be worked out in Step 4. Steps 5–7 are self-explanatory.

By following this procedure, we believe that it would be possible to fly essentially a second-generation satellite on the first mission. Because a single SAS could not survey the entire sky in its relatively short lifetime, two packages would be required. We anticipate that the SAS packages could be flown during the first half of the decade. The SAS would be followed by a larger satellite, which would be flown near the end of the decade. This larger instrument would be designed to make

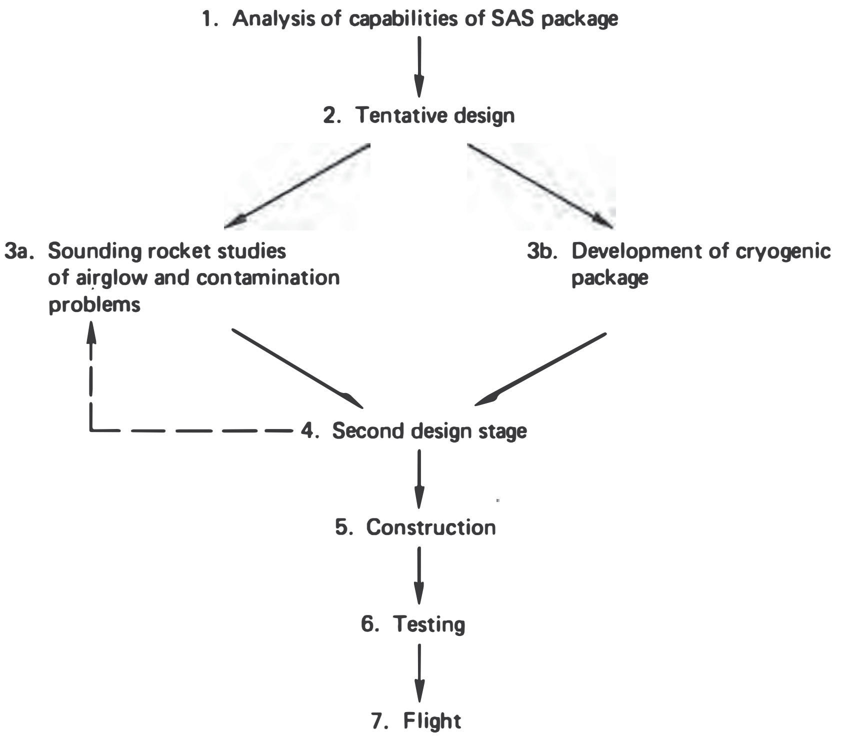

FIGURE 3.1 Design stages leading to the construction of a Small Astronomical Satellite. Steps 1 through 5 are to be completed in two years.

detailed measurements of specific objects. These would include studies of both spectral characteristics and variability.

VI. SUMMARY AND RECOMMENDATIONS

Infrared astronomy is a subject that touches on almost all interesting astrophysical questions. Some spectral regions, particularly the far infrared, have just begun to be explored. The relevant questions may not have as yet been asked, and programs involving surveys and reconnaissance efforts are required. Other areas, such as stellar and planetary studies, contain well-defined problems that need for their solution the development of sophisticated instrumentation and its application at good observing sites. Some measurements can be carried out at ground level, if enough observing time at good sites is made available. Other problems require airborne or space platforms. Thus a program for the development of infrared astronomy must involve a variety of approaches.

In the near infrared, we *recommend* completion of the 2-μm survey by extension to the southern hemisphere, a reconnaissance survey in the 5-, 10-, and 20-μm windows from the ground, to be followed by a deeper survey at these bands from rocket or airplane altitudes. Some of these programs are already under way and should be carried to completion as soon as possible, for they lay the groundwork for an orderly development of more detailed studies of individual objects. Farther in the future, we see a need for a satellite survey reaching to lower flux levels.

Detailed studies of individual objects require new telescopes, designed and devoted to infrared observation, at good sites. In the coming decade, we see a need for a large (3–4-m) infrared telescope in each hemisphere, supplemented by several moderate size (1.5-m) telescopes, with at least one located in the southern hemisphere. Planning should be started now for a giant telescope (>10 m), and new concepts, such as a multielement array, should be experimented with on smaller scales. Interferometric instruments for obtaining high angular resolution are also needed.

The demands of infrared astronomy are different from optical astronomy, and the search for sites with low water vapor and low sky noise should continue. In particular, the characteristics of the south polar region should be examined. However, the construction of new instruments at already developed good sites will have an immediate impact on the development of the science and should not be delayed until the ultimate site is found.

Telescopes are useless without good instrumentation, and the continued development of infrared technology should be encouraged. A greater variety of filters is needed, particularly in the 30–1000-μm range. Greater sensitivity detectors are also needed, particularly in the far infrared beyond 30 μm. Even at shorter wavelengths, where broadband detectors are background limited on ground-based telescopes, detectors are a limiting factor in high-resolution devices and the cooled systems of rocket and satellite instruments. Development of sensitive narrowband detectors looks promising and should be given strong support, for we foresee many problems arising in the future involving the study of individual emission or absorption lines. Other problems involve high-resolution spectroscopy over broad wavelength ranges, and here the already existing techniques of multiplex spectroscopy should be exploited to the fullest.

Almost all of astronomy in the far infrared has to be done from off the ground. One of the most exciting problems in the field is the study of infrared galaxies. We, therefore, feel an urgent need for a preliminary balloon survey of the sky at 100 μm to discover the extent of this phenomenon, to be followed later in the decade by a cooled telescope survey to a deeper level. Studies of known sources are also needed, and we *recommend* that strong support be given to current high-altitude airplane programs and to the development of a large stratospheric telescope. We also *recommend* reconnaissance surveys at other wavelengths, from an airplane in the case of a 350-μm survey and from the ground at 1000 μm. The rocket programs aimed at measurement of the cosmic background and detection of diffuse sources should be continued. Detailed work at long wavelengths, as well as deep-sky surveys, will require satellite instruments, and we *recommend* an immediate start on a satellite development program, using rockets to work out some of the more difficult technological problems.

Although the Infrared Panel believes that all the programs and instruments discussed in this report are important and will produce good scientific results, some ordering of priorities, mainly with regard to timing and cost, can be made. We have listed our recommendations in three categories below, but within each category the relative ordering should not be taken as significant.

A. HIGHEST PRIORITY

1. Expansion of Current Support Level Present work in infrared astronomy is inadequately funded and lacking in manpower. The Panel believes that a large return of astronomical results could be achieved by expansion of many programs discussed in this report, particularly in the following areas: (a) detector and filter development; (b) angular size measurements; (c) exploitation of multiplex spectroscopic techniques; (d) construction of more moderate-sized infrared telescopes, particularly in the southern hemisphere; (e) support of airborne, balloon, and rocket programs; and (f) study of promising sites for infrared telescopes.

Increase in support level required: $2 million/year

2. Large Stratospheric Telescope Design An instrument of at least 10-sec of arc resolution at 100 μm will be needed before the end of the decade (Section III.B), and the Panel therefore *recommends* that a design study be initiated now to determine the most suitable and economic platform.

Cost: $1 million

3. Preliminary 100-μm Sky-Mapping Program This shallow survey

(Section V.B), requiring approximately 10 balloon flights, could be carried out almost immediately.

Cost: $200,000

4. Large Ground-Based Telescope A 3–4-m aperture instrument (Section III.A), built especially for infrared work, could be utilized immediately for many applications and does not require development of new technology. A northern-hemisphere instrument is desired first, since sky surveys and studies of bright objects are more complete here.

Cost: $5 million

B. SECOND PRIORITY

5. Multielement Telescope Very-large-aperture instruments needed later on (Section III.A) will require new departures in design. A modest instrument can be constructed now that will be useful in its own right and test the concept of a multielement array.

Cost: $3 million

6. Ultra-High-Resolution Angular Interferometer Angular size measurements are important (Section III.A), and high resolution with single dishes would require very large apertures. The development of interferometric instruments is now possible and could start immediately.

Cost: $3 million

7. Large Ground-Based Telescope (Southern Hemisphere) A 3–4-m-aperture telescope will also be needed in the southern hemisphere (Section III.A) but could come after development of one in the north.

Cost: $5 million

C. THIRD PRIORITY

8. Large Submillimeter Telescope Important problems in background measurements and molecular astronomy require a large aperture (30-ft) instrument in this wavelength range at a good site (Section III.A).

Cost: $1 million to $2 million

9. Extensive 100-μm Sky Survey A survey to a deeper level than Recommendation 3, using either a rocket or hybrid telescope (Section V.B) requires some technological development and could come later in the decade.

Cost: $2 million

10. Small Astronomical Satellite A survey from such a satellite would extend the current rocket surveys in the intermediate infrared to a deeper level (Section V.A) but requires considerable technological development, which should be started soon (Section V.C).

Cost: $20 million

11. Large Stratospheric Telescope Construction of this instrument, using the platform determined by the study in Recommendation 2, should be implemented toward the end of the decade.

Cost: $5 million to $40 million

12. Giant Ground-Based Telescope The eventual need for a very-large-aperture instrument is realized even now (Section III.A), but design concepts must be worked out with preliminary efforts such as in Recommendation 5.

Cost: $50 million

13. Cooled Satellite Telescope–OAO Class This instrument will be needed in time (Section III.C), but technological problems are severe, cost is high, and much can be done from a stratospheric platform first.

Cost: $100 million

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CHAPTER FOUR

Space Astronomy

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I. SUMMARY OF RECOMMENDATIONS

1. At a time of diminishing funding for space astronomy projects, it is more than ever important to support experiments that can be done using less expensive vehicles. Thus the Panel *recommends* prompt increase of support for aircraft, balloons, and rockets to double the present budget within a period of three to five years.

2. For the near future, the Panel *recommends* that four areas of development be given the highest priority, and each one should be pursued as rapidly as technology permits:

(a) Development of heavy survey and pointable x-ray and gamma-ray instrumentation to be flown in earth orbit (these are major components of NASA's proposed High Energy Astronomical Observatories).

(b) Development of an infrared-sensitive space telescope with optics cooled by liquid helium, unless technical developments allow such a system to be operated in the high atmosphere. The initial task of this system would be to conduct a far-infrared sky survey.

(c) Development of a series of large, accurately pointable observatories and associated instrumentation. These observatories should be designed primarily to support high-resolution studies from 0.1 to 5 μm. However, their use to support observations in x rays and the infrared, as well as for planetary and possibly for solar studies, should be explored.

(d) Development of a deep-space probe to make a full range of particles-and-fields measurements out to a distance of the order of 30 A.U. The goals of this mission would be to study the low-energy galactic cosmic-ray flux essentially uncontaminated by solar modulation effects and to study the structure of the interplanetary medium out to large distances where it begins to merge with the interstellar medium. It should be possible to combine this with a mission to one or more of the outer planets.

3. It is essential that a balance be maintained in the research efforts in the different fields of space astronomy, for the studies in different fields complement one another in important and unique ways. The Panel *recommends* that the following programs should have high priority:

(a) In particles-and-fields astronomy, heavy cosmic-ray equipment in earth orbit and a continuation of the IMP series of spacecraft to measure fine details of the particles and fields in the interplanetary medium.

(b) In ultraviolet astronomy, a space observatory to follow the Orbiting Astronomical Observatory (OAO) series, possibly of an OAO-B or Small Astronomical Satellite (SAS) type. This is especially important due to the failure of OAO-B to achieve orbit and becomes critical if the very large observatory for ultraviolet studies should suffer a long delay.

(c) As a follow-on step to the second radio explorer for long-wavelength radio astronomy, a two-satellite radio interferometer.

(d) In solar physics, a continuation of the present Orbiting Solar Observatory (OSO) series through OSO-L, -M, and -N, with continued improvement of the time and space resolution capability of the observatories and instrumentation.

4. The present and above recommended programs in solar and radio astronomy should lead to the following highly desirable major projects:

(a) A heavy payload satellite with 1-sec of arc or better pointing capability for high temporal and spatial resolution solar studies.

(b) A 1-deg of arc survey telescope for long-wavelength radio astronomy.

(c) A satellite terminal in space for very-long-baseline interferometer measurements, which also utilize ground-based radio telescopes.

II. SPACE ASTRONOMY

During the past decade, our ability to make observations from rockets and spacecraft has revolutionized our understanding of the sun and the solar system. There has been an equally great impact upon the astronomy of the universe beyond the solar system.

For centuries astronomy had relied on observations in visible wavelengths. In recent decades, the opening up of the radio window has profoundly revolutionized astromony. The space program has opened up the ultraviolet, the infrared, the x-ray and gamma-ray regions, and the long-wavelength radio regions of the spectrum. Direct measurements of the interplanetary medium and of planetary environments have had a revolutionary impact on studies of solar-system astronomy. These new developments have not yet had time to come to maturity, but already they are playing a large role in observational programs in optical and radio astronomy as well as in the space program.

A number of recommendations have been made to the U.S. Government concerning the conduct of the space program in astronomy. These have included reports from the Space Science Board of the National

Academy of Sciences, as well as a lengthy position paper from the Astronomy Missions Board of NASA. The Space Astronomy Panel of the Astronomy Survey Committee has been constituted from scientists representing the various fields of space astronomy who have not been involved in these earlier studies. We have generally found ourselves in agreement with the conclusions that have been reached in previous studies. To the extent that we have differed in our recommendations, these reflect advances in the technology of space exploration and do not differ in any essential respect from previous general recommendations. At most they amount to differences of emphasis. Our technical study was largely completed by June 1971, and the march of events has affected its immediate relevance.

Our basic premise has been that space astronomy should be supported in a balanced way. There are some areas that seem very promising in which a large investment in equipment and effort is highly desirable. But we believe that it would be wrong if the investment in space astronomy were to be confined only to the most promising areas. Other, less-developed, areas may well contain some of the most important advances for the future. Furthermore, advances in any one area of astronomy have always proved to produce profound effects on studies in related areas of astronomy. Because of this, we believe that space astronomy requires strong ground-based support in order to exploit the discoveries that are made from rockets and spacecraft.

Our report opened with our principal recommendations. In this section, we discuss some of the reasoning that has led to these recommendations and the relative emphasis that we place on the different fields of space astronomy. The detailed technical discussions of space astronomy can be found in the specific sections devoted to them.

Space astronomy is expensive compared with ground-based astronomy because of the expense of launching vehicles and the technological development required to assure that spacecraft will operate reliably in the space environment. Astronomical measurements that can be made from the ground should be done in this way, because of the long-term continuity possible and the much smaller expense involved. Although all branches of space astronomy cannot be pursued vigorously at present because of budgetary restraints on the entire space program, measurements made with aircraft, balloons, and rockets can be made considerably more cheaply than those from spacecraft, even though the amount of information returned is greatly reduced. Often even a small amount of information in a new area of astronomy will maintain vigor in that field and produce fundamental discoveries. Therefore, because of the relatively low cost, we are convinced that the program of research by aircraft, balloons, and rockets should be expanded at a time of budgetary restraint, when major new starts in spacecraft programs are difficult. This is the basis on which we have recommended a doubling of expenditures in this program, mostly for rockets, to take place in the shortest possible time, which we estimate might be three years.

X-ray astronomy has been one of the most fruitful of the fields of space astronomy. It is closely linked to gamma-ray astronomy, which has recently started to produce interesting results. Until recently, all the discoveries of new x-ray sources and of the x-ray background had been made with rockets. More recently, important advances have occurred as a result of the measurement of the x-ray background from an Orbiting Solar Observatory and from more specialized measurements made by the first Small Astronomical Satellite. One of the most important early discoveries made with the x-ray satellite, *Uhuru,* was the fast irregular fluctuations, on a time scale of tens of milliseconds of the x-ray source Cygnus X-1. It is interesting that, because of limitations of the scanning mode of operation of the *Uhuru* satellite, studies of the fluctuations in this source can be carried out better from rockets than from this spacecraft. Nevertheless, *Uhuru* remains a superior instrument for locating new and interesting x-ray sources. It has discovered rhythmically pulsing, eclipsing x-ray sources, galaxies, and quasars.

At the time of the Space Astronomy Panel's deliberations, the High Energy Astronomical Observatories (HEAO) were a high priority item in NASA planning, and authorization for these had been requested from Congress; however, payloads had not yet been chosen. The Space Astronomy Panel recommended as a highest priority item the flying of heavy x-ray and gamma-ray equipment in space on HEAO.

It was evident that both survey and pointed modes of operation of such spacecraft would be required in order to carry out a comprehensive program. However, since the Panel did not possess detailed proposals for instrumentation in these satellites, we were not in a position to make detailed recommendations.

In the meantime, NASA has chosen payloads for the first two of the survey type of HEAO's. The payloads contain a balanced set of instrumentation for discovering and measuring the properties of new x-ray sources, for surveying the x-ray background, for attempting to detect gamma-ray lines and measuring the gamma-ray background, and for determining astrophysical properties of cosmic rays. These payloads can be expected to carry out very well the objectives for research in x-ray and gamma-ray astronomy discussed by the Panel, and, therefore, the Panel endorses the NASA proposals for the survey modes of operation of the HEAO.

NASA has not yet requested authorization from Congress for the pointable HEAO's. However, the Panel believes that the pointable spacecraft will, in the long run, be just as important, if not more important, than the survey spacecraft. Some information on x-ray sources can only be obtained through long integration times during which the spacecraft must be pointed at the sources. In addition, many of the x-ray sources exhibit short-term fluctuations in intensity, with multiple periodicities, which should be studied for a considerable time on a number of different occasions. For those x-ray sources that have been identified optically, a coordinated program of simultaneous space- and ground-based observations should be carried out. Both the survey and the pointable modes of operation of the HEAO remain among the highest priority items on the Space Astronomy Panel's list.

Infrared astronomy is another field that is proving extremely productive. Startling new discoveries have been made, both as a result of ground-based measurements through the relatively narrow infrared windows in the atmosphere and from aircraft and balloons flying above most of the absorbing water-vapor content of the atmosphere. It appears that much of the energy emitted from astronomical objects and from the universe itself probably resides in the infrared, and hence some of the most important astronomical discoveries will be made in this region.

Infrared measurements can be made from high in the atmosphere and in space. In the atmosphere, infrared optics cannot be cooled to liquid helium temperatures and, therefore, retain a high infrared background emission. Infrared measurements must be made by subtraction of the background by oscillating the optical system so that it alternatively measures a source and the nearby sky. Measurements would be made to much lower source emission levels, if the entire optics could be helium cooled so that background subtraction would no longer be needed.

It is important to make infrared measurements with liquid-helium-cooled optics, and techniques may be developed to prevent condensation of atmospheric constituents on optics (e.g., by suspending a very thin plastic film across the aperture of the system). This would introduce very little infrared emission into the optics even though the film itself might be at local ambient temperature. If these techniques can be developed, then the Panel *strongly favors* carrying out a high-sensitivity survey of the sky for new far-infrared sources, utilizing helium-cooled optics. If flying large helium-cooled optical systems within the atmosphere should prove impossible, then the Panel *recommends* that the infrared survey measurements be carried out in space. A series of relatively inexpensive infrared survey telescopes, cooled by liquid helium for periods of a month or more, would suffice to carry out such a program. The Panel recognizes that a great deal of developmental work would be required before such spacecraft could be flown. It *recommends* that some of this developmental work be carried out in the near future, utilizing rockets. These rocket developmental programs are in any case likely to produce results of great interest to astronomers.

Ultraviolet astronomy is another field of space astronomy that until a few years ago was carried out largely by rocket experiments. This wavelength region is not expected to produce as novel results as x-ray and infrared astronomy, because most types of object detected in the ultraviolet are expected to be known already from studies in the visible wavelength region. Nevertheless, much information that astronomical spectroscopists would like to have about specific objects is hidden in the ultraviolet region; hence ultraviolet astronomy is expected to interact intimately with ground-based observations in the visible.

Ultraviolet astronomy has received a series of very unfortunate setbacks, through loss of the first and third Orbiting Astronomical Observatories (OAO's). Only one more of these observatories is to be flown. The original set of three OAO's was chosen with complementary sets of instrumentation, and the loss of OAO-B will be keenly felt. With no further spacecraft authorized for launching to exploit the ultraviolet wavelength range, it appears that there will be a long period in which this field of space astronomy will stagnate.

We believe that it is particularly important to maintain a continuing program of ultraviolet observations from space. For this reason, if more ambitious spacecraft could not be planned for launching in the next few years, it would be important to launch a modest instrument to continue ultraviolet measurements in space. Such an instrument could be a Small Astronomical Satellite such as is now being considered for the International Ultraviolet Explorer (IUE).

The Panel gave its *highest priority recommendation* to the development of a series of large accurately pointable observatories for work in the ultraviolet. The major goal in such a program should be a Large Space Telescope (LST), with a diffraction-limited aperture of the order of 120 in. Such large and accurate instrumentation will eventually be needed to make space observations in the ultraviolet comparable with or greater in sensitivity to those now obtained with large ground-based telescopes in the visible. Such instruments would also be exceedingly important for use in the visible, because of the high angular resolution possible in space but impossible, due to atmospheric turbulence, from the ground. Thus the LST will have a much greater general utility than just for ultraviolet astronomy.

It may not be desirable to jump directly from the OAO to the LST.

So much technological development would be required that the Panel believes that a series of ultraviolet space telescopes should be launched, gradually building toward the LST. The first in such a series could have an aperture of about 60 in. Such an instrument would be an immense advance over the OAO and could be used to test a number of the developments required for the larger LST.

The spacecraft technology needed for the LST also might be useful for large telescopes in the x-ray and infrared regions and possibly for solar astronomy. Therefore the Panel has phrased its recommendation for the development of the large pointable spacecraft in more general terms, calling for the development of a general-purpose, accurately pointable carrier, which may contain instrumentation devoted to any of these wavelength regions, if common development is technologically advantageous.

One of the most important advantages of space technology for solar-system astronomy is the ability to make *in situ* measurements. Space probes to the planetary bodies can give dramatic new results. Our survey has excluded this realm of space astronomy from the scope of its deliberations. However, there is another type of *in situ* measurement that is of great astronomical interest: the interplanetary plasma. Throughout the past decade, a large number of spacecraft have studied particles and fields in the magnetosphere and in the interplanetary medium. Most of these measurements tend to be more of interest to aeronomy, geophysics, and physics than to space astronomy. The Panel considered particles-and-fields measurements that would be specifically astronomical in character, phenomena involved in stellar winds, generally, and in plasma in interstellar space. Many such properties cannot be observed anywhere except in the interplanetary medium of the solar system.

The Panel believes that the most important areas that should be pursued in particles-and-fields astronomy include the measurements of spatial and temporal correlations on a relatively local scale within the interplanetary plasma and the determination of the large-scale structure of the solar-wind region out to the heliosphere boundary. Another problem of great interest, although not given the same degree of priority as the others, would be the measurement of the properties of the solar wind at large distances from the plane of the ecliptic. The Panel gives its highest priority to measurements of the structure of the interplanetary plasma at large distances from the sun, several tens of astronomical units, where the magnetic field would be wound into a rather tight spiral, would probably become turbulent, and would connect in a largely unknown way with the interstellar medium proper. Such measurements would be of extremely high interest for plasma astrophysics and for studies of the properties of the interstellar medium.

The final wavelength region of interest to space astronomy is the long-wavelength radio spectrum. This wavelength region has been initially investigated by the first Radio Explorer Satellite. Interesting measurements have been made of radio noise from within the solar system and from the galaxy; however, precise measurements are hampered by lack of angular resolution.

The Panel endorses the plan to put a second Radio Explorer Satellite into orbit about the moon, in order to remove it from the sources of terrestrial background noise. A two-satellite interferometer system would be desirable to improve the angular resolution at long radio wavelengths. It was evident that a number of technological developments would be required in order to make such an interferometer system work, and the Panel gives high priority to a study of the necessary developments. Since the long-wavelength radio region should contain information about the properties of the plasma in our own and other galaxies, it is important to launch the two-satellite interferometer system if these studies should prove it to be feasible.

It is evident that for the more distant future, the development of long-wavelength radio astronomy will require a much larger instrument in space—a survey telescope capable of 1-deg of arc resolution. The problems facing the development of such an instrument are numerous, and the Panel does not believe that anything more than a design study of such an instrument is warranted at present.

A satellite terminal for a very-long-baseline interferometer (VLBI) would be a space project at conventional radio-astronomy wavelengths. VLBI systems already have reached across the earth; still higher resolution would require longer baselines, which can only be achieved by locating one terminal in space. This system would also require advanced technological development; there is an unresolved question as to whether the resolution attainable would be limited by interstellar scintillations. The Panel believes this system is sufficiently promising to justify a detailed design study, to determine the desirability and practicability of launching such a VLBI terminal.

The Panel considered various types of solar-system space astronomy that could be carried out from instruments in earth orbit. Foremost among these are studies of the sun. The sun is an interesting object in all wavelength regions. Because of the much greater flux of radiation from the sun than from other stars or galaxies, the instrumentation for study of the sun is very different from that required for other areas of astronomy. Furthermore, the problems of interest in solar physics tend to involve rapid time variations and small-scale spatial variations, which require high-resolution instruments with large collecting areas.

There has been an extensive study of the sun from space during the last decade. The body of knowledge built up means that solar studies are in a more mature stage than those in most other branches of space astronomy. The Panel believes that it is important to continue these studies in order to improve the temporal and spatial measurements of solar phenomena. For this reason, a high priority was given to the continuation, with growing capabilities, of the series of Orbiting Solar Observatory (OSO) spacecraft and to the concepts of the OSO-L, -M, and -N, as well as an eventual heavier payload satellite with 1 sec of arc or better pointing capability for solar studies.

The study of other objects in the solar system by space-astronomy techniques involves detection of fluxes of radiation comparable with those received from stars and galaxies. Therefore, the program of instrumentation in space astronomy outlined above would be beneficial for planetary studies, and we endorse such use of this instrumentation on a part-time basis. Ultraviolet and infrared measurements are particularly important in planetary studies. The Panel on Astrophysics and Relativity considered the optimum strategies for general relativity measurements from space.

The Panel believes the foregoing program of recommendations to be the optimum strategy for development of space astronomy in the next few years. It is a balanced program that is less ambitious than a number of other recent studies, because we recognize the stringency of the budgetary situation. In a field that is developing as rapidly as space astronomy, new developments and discoveries may change the optimum strategy in different areas, making it more urgent to carry out projects that at present are not yet recognized as most important. The optimum strategy for space astronomy should be reviewed in about five years; not all the recommendations made by the Panel can be carried out in the next five-year period. The Panel believes that for projects that are likely to be more than five years in the future, design studies should be carried out within the next five years, to guide future reviews.

III. X-RAY AND GAMMA-RAY ASTRONOMY

A. INTRODUCTION

The discovery of stellar x rays was made ten years ago. Although attempts to observe cosmic gamma rays have been carried out for a similar length of time, they have been detected only recently. Most of the important x-ray discoveries were first made with sounding rockets; some have been made with balloons (for the higher-energy x rays) and with satellites. In the last year, the x-ray satellite *Uhuru* has enormously broadened our knowledge of x-ray phenomena. There is a wide variety of x-ray sources in the sky and also an apparent isotropic background of x rays that may be of cosmological origin. The majority of galactic x-ray sources have not been identified with optical counterparts; however, the few identifications that have been made indicate a rich variety of phenomena in the x-ray sky.

The following types of x-ray objects have been identified with reasonable certainty:

(a) Peculiar blue stellar objects–the x-ray sources Sco X-1 and Cyg X-2. Massive binary systems with flickering or rhythmic x-ray companions.

(b) Transient x-ray sources–Cen XR-2 and Cen XR-4.

(c) Supernova remnants–both the Crab nebula and the pulsar NP 0532 within the Crab nebula as separate x-ray emitting objects, the Tycho and Cas A supernova remnants, and, more recently, the Vela X supernova remnant and the Cygnus loop.

(d) Normal galaxies–the Large Magellanic Cloud.

(e) Active radio galaxies–the peculiar radio object M87.

(f) Quasi-stellar objects–the quasar 3C 273.

There has now been a positive identification of high-energy gamma rays coming from the plane of our galaxy, with a concentration toward a source in the galactic center. The gamma rays have energies in the vicinity of 100 MeV and are believed to arise predominantly from the production and decay of neutral pions as a result of collisions between energetic cosmic-ray particles and the gas in the galactic disk.

B. STELLAR X-RAY SOURCES

Sco X-1 has been the most extensively studied of the x-ray sources, partly because it is normally the brightest x-ray object in the sky, nearly as bright as the sun in x-ray wavelengths. It has been identified with a peculiar blue stellar object that has many of the optical spectral characteristics of an old nova–strong emission lines superimposed upon a strong blue continuum. Optically, the source is quite variable, with fluctuations of the order 10^{-2} magnitude on a time scale of minutes and variations of up to one magnitude in an hour. In the x-ray region, the variability is more enhanced, with "flares" in which the x-ray intensity may vary by a factor of 4. The variations in the optical and x-ray region are usually closely correlated. An important program has been the simultaneous optical and x-ray observations of these variations in Sco X-1.

The x-ray spectrum of Sco X-1 is exponential and has usually been interpreted as arising from bremsstrahlung in an optically thin source having a characteristic temperature of about 5×10^7 K. This characteristic temperature varies with the changes in the x-ray spectrum and covers a range of about a factor of 2 in apparent temperature during such variations. There are some problems, however, with the interpretation of the Sco X-1 source as a pure thin source bremsstrahlung emitter. The source must be optically thick in the optical range, and there is conflicting evidence concerning its brightness in the soft x-ray range around 50 Å. If it were a pure high-temperature plasma, then it should be a copious emitter of discrete lines in the x-ray range corresponding to the K-shell transitions of a number of the intermediate elements, from oxygen up to the iron region. Attempts have been made to observe these lines, and the observed line emission is at least one order of magnitude less than the expected emission from an optically thin plasma. It is evident that many more refined measurements must be made before the true nature of Sco X-1 can be determined. It may even be premature to rule out nonthermal sources of x-ray emission; polarimeter measurements in the x-ray range may be needed to determine this point.

The source Cyg X-2 is significantly different from that of Sco X-1. In the optical region it has both emission and absorption lines. Once again this source is variable in time, but it has not been observed nearly so extensively as Sco X-1.

It has been suggested that the x-ray emission from these objects arises from a binary system in which one of the stars is quite compact, a white dwarf or neutron star, onto which gas accretes from the binary companion. However, there is no spectroscopic evidence of the binary nature of Sco X-1 and Cyg X-2.

C. TRANSIENT SOURCES

Two prominent sources in the x-ray sky have been transient in nature. In 1967, the source Cen XR-2 grew rapidly in intensity until it was approximately as bright as Sco X-1. After a few weeks at this intensity, the source faded until it was no longer easily detectable.

In 1969, the source Cen XR-4 was discovered from routine satellite x-ray monitoring operations. It increased in brightness until it was some three times the brightness of Sco X-1, and then, after about a month, it declined by more than a factor of 200 from its peak flux until it could no longer be observed.

These two bright sources were not visible for a long enough period for x-ray modulation collimators to be flown to determine their positions accurately. Therefore it was not possible to make optical identifications of these objects. Until such optical identification can be made, the nature of these strong transient x-ray sources must remain a mystery. However, because two such strong sources have been observed in our galaxy within the past few years, the phenomenon must be quite common, and x-ray instrumentation on board satellites will permit accurate location of the sources and, it is hoped, optical identifications.

D. SUPERNOVA REMNANTS

One of the brightest of the x-ray sources is the Crab nebula supernova remnant. This has been resolved into two separate types of x-ray source. The radio and optical emission appears to be synchrotron radiation from high-energy electrons spiraling in the magnetic field of the Crab nebula. The x-ray emission from the nebular part of the Crab may also result from the synchrotron process; polarimeter measurements in the x-ray range are highly desirable to verify this.

The pulsar NP 0532, which is situated near the center of the nebula, appears to inject high-energy electrons into the Crab nebula. This pulsar has the shortest period of any known object of its class, only 1/30 sec. This object is believed to be a rotating neutron star; the loss of rotational energy by a rotating neutron star slowing down at the rate that the Crab pulsar is observed to do is just sufficient to account for the radiation in the nebula in the radio, visible, and x-ray regions. The x rays from the pulsar arrive in twin bursts having similar timing but different amplitudes than the optical pulses. It would be highly desirable to determine whether the x-ray pulses have the same polarization properties as do the optical pulses.

The spectrum of the Crab nebula follows a general power law, and the nebula is the brightest object in the sky in the hard x-ray wavelength range. A few of the unidentified x-ray sources appear to have power law spectra similar to that of the Crab nebula, and these may also be old supernova remnants.

The x radiation from other supernova remnants, Tycho, Cas A, Vela X, and the Cygnus loop, may have a rather different character than the Crab nebula. This radiation may arise from the interaction of the ejected supernova envelope with the interstellar medium. Such interaction can shock-heat and compress the interstellar medium to high temperatures, possibly leading to particle acceleration and x-ray emission. In particular, the x-ray emission from the Cygnus loop is associated with certain parts of the loop structure, is strong where the loop is weak, and does not show a central source in the Cygnus loop. However, the observations that have so far been made on these remnants do not identify the character of the emission.

Surveys have failed to detect x-ray emission from other old supernova remnants; these will remain prime candidates for x-ray sources when instrumentation with greater level of sensitivity to x-ray emission is available to survey the sky.

E. EXTRAGALACTIC X-RAY SOURCES

The Large Magellanic Cloud radiates an x-ray flux equivalent to the emission of about 150 sources like Sco X-1. The nature of the sources in the Large Magellanic Cloud cannot be determined from a gross measurement of this kind. Since the Large Magellanic Cloud occupies a region about three degrees in the southern sky, it should be possible, with greater sensitivity x-ray detectors, to pinpoint the individual sources in the Cloud and to learn about the general nature of the x-ray sources in another galaxy, more gas-rich than ours.

The bright radio galaxies are generally thought to produce large numbers of energetic electrons, which emit by the synchrotron process. If there is a continuing injection of high-energy electrons in such sources, it is quite possible that they emit x rays in a manner analogous to that of the Crab nebula. It is therefore interesting that the galaxy M87 has been identified with an x-ray source; M87 emits some 10^{43} ergs/sec in the x-ray region, almost as much as in the optical region. This galaxy has a prominent jet, which emits the strong flux in the radio wavelength range and hence is the prime candidate to be the emitter of the x rays; this is a major hypothesis that can only be tested by measurements with high angular resolution.

Many high-energy phenomena are associated with quasi-stellar objects. The objects are generally very strong radio emitters, and some of their visible emission may be due to the synchrotron process. If there is a continuing acceleration of very energetic electrons in such objects, this might be detected via x-ray emission. 3C 273, the source that has so far been identified as an x-ray emitter, is the brightest quasi-stellar object. It was evident that, as the sensitivity of detection increased, other quasi-stellar objects would be studied for x-ray emission—peculiar galaxies, such as Seyfert galaxies and galaxies with very bright condensed central cores—and the *Uhuru* satellite has started this important survey.

F. THE ISOTROPIC X-RAY BACKGROUND

One of the most important results in x-ray astronomy has been the discovery of the diffuse isotropic background radiation, made as a result of a sky survey carried out on an Orbiting Solar Observatory. The spectrum of the background radiation is known with considerable accuracy from about 1 keV to 1 MeV. The slope of the spectrum increases above about 40 keV. In order to elucidate the nature of the background radiation, it will be important to determine more accurately the structure of the spectrum near this change of slope.

The isotropy of the x-ray background radiation is probably its most significant characteristic. The intensity appears to be the same from all directions of the sky to within less than 1 percent. Since we are situated well off center in our own galaxy, the x-ray background radiation almost certainly has an extragalactic origin. It has been suggested that the background x radiation must be cosmological in origin, either a continuum or arising from a large number of sources evenly spread over the sky at large distances. The bulk of the evidence seems to favor this interpretation, and if this is correct, the x radiation must come from very large distances in the universe and have been emitted early in the history of the expanding universe about 10 billion years ago. The background is sufficiently intense to be easily distinguished from galactic x rays, and if it was produced in the distant cosmological past, a better determination of its features will discriminate among cosmological models.

G. SOFT X RAYS

So far very few observations have been carried out in the soft x-ray region, at a wavelength of 44 Å and longer. A great deal of information about the galaxy should be obtainable in this soft x-ray range. The absorption coefficient for such x rays in the interstellar medium is rather high, so that x rays from distant sources in the plane of the galaxy are strongly attenuated. This attenuation will give information about the structure of the interstellar medium between us and the x-ray sources. Similar attenuation of the isotropic background radiation occurs. The absorption of the background radiation would seem to provide a means for determining variations in the column densities of matter in the interstellar medium. However, recent measurements have suggested that there are many local sources of soft x rays that make absorption measurements of the diffuse background radiation complicated. Clearly, the soft x-ray region merits considerable further study.

It is important that the measurements in the soft x-ray region be made with reasonably good spectral resolution, because the K-shell absorption edges of elements from oxygen to magnesium should be readily detectable as a result of interstellar absorption of soft x rays. It is also desirable to map the sky in the soft x-ray region with resolution of the order of seconds of arc. This will show the density and structure of the interstellar medium. It would also permit a search for x-ray halos produced by scattering by interstellar grains of x rays from distant sources.

H. RESULTS FROM THE *Uhuru* SATELLITE

At the time that most of this panel report was written, only early preliminary data from the *Uhuru* satellite were available. In the past year, a number of significant and unexpected results have come from the satellite. These do not significantly change the recommendations of the Panel, but they are indiciative of the rapid rate of change in this field. A brief summary of major results follows.

The *Uhuru* satellite (SAS-A) was launched on December 12, 1970, in an equatorial orbit of about 300 nautical miles. Notwithstanding the loss of the tape recorder and other spacecraft malfunctions, the x-ray instrumentation has shown no degradation to date (August 1972) and data on celestial x-ray sources have been obtained continuously during the past 20 months.

A catalog has been compiled, which includes some 130 sources ranging in apparent luminosity from Sco X-1 (the first observed x-ray star and the most intense in the 1–10 keV energy region) to 10^{-4} Sco X-1. Of these sources 50 appear to be extragalactic and 80 galactic.

With respect to galactic sources:

1. Large variations in intensity of the x-ray sources is the rule rather than the exception. Several novalike sources have been detected in which the intensity changes by factors of as much as 100 in a few days. Variability of factors of 2 in time scales of months to minutes is present for almost all sources. Several sources emit pulses (Cyg X-1, Circ X-1) or trains of pulses in time scales of 100 msec or less. Some sources (Cen X-3, Herc X-1) are emitting periodic pulsations with characteristic periods of seconds.

2. Individual stars can emit x rays with very large intrinsic luminosities of the order of 10^{38} ergs/sec making them among the brightest stars in the sky. This became apparent with the detection by *Uhuru* of individual stars in external galaxies of the local group, the Large and Small Magellanic Clouds.

3. The binary nature of several galactic x-ray sources has been re-

vealed. The measurement of the sinusoidal Doppler shift in the period of pulsation by Cen X-3 and Herc X-1 has allowed the precise determination of orbital velocities, a direct measurement of the size of the orbits, and a measurement of the reduced mass of the systems, with finer precisions than have yet been achieved in optical measurements. The observation of x-ray light curves corresponding to eclipse periods of a few days has allowed the identification of several other binary systems, including one in the Small Magellanic Cloud. An x-ray emitting phase may be a common event during the evolution of stars in short-period binary systems.

4. The existence of fast time variations in several x-ray sources and the measured parameters of the components of the binary systems strongly indicate that galactic x-ray sources correspond to stars near the end point of their evolution. In most cases, the x-ray emitting object appears to be a white dwarf or a neutron star. In one case, Cyg X-1, the evidence strongly suggests a very massive compact object, possibly the first black hole ever detected.

Uhuru has increased by tenfold the number of x-ray sources believed to be extragalactic objects.

All galaxies are found to be x-ray emitters with intrinsic luminosities spanning a range from 10^{39} ergs/sec to 10^{46} ergs/sec. In normal galaxies, such as our own, the emission is predominantly the result of the integrated contribution from individual stellar sources. This holds true for the Magellanic Clouds as well as for M31, the spiral galaxy in Andromeda. The intrinsic luminosity is of the order of 10^{39} ergs/sec.

Seyfert galaxies, N-type galaxies, quasars, and radio galaxies have much greater intrinsic luminosities spanning the range from 10^{42} to 10^{46} ergs/sec. This emission appears not to be due to stellar sources but rather to high-energy events taking place in the nucleus of the galaxy. This view is supported by the detection of significant low-energy cutoffs in all such sources, which imply that the x-ray sources are imbedded in a dense region such as is believed to exist in the nucleus of active galaxies.

In addition, the most significant of *Uhuru's* contributions to extragalactic astronomy have been the following:

1. The discovery of another type of x-ray emission associated with clusters of galaxies. In the Virgo, Coma, Perseus, and several other clusters, an extended region of emission of about 1° diameter has been detected. The absence of low-energy cutoffs implies that this emission is not due to the integrated contribution of individual galaxies. The emis-

sion may be due to a tenuous, hot gas having a mass about equal to the one contained in the galaxies and emitting x rays via the thermal bremsstrahlung process. There is some evidence that x-ray emission is related to the cluster kinematic parameters. The intrinsic luminosity is of the order 10^{42} to 10^{44} ergs/sec.

2. The discovery of a large number of objects that are believed to be extragalactic but are as yet unidentified with any known class of galaxies. These objects are not simply the next brightest member in each of the classes of galaxies that have been identified as x-ray emitters. The absence of peculiar visible or radio counterparts at their location seems to indicate that the x-ray emission is the predominant form of electromagnetic energy loss. The evidence seems to be compelling for the existence of a new class of galaxy—the x-ray galaxy. In addition to the intrinsic interest in the physical conditions that might lead to such abundant x-ray emission in a galaxy, this class of objects has great interest in that they might be responsible for a large fraction of the diffuse x-ray background.

I. FUTURE PROGRAMS IN X-RAY ASTRONOMY

An object like the Crab nebula would be reduced in intensity by a factor of 10^3 if it were at the distance of the Large Magellanic Cloud and by a factor of 10^5 if it were in M31 (the Andromeda galaxy). It is quite feasible with existing technology to build x-ray detectors capable of detecting the Crab nebula in the Andromeda galaxy, and this should be one of the major aims for x-ray astronomy in the near future. The detection of individual sources in nearby galaxies, where all the sources are essentially at the same distance from us, may aid in determining the nature of the sources. This will require a positional accuracy of a few seconds of arc. A similar positional accuracy is required for galactic sources in order to make unambiguous optical identifications. Hence, an x-ray survey reaching to limits of 10^{-5} of the Crab nebula with positional accuracies of a few seconds of arc is highly desirable.

Once many x-ray sources have been accurately located, further studies of these must be carried out using telescopic principles for photon collection and concentration and a variety of different instrumentation to determine characteristics of the x-ray emission. The source should be imaged in soft x rays to a high enough accuracy so that any departures from a point source can be determined, such as finite area of source emission associated with supernova remnants or halos resulting from grain scattering. It is important to examine the x-ray spectra to determine whether x-ray emission lines are present in the source or whether absorption edges of elements in the interstellar medium are present. It is important to determine whether the x rays have any net polarization that may result not only from the synchotron process but also from Compton scattering under conditions of nonspherical geometry.

A variety of x-ray instrumentation is planned to be launched in the near future on board Orbiting Solar Observatories, Small Astronomical Satellites, and satellites in the international series. The programs include sky surveys, spectral measurements, and polarimeter measurements. The levels of sensitivity in the survey may reach about 10^{-3} of the Crab nebula with positional accuracy of the order of minutes of arc. These programs should have an immense influence on the near-future development of x-ray astronomy, since they will extend the observations to a much greater sensitivity than has been possible in rocket flights, and they will provide instrumentation for scanning the sky on a regular basis. Because the field of x-ray astronomy is still very new, it is likely that a variety of significant basic discoveries in x-ray astronomy will be made as a result of these programs. It does not seem possible to recommend further instrumentation in the size and weight range of these small satellites until the results from the planned instruments are available.

These small satellites, however, will not exploit the technology that now exists for x-ray astronomy. In order to achieve the goals outlined earlier—detection of sources 10^{-5} times the strength of the Crab nebula with the positional accuracy of a few seconds of arc and spectral and polarimetric measurements made via x-ray telescopes—it is necessary to put much heavier x-ray instrumentation in space. Such a program is planned in the proposed NASA High Energy Astronomical Observatories (HEAO's). The observatories are planned to have both scanning and pointable modes. The scanning modes would be used mainly for a sky survey, and the pointable modes would be used primarily for determining the properties of individual sources. The Panel endorses the concept of both types of observatory very strongly. It is highly desirable that the x-ray sky survey be made to limits as close as 10^{-5} times the strength of the Crab nebula as possible, that the positional accuracy of the faint x-ray sources be determined at least to minutes of arc, that the positional accuracy of the stronger ones be determined at least to seconds of arc (probably using modulation collimators), and that the survey be carried out not only for the ordinary x-ray region, from about 1 to 100 keV, but also simultaneously for the soft x-ray region, perhaps using a survey telescope. At the same time, it is desirable that additional information be obtained regarding isotropy of the diffuse background radiation and that better spectral measurements be made of this radiation.

The large instrumentation that is required to carry out these objectives will probably include focusing x-ray telescopes at least for the softer x-ray region. These telescopes have the advantage of concentrating the x rays in a small image, which facilitates the determination of source structure and greatly reduces background effects due to the diffuse x-ray background and cosmic rays traversing the detectors. The x-ray detectors may include proportional counters for the usual x-ray range, scintillation detectors for the higher-energy x rays, and perhaps solid-state devices for the softer x-ray region. The accurate determination of x-ray spectra, including line spectra, probably requires the use of Bragg spectrometers. X-ray polarimeters can be constructed in a variety of ways, but one possible detection method uses Compton scattering of the x rays.

The launching of a series of HEAO's carrying x-ray instrumentation of the heavier variety described here would have a truly revolutionary impact on the development of x-ray astronomy and would rapidly raise it to the level of a mature astronomical science. Only by placing heavy instrumentation of this type in earth orbit is it possible to study large numbers of individual x-ray sources, to locate positions sufficiently accurately for optical identification, and to make possible coordinated studies of the x-ray and optical variations of such sources.

J. GAMMA-RAY ASTRONOMY

The recent satellite detection of 100-MeV gamma rays marked the successful completion of a series of attempts to discover such gamma rays. A galactic gamma-ray source was expected, because cosmic rays interacting with atomic nuclei in the interstellar gas provide a number of mechanisms for gamma-ray production. The most important of these processes is the decay of the neutral pi-mesons produced in nuclear collisions of cosmic rays with the interstellar gas. The calculated flux of gamma rays from this process is in approximate agreement with the observed flux from the general region of the disk of the galaxy, but in the direction of the galactic center the observed intensity of gamma rays is about 5 times as large as the calculated flux. This may indicate a high flux of cosmic rays near the galactic nucleus.

The gamma-ray experiment provides very poor angular and energy resolution. An important observational development for the future would be a measurement of the gamma-ray spectrum. Such information would provide conclusive tests for theories of the origin of these gamma rays. In particular, the π^0 decay process predicts a flat-topped spectrum that is symmetric about $\frac{1}{2}m_\pi c^2 = 67.5$ MeV if the photon energy is plotted logarithmically.

The future of gamma-ray astronomy requires the placing of larger instrumentation in earth orbit. Because the flux of gamma rays is very small, and because gamma rays are deeply penetrating, the detectors must have a large area and a considerable mass. Thus, further instrumentation for the study of gamma rays in the 100-MeV range from the galaxy requires heavy instrumentation such as that which would fit into the proposed HEAO.

This Panel *recommends* the inclusion of such heavy gamma-ray instrumentation in the HEAO's for the purpose of increasing our knowledge and information about the gamma rays that are produced from our own galaxy and also with the possibility of detecting such gamma rays from extragalactic sources.

In the lower range of gamma-ray energies there are also important phenomena that might be detected with large instrumentation in space. In particular, there are many processes that probably exist in our galaxy that produce line sources of gamma rays in the MeV region. These processes include electron–positron annihilation, giving a line at 0.51 MeV, and the capture of neutrons by hydrogen, giving a line at 2.2 MeV. In addition, current theories of supernova explosion require that they eject a variety of both short-lived and longer-lived radioactivities, which in turn should produce many different gamma-ray lines. These radioactivities may include everything from large amounts of ^{56}Co with a 77-day half-life to much smaller amounts of transuranic elements. The optical spectra of supernovae suggest large masses of nearly normal composition as seen in the first year; but in older supernovae remnants, the intermediate life of radioactivity may still be present. One of the major suspected sources of such radioactivities is the Crab nebula. Successful detection of radioactivities in the Crab nebula would require the inclusion of a gamma-ray spectrometer on one HEAO.

K. INSTRUMENTATION

Much new instrumentation has been developed recently for use in high-energy astronomy. Although these instruments are based on the same principles as those regularly used in the laboratory for x- and gamma-ray measurements, the particular requirements imposed by the space environment, the cosmic radiation background, the weak fluxes, and remote operation present unique problems. The instruments that may be used on the HEAO's are particularly demanding of experimental ingenuity, and some of the unique features are indicated here.

Measurements in the soft x-ray region (100 eV to 10 keV) may require very-thin-window proportional counters with areas of 500 to

1000 cm^2. These require gas flow and replenishment systems with one-year lifetime. Two methods, the use of multichambered counters and pulse rise-time discrimination, have been developed for reducing the background in proportional counters. At x-ray energies in the 1–10 keV range, counters with a total area of about 50,000 cm^2, usually constructed in modules, can be used for high sensitivity. Modulation collimators, requiring precisely aligned grids of thousands of fine wires placed in front of proportional counters are used for precision source location and angular sizes.

The development of grazing incidence reflection optics for x-ray astronomy is of particular significance. Here concepts of focusing coupled with new material and manufacturing and alignment technology permit x-ray images to be obtained and have already resulted in new information on the size of solar active regions. X-ray image conversion and TV systems are being developed for use in the near future. These devices, which now have diameters to several meters, can also be used as large area collectors with Bragg crystal spectrometers, x-ray polarimeters, or high-resolution counters at the focus.

Scintillation counters and solid-state Ge(Li)-cooled detectors are the basic components of detectors in the 10-keV to 10-MeV range. Shielding and collimation problems are extremely complex, and detectors that might be flown in HEAO consist of up to 500 kg of active anticoincidence scintillation material surrounding clusters of detectors. Sandwiched and stacked phosphors that have different characteristic light decay times in the shield and detector complex are used to identify different interaction types and therefore reject background. Cooling Ge(Li) to liquid nitrogen (77 K) temperatures for periods of a year requires the new cryogenic engineering methods now being applied to the problem (which would be ultimately necessary for infrared satellites). Detectors with areas of several hundred cm^2 are now feasible; extreme sensitivity by using methods other than the brute-force clustering of many identical modules requires techniques not now available.

Spark chambers of 1-m volume with extremely complex anticoincidence systems, can be used for astronomy in the 10–10^3-GeV range. These, if coupled with a total absorption device or a "calorimeter," present extreme data transmission and handling and analysis problems but would give sensitivity of about 10^{-7} photon cm^{-2} sec^{-1}, angular resolution of a degree, and energy resolution of 5 to 10 percent. Alternative instrumentation, which is lighter and of higher sensitivity, but requires technical development, could consist of a transition radiation detector together with a gas Cerenkov telescope, possibly supplemented with two planes of spark wires of Charpak counters. Such instruments present a very large increment over the previous satellite and balloon experiments, which were just capable of detecting the weak galactic flux at 100 MeV.

IV. INFRARED ASTRONOMY

A. INTRODUCTION

The infrared region of the spectrum extends from approximately 1.5 μm to 1.5 mm. As discussed in the report of the Infrared Panel, within this range lies a wealth of information concerning almost all astronomical objects, from the sun and solar system to galaxies and the universe. At ground level, atmospheric transmission windows near 2, 5, 10, and 20 μm have been explored, and attempts are being made to utilize the much poorer windows at 350 and 450 μm. Only the first few of these are relatively transparent; the opacity of the atmosphere is principally due to the presence of water vapor, and the transparency of the higher-wavelength windows can vary markedly from day to day.

Modern infrared photometers are limited by noise associated with fluctuations in unwanted background, which may come from warm optical components of the telescope or from an external source such as the sky. In the case of the ground-based telescope, the noise contribution from the sky dominates other sources, and observations are possible only when the field of view is restricted to allow only a small amount of light from the sky to reach the detector. These fields of view are typically a few seconds of arc and allow observation of small sources, but, because of the long observing time required, they do not permit surveys to be carried out over large areas. Even though the average background radiation may be 10^6 times the signal from the point source under study, it can be electronically subtracted from the data by the use of a technique called spatial differential chopping. The optical beam is rapidly moved from the source to the nearby sky, and only the difference in signal is amplified with an ac amplifier and coherent detector. Statistically, the signal-to-noise ratio increases with the square root of the observing time; and, in principle, given enough time (typically an hour), any point source can be studied regardless of instrument or sky radiation as long as the wanted light is not absorbed by the atmosphere. Mathematically, the differential chopping technique is equivalent to measuring the first derivative of the spatial intensity distribution. For objects with slowly varying surface brightness such as nebulae or galactic background, the technique becomes less sensitive and is useless for a general background.

If an infrared telescope is used at high aircraft or balloon altitudes, the bulk of the water vapor lies below, and hence the sky background noise is greatly reduced. There are still some water vapor and other infrared absorbing molecules above the instrument. The infrared emission lines from the atmosphere are still plentiful, but their pressure-broadening has been greatly reduced; atomic lines, such as the 63-μm transition of atomic oxygen also persist. Instruments must still use differential chopping because of the warm optics and, less importantly, because of the residual emission of the atmosphere. Efforts to reduce the infrared emissivity of optical mirrors run into practical limits due to dust and surface imperfections, with the result that minimum effective background temperature of an airborne system appears to be about 150 K. The advantages of the airborne system are its increased field of view due to the lower background level and an essentially unlimited wavelength range. Absolute cosmic background measurements are impossible, and observing times of the order of a minute are needed to correct for fluctuations in the instrumental background.

The basic reason that cold optical systems cannot be used within the atmosphere is condensation of the atmosphere on cold surfaces. At high altitudes with minimal atmospheric emission, the telescope must still have a window, which cannot be cooled below the boiling point of the surrounding atmosphere and therefore radiates. Only at altitudes of above 100 km can the optical system be cooled by liquid helium without atmospheric condensation. An effort is being made to develop helium-cooled optics for use in the atmosphere, protected by a thin membrane, warm but negligibly radiating. This is an extremely difficult technical problem, uncertain of success under the field conditions but should be vigorously pursued.

Spacecraft, rockets, or satellites present no such limitation on the cooling of optics; the instrumental background can be completely eliminated. The field of view can now be chosen to suit the astronomical problem at hand, and even absolute background measurements become possible. Chopping of the light is still employed but for a different reason: amplifiers and detectors have noise varying inversely as the frequency; modulation by an alternately opaque and clear stop allows higher electronic gain. Detectors and amplifiers may be made with microsecond response times, since there is no background noise. The only basic limit to signal-to-noise ratio is then the photon-counting statistics of the source being studied. Bright sources should be observable in a few milliseconds. This makes a space system with a cooled detector ideal for sky surveys.

These technical limitations have profoundly influenced the development of infrared astronomy. Most far-infrared studies have been motivated by pre-existing knowledge of objects discovered in the optical wavelength region. The source characteristics of many such objects have been studied in the infrared, both from the ground and within the atmosphere. By accident, a few dominantly infrared emitting objects have been found that happened to lie close to objects being studied. It seems quite probable that many types of object emitting mostly far infrared will be discovered when the sky is surveyed at longer infrared wavelengths. The only reasonably complete sky survey so far carried out is at 2 μm. Currently, an effort is being made to carry out a systematic survey at 5 μm. The only practical way to carry out an infrared sky survey at longer wavelengths is to use satellite or rocket instrumentation. Infrared studies on the ground and from the atmosphere will benefit greatly from the results of such a far-infrared sky survey. The survey would enable uniquely infrared phenomena to be studied, as well as those that have optical counterparts.

B. CHARACTERISTICS OF INFRARED SYSTEMS

The cost of these observations rises steeply with the altitude necessary, making it important to understand which types of observation are possible from which kind of platform.

Many ground observations utilize the region from 8 to 14 μm generally termed the 10-μm window. The state of the art, utilizing a 60-in. infrared telescope, allows detection of infrared fluxes down to a limit of about 0.05 flux unit (one flux unit is 10^{-26} W Hz^{-1} m^{-2}). Future improvements in instrumentation and techniques may reduce this to near the 0.005 flux unit. This is needed for a general study of infrared galaxies.

A NASA airplane will incorporate a 36-in. telescope for infrared observations. This airplane platform, if it performs to specifications, will extend the spectral range for broadband observations of pointlike sources. The telescopic aperture is smaller than available from the ground, and integration times cannot be as long, making ground-based telescopes superior in the near-infrared 10-μm window. Broadband high-resolution spectroscopy, using an interferometer, can best be made with the aircraft telescope, since the number of overlying narrow atmospheric lines is greatly reduced at altitudes of 50,000 ft. Neither a ground telescope nor the NASA aircraft telescope is suitable for a sky survey in this window, leaving as the only practical means the high detector sensitivities associated with helium-cooled optics at very high altitudes.

Observations near 100 μm can only be made from above the tropopause; from balloons, a sensitivity for detecting 100-μm infrared fluxes down to about 1200 flux units has been obtained. Because of the difficulty inherent in pointing a balloon accurately and with stability, only about ½ sec of observation time can be utilized out of a minute of scanning of the sky, without expensive pointing instrumentation, such as that in Stratoscope.

If various peripheral improvements are made in balloon systems, still within the present state of the art, then the observation time on a single object can be increased to about 3 min, permitting intensities as low as 8 flux units to be observed, although this theoretical limit would be difficult to achieve in practice. Nevertheless, it indicates that improvements by over a factor of 100 can be made in infrared astronomy using balloon techniques. Future balloon observations will yield valuable results.

The NASA aircraft flying at 50,000 ft, carrying a larger-aperture telescope than balloons, should also be capable of accurate pointing and longer integration times than are probable from balloons. If the aircraft telescope performs at its theoretical limit with existing technology, sources as faint as 0.4 flux unit may be detected at 100 μm.

In a small infrared satellite, the logical telescope aperture is probably near 12 in., fully cooled to at least 4.2 K. Such satellites should probably not be flown until infrared detectors with noise backgrounds lowered by about a factor of 10 have been developed. If such a satellite can point and use 10 h of integration time, then about a factor of 5 in broadband spectroscopy would be gained over the theoretical maximum performance of the 36-in. aircraft telescope. This is a relatively minor gain for the large cost involved in operation of a small cooled infrared satellite telescope. The value of such a small satellite telescope lies in the survey mode, which cannot be done within the atmosphere, rather than in the broadband spectroscopy of infrared sources. A small infrared pointable space telescope would detect strong infrared galaxies across enormous distances in space. If an infrared galaxy can be expected to emit about 30,000 times as much radiation in the infrared as does the nucleus of our own galaxy, a satellite should detect it at a distance of 1500 megaparsecs.

An infrared space telescope of large aperture would be an extremely versatile and powerful instrument for the future of infrared astronomy. Developments in technology may push the limits of sensitivity discussed above to considerably lower levels than quoted. Such possible directions of technological advance would include the development of tunable Josephson-junction detectors (very promising for a narrow-band infrared detection), nonlinear mixing devices, and development of infrared photon counters.

C. GENERAL RECOMMENDATIONS

Major advances in infrared astronomy are likely to follow the development of the NASA infrared airplane. The Panel therefore *very strongly endorses* this airplane and *recommends* that it be utilized in a diversified program of infrared research with as many hours available for observation as possible. Other specialized experiments in infrared astronomy, particularly those involving spectrophotometric work, are likely to be required, many of which can use smaller aircraft. It is *recommended* that such more specialized infrared programs should also be funded.

Balloons are relatively inexpensive and can attain higher altitude than available aircraft. Since neither aircraft nor balloon techniques are fully developed, the Panel *recommends* a continuing balloon effort until the relative advantages of aircraft and balloons for investigations of different problems have been determined.

The observations from balloons and aircraft will be greatly enhanced by completion of an infrared survey at longer wavelengths, which is likely to discover unique infrared objects without optical or near-infrared counterparts. The Panel therefore considers it desirable to carry out such a survey at several longer infrared wavelengths as soon as possible. The only practical mode for this survey appears to be a small infrared astronomy satellite, carrying a telescope of approximately 12-in. aperture. Such a telescope should only be flown after further development of infrared detectors and provided that a reliable system with helium-cooled optics cannot be developed for use in the high atmosphere. An active program of rocket infrared astronomy is required in order to develop and test such sensors. Such rocket experiments, in the meantime, give extremely valuable information about infrared sources.

The rocket infrared program has so far not given unambiguous results. Rocket infrared telescopes have seen bright pointlike sources of radiation, but it is not yet certain that these are not artifacts such as dust from the rocket itself. A high level of infrared background measured over a band several hundred micrometers in width was measured from a rocket. It is not yet certain that the excess infrared radiation did not arise from problems of absolute calibration or possible inadequate baffling. The most reliable rocket infrared results concern the atmosphere.

Atomic oxygen has been detected by its 63-μm emission at 200 km, and other emissions have been seen in the 10–30-μm band. No rocket altitude atmospheric spectroscopy is as yet available.

One of the objectives of future rocket infrared astronomy should be development of techniques that can be incorporated into a satellite-borne telescope, cooled to below 4 K by liquid helium. With a telescope of 12-in. aperture, the diffraction disk at 10 μm is near 16 sec of arc; at 100 μm the diffraction disk is about 3 min of arc. The resolution becomes further degraded at the longer infrared wavelengths. Therefore, such a telescope needs a pointing capability of only a few minutes of arc, now attainable in the Small Astronomical Satellites. Given the very long lead time for development of such a satellite, it would be well to begin at least conceptual design now; decision on detectors and objects for observation must await the results of rocket probes.

No attempt should be made to combine a cooled infrared system with a visible or ultraviolet telescope. Diffraction-limited optics in the infrared will be easier to obtain, and extreme cooling of ultraviolet optics is unnecessary. Detection of that infrared between the visible and 5 μm which does not require cooling may be incorporated into a system like the Orbiting Astronomical Observatory.

A small infrared astronomy satellite for an infrared survey might have a cryogenic reservoir adequate for perhaps one month of operation. The efficiency of utilization of the spacecraft and rate of expenditure of cryogen would depend on the orbit chosen for the spacecraft. If orbits can be chosen that minimize the time spent looking at the earth, the moon, and the sun, the cryogenic reservoir will last a considerably longer time. For a complete sky survey, three such telescopes are needed, at different seasons of the year, so that the sun and the moon would have different locations in the sky. An alternative to launching three survey telescopes would be replacement of liquid helium from the ground at regular intervals. The proposed space shuttle may be useful for a servicing operation of this sort.

For the larger infrared telescopes of the future, closed-cycle refrigeration of the entire telescope might be possible. This would require power levels higher than those obtainable from solar cells, a nuclear power source, for example.

The Panel gives highest priority to the development of small infrared astronomy satellites for the purposes of an infrared survey. It *recommends* that a program of development of the necessary technology be carried out as rapidly as possible, and that the components of this technology be tested via an expanded program of infrared astronomy rockets. It also encourages attempts to develop for use in aircraft the alternative of helium-cooled optics protected by thin membranes in order to carry out an infrared survey.

V. PARTICLES-AND-FIELDS ASTRONOMY

A. INTRODUCTION

Particles-and-fields astronomy is concerned with the properties of the particles (atoms, nuclei, electrons) and electric and magnetic fields that are present in space. Cosmic rays, interplanetary plasma with its associated electric and magnetic fields, and interstellar plasmas and fields are discussed in this section. These varied topics may be thought of as constituting the unified topic of rarified astrophysical plasmas. Many phenomena of the earth's magnetosphere (and other magnetospheres) are closely related to phenomena discussed here, but specifically magnetospheric phenomena will not be considered here.

The importance of particles-and-fields astronomy for present-day astronomy arises in large part from two unique features. The study of the interplanetary medium from space probes provides the possibility of measuring directly, *in situ*, many of the most important astrophysical processes difficult to study experimentally on the ground. Examples of such phenomena are collisionless shock waves, acceleration of charged particles to high energies, plasma turbulence, and magnetic-field annihilation. The knowledge of plasma physics gained in this new regime should have important application in the studies of laboratory plasmas and controlled thermonuclear fusion. The second important feature of particles-and-fields astronomy is that cosmic rays provide us with a sample of matter from the sun to the farthest reaches of the galaxy (and, perhaps, beyond), in addition to telling us much about interstellar and interplanetary space.

At present, much of particles-and-fields astronomy is concerned with the manifold phenomena in the solar wind. Following the early work of Biermann and Parker predicting a continuous, supersonic outflow of gas from the sun, the measurement and interpretation of the properties of the solar wind became one of the most exciting programs of the early years of the space age. Our knowledge of the solar wind is far from complete, but many of the basic phenomena are of fundamental importance elsewhere in astronomy and astrophysics, as well as in other branches of physics. For example, the discovery in the solar wind of collisionless shock waves in which collisions are not the basic mechanism of particle–particle interaction, in contrast to usual shock waves in the laboratory, provided knowledge of use to laboratory plasma physicists, as well as adding to our understanding of solar flares, supernova explosions, and similar violent events. Fast charged particles, cosmic rays, interact strongly with the electric and magnetic fields in the solar wind, so that cosmic rays tell us about the solar wind and vice versa.

The other chief concern of particles and fields is the elucidation of the nature and origin of cosmic rays. The strong interaction of these charged particles with the solar wind and the interstellar medium is important. Also exciting is the prospect of better understanding the evolution of the galaxy through a study of the composition of cosmic rays.

B. RELATION TO OTHER FIELDS OF ASTRONOMY

The sources of the radiation of some radio-astronomical objects are energetic charged particles (or cosmic-ray baryons and electrons) emitting energy via synchrotron radiation or bremsstrahlung; understanding the source of the radiation entails understanding cosmic rays. Similarly, scintillation (fluctuation in brightness) of certain small radio sources is due in part to the effect of fluctuations in the solar-wind density on the propagation of radio waves (analogous to the "twinkling" of stars in the terrestrial atmosphere). There are many other similar examples.

A second influence is more indirect, involving application of knowledge gained in particles and fields to problems in other disciplines. An illustration is the light cast on problems of nucleosynthesis, and stellar and galactic evolution, by studies of the composition of cosmic-ray particles. This input is particularly useful because galactic cosmic rays are an actual sample of matter from the galaxy. One of the important goals in cosmic-ray research outside the atmosphere must be the determination of the isotopic composition of cosmic rays at least as high as the abundance peak at iron. This will give much needed information about the astrophysical sources of these cosmic rays.

An accurate determination of the charge composition of the cosmic rays beyond iron, possibly up to the hypothetical semistable superheavy elements beyond uranium, is badly needed to understand heavy-element nucleosynthesis.

Cosmic rays, because of their high energy, appear to be one of the significant (and until recently little studied) factors in the dynamics and structure of the interstellar medium, important for the structure and evolution of the galaxy. Recent studies by Spitzer, Field, and others indicate that low-energy cosmic rays may determine the thermal properties of the interstellar medium. The dynamical effects of cosmic-ray pressure are important in the equilibrium of the interstellar gas and magnetic field. Detailed understanding depends on the as yet unknown low-energy cosmic-ray spectrum in the galaxy.

Observation of the characteristics of the interaction of a planet with the solar wind should give information of interest to planetary exospheres and magnetic fields.

The study of particles and fields is important in solar astronomy; the solar wind is the tenuous outer portion of the solar corona. Insights into the solar-flare phenomenon are provided by measurements of solar cosmic rays accelerated during flares, it is expected that correlated studies of solar cosmic rays, the solar wind, and the sun will provide further valuable information.

C. THE PRESENT STATUS OF PARTICLES-AND-FIELDS ASTRONOMY

Most of the observations in particles-and-fields astronomy come from balloons high in the atmosphere or from space probes. Observations carried out on the ground can be used effectively but are indirect, if relatively inexpensive, ways of obtaining information better obtained by direct measurement.

Modern observations in particles-and-fields astronomy began in the early 1960's. Before satellite and space-probe measurements, the very existence of the solar wind was doubted by some astronomers, the cosmic-ray nucleon spectrum was unknown below some hundreds of MeV, and cosmic-ray electrons had not been discovered. High-altitude balloon flights have since been important in extending our knowledge of cosmic rays, but satellite and space-probe experiments were necessary to measure the properties of the solar wind and of the lowest-energy cosmic rays. Since 1960, many of the gross, average properties of various parameters have been obtained. We have reasonably good energy spectra and fluxes at earth for the dominant constituents of cosmic rays, in the energy range from roughly 10 MeV/nucleon to several GeV/nucleon and some knowledge of long-term variations during part of the 11-year solar cycle. Similarly, the average solar-wind velocity, density, temperature, and magnetic-field vector at the orbit of earth during these years are reasonably well known. Short-term, transient phenomena such as shock fronts and Forbush decreases have been studied. Big gaps in our knowledge of quantities that are averaged over short-term fluctuations at the orbit of earth still remain. There are little data available about the interplanetary particles between solar-wind energies (5 keV) and cosmic-ray energies (1 MeV). The composition of the solar wind is still not well known. In addition, the isotopic composition of cosmic rays at high charge values is not known. Finally, the spectrum of cosmic rays at energies in excess of roughly 10 GeV is still somewhat in doubt; more precise measurements of composition and other parameters would be useful.

However, it should be noted that the nature of particles-and-fields parameters is that the observed quantities vary, sometimes sharply, as a function of position or time. For example, the particle distribution function in the solar wind at 5 A.U. or $^1/_5$ A.U. is probably much different from the value at 1 A.U. The density of the solar-wind plasma falls off as the square of the distance to the sun. Various wave phenomena generated near the sun may dissipate their energy and disappear far from the sun. The solar wind must eventually make a transition to the interstellar medium, whose properties must change drastically. Measurement of the properties of the plasma at this transition would add greatly to our understanding of both the interplanetary *and* interstellar media. Present theory indicates that this transition begins within some 20 A.U. from the sun. Recent observations of Lyman-α suggest that important changes may occur even somewhat closer to the sun.

Similarly, the energy spectrum and composition of cosmic rays probably varies quite rapidly with heliocentric radius, as well as with the solar cycle. Nor can the probability that these quantities vary with heliocentric latitude be neglected. The solar wind, by way of the interaction of cosmic rays with the interplanetary magnetic field, tends to sweep cosmic rays out of the inner solar system. As a result, the intensity of cosmic rays with energies less than 100 MeV is much lower at earth than in interstellar space. The most accurate measurements of the cosmic-ray gradient were carried out on the Mariner 4 probe to Mars by comparing intensity measurements at 1 A.U. and 1.5 A.U. The results of different experimenters on this flight are in conflict, because of the difficulty of measurement within the weight limitations of Mariner 4. Confirmation of and delineation of "radial gradient" of cosmic rays is an important goal of particles-and-fields astronomy. Beyond this essentially local problem, the cosmic-ray intensity needs to be measured over a broad energy range, at large heliocentric radii. At earth, the *galactic* energy spectrum and composition at energies less than roughly 100 MeV are unmeasurable. At distances of more than 10 A.U. these parameters should be essentially undistorted by the solar wind. Knowledge of the "galactic" cosmic-ray spectrum and composition down to low energies where much of its total energy may be found will add to our knowledge of the dynamics and evolution of the galaxy. Observations of any possible net cosmic-ray anisotropy would also be of great value.

Another significant aspect is the small-scale fluctuations in all quantities as a consequence of solar-wind turbulence. These small-scale fluctuations do not imply that the dynamical processes are necessarily the same as in ordinary hydrodynamical turbulence. The *average* properties of the solar wind may be profoundly influenced by the effect of fluctuations.

Most discoveries and results have been extracted from the data now available. With important new inputs from recent probes, investigators can continue to obtain useful results from the National Data Bank, set up by NASA, which should be an increasingly useful facility.

D. FURTHER OBSERVATIONAL PROGRAMS

Particles-and-fields astronomy has had success in opening an essentially new field of astronomy during the past decade. Currently, the two problems in particles-and-fields astronomy that should continue to be actively pursued are (a) the study of cosmic-ray composition, energy spectra, and spatiotemporal variations and (b) the study of the plasma-dynamical processes (waves, fluctuations, turbulence) and the composition of the solar-wind plasma. A program of opportunities for experimenters to continue working is needed if progress is to be maintained in this field. The present outlook is extremely bleak, however, from the point of view of particles and fields. The only opportunities are the deep-space probes Pioneers F and G, which will fly by Jupiter. The German–American HELIOS offers selected investigators experimental opportunities. Also, the High Energy Astronomical Observatory (HEAO) would enable measurements of the composition and spectra of high-energy cosmic rays outside of the earth's atmosphere. With these few exceptions, particles and fields have no scheduled flight opportunities. There have been no solicitations for Interplanetary Monitoring Probe (IMP) experiments since 1966. The proposed Venus–Mercury flyby will be oriented toward planetary measurements, not particles and fields.

To maintain progress in particles and fields, the Panel *strongly recommends* that the following minimum programs be pursued:

1. A deep-space probe to a distance of the order of 30 A.U. The technology for this is available, and it would be one of the most dramatic and meaningful programs in space astronomy. As mentioned above, it would study galactic cosmic rays effectively unmodulated by the solar wind. The measurements of spectrum, composition, and any net flow of cosmic rays would yield important parameters for the study of the dynamics and evolution of the galaxy. It is quite likely that most of these results cannot be obtained in any other way. At 30 A.U. the solar wind is probably merging into the interstellar gas, and the possible inferences concerning the interstellar medium would be of considerable interest to many fields of astronomy.

2. The Pioneer F and G Jupiter flyby and HEAO programs should be carried through, with good opportunities for cosmic-ray experiments on HEAO and provision for a full array of particles-and-fields experiments on the flybys.

3. A series of high-bit-rate interplanetary monitors. These would be designed to give high time-resolution studies of the solar wind to permit detailed studies of fluctuations and turbulence, in addition to providing opportunities for deepening our knowledge of the cosmic-ray spectrum and composition.

VI. ULTRAVIOLET ASTRONOMY

A. INTRODUCTION

During the last 15 years, considerable effort has been made to observe celestial objects in the ultraviolet wavelength region (~3000–900 Å) from above the earth's atmosphere, primarily using sounding rockets equipped with small photometers. Not only did the appropriate instrumentation have to be developed, but the vehicles themselves, as well as their stabilization and pointing systems, had to be designed and built. These efforts led to the development of, among others, the widely used Aerobee-Hi sounding rocket and, within the past few years, of an attitude-control system capable of pointing an Aerobee at a series of preprogrammed points in the sky and holding it stable on each to within a few seconds of arc. Most recently, the successful launch of the long-lived Orbiting Astronomical Observatory-2 (OAO-2) opened a new era in stellar space astronomy by providing the first real optical observatory in space.

B. RECENT DISCOVERIES

Although uv rocket astronomy has been limited to naked-eye objects and OAO-2 typically reaches only a few magnitudes fainter, several new results have already been obtained. Ultraviolet resonance lines in early-type giants show pronounced P Cygni-type profiles and indicate that matter streams out from these stars with velocities on the order of 10^3 km/sec, well beyond the escape velocity. This mass loss, estimated to amount to about 10^{-7} solar mass/yr, is of considerable interest for understanding the atmospheric structure of such stars.

The uv interstellar extinction curve is completely different from that predicted on the basis of observations in the visual region. The uv curve is generally characterized by a prominent absorption feature at about λ2200 and a rapid increase in the extinction in the far uv. It is difficult to account for these observations with only one kind of grain or with one particle size distribution; interstellar dust may be more complex than heretofore realized. Furthermore, significant variations appear in the character of both the strong absorption feature at ~λ2200 and in the extinction rise toward shorter wavelengths, suggesting that stars appreciably modify the surrounding medium. Interstellar absorption by Lyman-α has been observed and compared with neutral hydrogen measured by radio astronomers. Several reflection nebulae have been found to be quite bright in the uv. These uv observations of scattering and extinction bear fundamentally on the nature of the interstellar grains and on the physical properties of the clouds in which stars are born.

OAO observations have shown that radiation from most galaxies becomes "bluer" at progressively shorter wavelengths in the uv. For example, around λ3000 the "color" of the radiation from M31 corresponds to that from a G8 star; by λ2000, however, the color corresponds to that of an early F-type star.

Large hydrogen clouds surrounding the recent bright comets Tago-Sato-Kosaka and Bennett have been detected in Lyman-α. These enormous halos should perhaps be considered as a fourth structural component of a comet. Moreover, it was possible to carry out very-high-resolution spectroscopic observations with the OAO, which yielded a temperature of the hydrogen cloud of about 1600 K and a thermospheric temperature of the earth of about 1000 K.

The usefulness of an observatory continuously operating in space was again demonstrated when the OAO was able to observe the outburst of Nova Serpentis. Photometric and spectrophotometric data were obtained for two months immediately following the outburst. Light curves in the uv of several variable and binary stars have been obtained more quickly from OAO than is generally possible from the ground, where, for example, day/night cycles and clouds often present severe observational limitations. OAO has also discovered a broad absorption feature at λ2550 in the spectrum of Mars which is possibly due to ozone.

Space observations complement those made from the ground in an obvious and direct fashion. A persistent problem in astrophysics has been the determination of the stellar effective temperature scale. This scale is poorly known for early-type stars because most of their radiation is in the far uv, unobservable from the ground. Direct observations of their uv flux will enable a reliable temperature scale to be established for these stars. This is of importance not only in the theory of stellar atmospheres but also in the correlation of real stars with model stellar interiors. Similarly, uv observations should allow good determinations of surface gravities of early-type stars to be made through comparisons with model atmospheres. Reliable bolometric magnitudes of hot stars are needed to determine the interstellar radiation field. Many astrophysically important elements have their resonance lines in the uv; observations of their equivalent widths and profiles will enable more precise determinations of their abundances to be made. The study of chromospheric activity in late-type giants and supergiants is under way now through observations of the Mg I doublet at 2800 Å. In short, nearly any field of astrophysics can benefit from the extension of observational data into the uv.

C. ULTRAVIOLET SURVEYS

Surveys in a new spectral region can be made in two ways: an all-sky survey to some limiting magnitude that might lead to the discovery of previously unobserved objects radiating primarily in that wavelength region or observations of a sample of known objects to measure their characteristics in the new spectral region. Presently, in uv astronomy the data are more complete in the second kind of survey than in the first.

The early rocket work was performed with only spin-stabilized vehicles whose precessional motions swept small telescopes over a portion of the sky. Telescope apertures were generally from 1 to 6 in., and observing times over their fields of view were typically 0.1 or 0.2 sec. Early-type stars no fainter than about fifth magnitude visual could be observed in the uv; stars later than middle A-types could not be seen at all with these systems. Thus most objects observed were within a few hundred parsecs of the sun.

A more systematic survey was undertaken by the Smithsonian Astrophysical Observatory (SAO) with their instruments on OAO-2. Four 12-in.-aperture Schwarzschild-type telescopes with 2° fields of view had as detectors Vidicon-type television tubes sensitive to broad spectral bands in the uv. With this system, early-type stars to about eighth visual magnitude could be reached with a spatial resolution of 1 to 2 min of arc. When the SAO ceased its OAO operations, it had observed about 15 percent of the Milky Way.

The Wisconsin instrumentation on OAO-2 makes photoelectric observations of objects already known and was not designed for all-sky surveys. Thus no deep surveys capable of detecting peculiar uv objects have been carried out, nor are any definitely planned for the near future. Schmidt-type wide-angle cameras sensitive in the uv have been developed but as of this writing are not yet scheduled to fly. Although no uv objects have been found that are not detectable in the visible, a number of unusual uv/visual intensity ratios have been found in galaxies and in a nova.

A survey in the sense of uv filter observations of a sample of known stellar objects is well under way, however. Rocket observations in various wavelength bands have been made for perhaps 100 early-type stars, but few of these objects are well observed over the whole uv, and combining several different observations of the same star is often difficult. Wisconsin OAO telescopes have so far observed several hundred stars scattered over most of the Hertzsprung-Russell diagram. This filter photometry, made with 300-Å bandpasses centered from 1200 Å to 4200 Å, is carried out with 8-in. telescopes with up to 64-sec integration times. This work has resulted in a large body of homogeneous filter photometry in the uv for a representative sample of stars. Some rocket scanner observations and especially OAO observations have been made of about 150 stars with 10 to 20 Å resolution in the 1000–3000 Å region. Sounding rockets have been used to obtain observations of approximately 20 stars with spectral resolutions of about 1 Å, primarily in the region shortward of 2000 Å. Princeton is providing instrumentation for the last OAO now scheduled. It consists of a 32-in.-aperture telescope with a spectrum scanner capable of resolutions of 0.1 to 0.4 Å. This instrument will be used to measure interstellar and stellar spectral line profiles. Thus within the next few years a considerable amount of spectrophotometric data on the brighter stars and interstellar matter should be collected and analyzed.

It is only with the OAO series of satellites that large amounts of uv data are available. Because of various operational limitations, primarily the restriction to nighttime observing, OAO-2 collects data during only about 25 percent of each orbit. This fraction may increase markedly with OAO-C because its sunshade might allow considerable daytime observing. A relatively modest guest observer program has been under way with OAO-2, involving about ten groups of astronomers and 100 or so objects. With its greater observing capability, OAO-C should be able to carry out such programs more efficiently. These programs provide useful experience for both NASA and astronomers, leading toward a national space observatory that would be used primarily by guest observers.

D. FUTURE PROGRAMS

The major uv astronomy program should be the development of a new satellite series that would be national space observatories and that would lead, through a series of intermediate steps, to a very large diffraction-limited telescope capable of operating in the near-infrared and visual regions as well as in the uv. Such an instrument could attack problems that are in all likelihood simply insoluble from the ground; it possibly also represents the ultimate instrument in the "optical" region of the spectrum. For example, it should be able to observe stars and star-like objects at nearly ten times the distance obtainable with the 200-in. telescope and achieve about 100 times its angular information density. The power of such a telescope is described in detail in the report *Scientific Uses of the Large Space Telescope.** Furthermore, the first step toward this instrument, e.g., a 60-in. non-diffraction-limited telescope (which is technically feasible now) could have a versatility and capability far exceeding any astronomical satellite now under consideration. It could be equipped, e.g., with four auxiliary instruments: one high-dispersion ($\Delta\lambda/\lambda \approx 2 \times 10^{-5}$) spectrometer, one low-dispersion ($\Delta\lambda/\lambda \approx 2 \times 10^{-3}$) scanner, a direct imaging system, and perhaps a filter polarimeter or area scanner. Such a satellite constructed in a modular fashion, enabling a man to replace a defective module or substitute one instrument for another, could have a very long life. Finally, it seems possible that the spacecraft for such a series could be used as stable platforms for any instruments requiring accurate pointing, a controlled environment, and a sophisticated command and data-handling system, such as an advanced x-ray imaging telescope, a far-ir telescope, and possibly a large solar telescope. Such a system would result in a considerable cost saving and increase in reliability for many programs.

Even if the Large Space Telescope series were undertaken soon, there would still be a considerable gap between OAO-C and the launching of the first satellite of the new series. To fill this gap, to provide continuity in uv astronomy, it is important to begin work on an intermediate-size satellite, of the OAO or SAS class, equipped with a medium- to high-resolution spectrograph with which earlier OAO discoveries could be exploited and extended. Such a satellite should be the first national, or possibly even international, observatory and would provide useful experience in operating this kind of facility; SAS-D, or as it is sometimes called, IUE, may fill this requirement.

It is important that NASA expand its support of the sounding rocket program in uv astronomy. Rockets are exceedingly useful for exploratory work and for developing new techniques, as well as for gathering important astrophysical data. Such programs also give astronomers first-

*Ad Hoc Committee on the Large Space Telescope, Space Science Board, *Scientific Uses of the Large Space Telescope* (National Academy of Sciences, Washington, D.C., 1969).

hand experience with the problems and potentials of uv astronomy. Something of the same kind of opportunity may be afforded by the Skylab program. Experiments with uv-sensitive wide-angle cameras capable of reaching, say, 17th magnitude visual, may be interesting, since no such relatively deep survey of even a limited portion of the sky is otherwise planned. Additionally, this has the virtue of testing man's ability to perform astronomical tasks in space.

Thus, an ideal program in uv space astronomy would include a vigorous sounding-rocket program, a replacement for OAO-B, a follow-on OAO or equivalent satellite to exploit OAO discoveries and provide continuity in research opportunities and experience in operating a national space facility, and the beginning of a new program to develop a series of satellites that would lead, through possibly several intermediate-size telescopes, to a large, diffraction-limited instrument.

VII. RADIO ASTRONOMY

A. INTRODUCTION

The contributions made by radio astronomy over the past two decades have revolutionized our understanding of the universe. Within the solar system, we have learned much about the workings of the active sun, the atmosphere of Venus, the complicated magnetosphere of Jupiter, and the large-scale structure of the solar wind.

The classical optical telescope can "see" only a short distance into the Milky Way, the plane of the giant disk-shaped galaxy to which the sun belongs, before gas and dust completely obscure what is beyond. Radio telescopes, however, can "see" through the Milky Way into the nuclear regions of our galaxy and beyond. Radio emissions from hydrogen gas, the most abundant material in the universe, have allowed astronomers to map out the giant spiral structure of our galaxy. Radio emissions also permit the study of regions of star formation and remnants of star destruction.

The discovery of intense radio sources–quasars and radio galaxies, all at immense distances–have revealed the existence of violent events in the early history of our universe. The release of energy equivalent to the total annihilation of millions of solar masses is required from these fantastic objects to account for the faint radio emission we see today, billions of years later. The nature of these violent events and their cosmological significance presents one of the biggest mysteries in astronomy today.

Observations of greater sensitivity, better angular resolution, and made over a wider wavelength range are now required to elucidate the many mysteries already discovered. Most of the new instruments required to obtain greater observing power will be built on the earth's surface. There are, however, at least two important areas where we now recognize the need for instruments in space: at long wavelengths, where the earth's ionosphere is opaque, and a terminal for very-long-baseline interferometer (VLBI) observations to obtain very high angular resolutions ($\sim 10^{-4}$ sec of arc).

B. LONG-WAVELENGTH RADIO ASTRONOMY

The long-wavelength limit of the terrestrial radio window as used by ground-based observers is imposed by the upper (F-region) ionosphere. Observations at wavelengths longer than about 30 m are severely hampered by refraction and scintillation in the earth's ionosphere. In order to extend observations to at least 1 MHz (300 m), it is necessary to escape the effects of the ionosphere and therefore to place radio telescopes in high (several thousand kilometers) earth orbit. The ionosphere, in addition to excluding celestial radio waves from the earth's surface, also shields the high-orbit radio telescope from the din of man-made and natural interference.

C. PROGRESS IN THE 1960's

During the past decade, there have been several dozen instruments flown on rockets and satellites by a half dozen groups in the United States, Canada, the United Kingdom, France, and the Soviet Union. The scientific results of these early observations are substantial, despite their exploratory nature.

Because of the long wavelengths involved and the consequent large structures required, most observations to date have been made with nondirective sensors, hardly worthy of the term "telescope." At a frequency of 1 MHz, for example, the wavelength is 300 m, so that in order to obtain the very moderate resolution of 1 deg of arc, a structure $\sim$20 km on a side would be required. Clearly, this was beyond the resources of the space program in the 1960's.

Early attempts in the United States and the United Kingdom to obtain reasonable angular resolution ($\sim$5–10 deg of arc) using various forms of ionospheric focusing have all failed. These failures have come about primarily because of the unanticipated presence of high ambient noise levels in the ionospheric plasma and the (related) problem of the complicated behavior of an electric dipole antenna in a magnetoactive plasma. Although many of the satellite and rocket programs developed to exploit ionospheric focusing failed to produce observations of interest for astronomy, they provided a new understanding of ionospheric radiophysics.

The early rocket, satellite, and deep-space-probe observations made with nondirective instruments produced considerable data in important areas. Primary among these are the spectrum of the cosmic noise background at long wavelengths, dynamic spectral observations of outbursts from the sun, and nonthermal radio emission from the terrestrial ionosphere and magnetosphere.

The first satellite program dedicated solely to long-wavelength radio astronomy is the Radio Astronomy Explorer (RAE) program at the Goddard Space Flight Center. The first satellite of this series, the RAE-I, was launched in 1968 and operated successfully until it was turned off in July 1972. This satellite represented the first crude attempt to place directive antennas in space (~20–100 deg of arc). The RAE-I has produced additional data in the cosmic noise background spectrum over various broad regions of the sky, has observed new and interesting solar radio-burst phenomena, and has obtained voluminous data on terrestrial radio emission. The second Radio Astronomy Explorer satellite (RAE-B) will be placed in lunar orbit in 1973.

Observational results from the RAE-I and previous rocket and satellite programs are spawning many interesting and important new models of the plasma, fields, and particles components of the interstellar medium. Observations of solar disturbances, some of which have been observed to propagate out through the solar corona to the distance of Mercury's orbit, are providing new insights into the structure of the corona. Maps of the cosmic noise background are being produced from the RAE-I data, which are of interest in problems of galactic structure.

D. AN OBSERVATIONAL PROGRAM FOR THE 1970's

Observations of very low directivity can extend just so far. Further progress in long-wavelength radio astronomy demands better angular resolution. The angular resolution that can be obtained from observations made within the inner solar system will be limited by propagation conditions in the interplanetary medium. In particular, the scattering of radio waves from distant sources by electron density irregularities limits the usable angular resolution. Observations at shorter wavelengths indicate that the limit imposed by interplanetary scintillations is about 1 deg of arc at 1 MHz. Scintillation effects scale as the square of the wavelength, so that at 2 MHz useful resolution is about ¼ deg of arc.

Angular resolutions of the order of a few degrees of arc are the minimum required for the study of discrete radio sources. The discrete sources are of major astrophysical significance, and observations at long wavelengths are important for their understanding. The vast majority of nonthermal radio sources have relatively uninteresting power law spectra over the fraction of the radio spectrum observable from the earth's surface. The revealing effects of self-absorption, ambient plasma absorption, and the Tsytovitch-Razin effect, for example, should occur for most sources between 20 MHz and 1 MHz. This portion of the radio spectrum is most sensitive to plasma, field, and particle distributions within the source.

The recent observations of superdense stars with high magnetic fields (pulsars and certain white dwarfs) suggest that highly nonthermal radio emission generated by some coherent mechanism may be emitted by such sources. The spectra of a coherent source may be very steep and highly peaked toward long wavelengths. An example of such a source is the Crab nebula compact source now recognized as the Crab pulsar NP 0532+21.

The most important future need of long-wavelength radio astronomy in space is for a telescope system that has a 1–2-deg of arc pencil beam at 1 MHz, operates over a frequency range from a few hundred kHz to 10 MHz, and is capable of providing nearly complete sky coverage. The primary aim of this telescope would be to extend the spectra of discrete sources to long wavelengths. The cosmic noise background should also be mapped with such an instrument to better determine the distribution of absorbing ionized matter and emitting fields and particles in the interstellar medium. A filled-aperture telescope would be valuable in observing time-varying solar and planetary radio emission, but a filled aperture is not needed to provide the resolution specified above for the galactic observing programs.

In addition to extending the spectra of known objects into the long-wavelength region, a 1-deg of arc telescope should also be used to systematically survey the sky for anticipated sources of radio emission. The opening of new wavelength ranges has always led to the discovery of new and unexpected phenomena.

The minimum program in long-wavelength radio astronomy in space in the 1970's should include the following:

(a) Explorer-sized spacecraft for interferometric observations primarily of the sun but also of Jupiter and the other planets. A modification of the above interferometer system could be used to make a preliminary survey of time-stationary discrete sources and background radiation using the techniques of aperture synthesis.

Which, if the earlier results so justify, should lead to:

(b) A telescope capable of producing a 1–2-deg of arc pencil beam at 1 MHz and operation over the range of a few hundred kHz to 10 MHz. This telescope should be used to systematically survey the entire sky as well as to monitor known sources. At a frequency of 1 MHz (λ = 300 m) a structure approximately 20 km in extent is required to obtain a 1-deg of arc beam. The very large physical structures required present the greatest technological and economical constraints on such telescopes. Receiver electronics and data-handling problems are routine.

E. VERY-LONG-BASELINE INTERFEROMETRY

The conventional radio-astronomy version of the Michelson interferometer uses a frequency (phase) and time reference that is common to all elements. A direct cable or microwave link must therefore be established to interconnect all the antennas in the system. However, direct links between terminals of an interferometer become more and more impractical as baselines are lengthened to obtain better angular resolution.

The technique of constructing radio interferometers using independent (noninterconnected) terminals was developed and used to overcome these limitations by groups in Canada and the United States. The very-long-baseline interferometer (VLBI) technique makes use of the very precise time and frequency standards that are now available. Independent, but very stable, frequency standards are used to control the local oscillators at each terminal. The predetection output of the radio telescope is then recorded on magnetic tape along with accurate epoch and time signals. A series of portable terminals have been developed in Canada, the United States, Australia, Sweden, and the Soviet Union, which can be transported to radio telescopes around the world. According to a prearranged schedule, a series of radio sources are observed simultaneously at each observatory participating in that observing program. The magnetic tapes are then gathered together and the tape-recorded predetection signals from various terminal combinations are correlated. The VLBI technique has been successfully applied at λ3 cm over baseline lengths near an earth radius. Angular resolution approaching 0.001 sec of arc is thus practical on the earth's surface. We still expect many sources to be unresolved at this angular resolution. Rapid variations in the brightness or separation of features in quasars and in a radio galaxy have been observed.

To further advance our knowledge of the violent events responsible for intense compact sources in galactic nuclei and quasars, angular resolutions of order 0.0001 sec of arc will be needed. Many problems in galactic structure, such as star formation, also require the use of angular resolutions of at least 10^{-4} sec of arc. The interferometer baseline lengths necessary at centimeter wavelengths to obtain 10^{-4} sec of arc are larger than the diameter of the earth.

Interstellar scintillations have been observed in the radiation received from pulsars. We lack, however, a good theory of scintillations that is applicable to the interstellar medium. We also have no *a priori* knowledge of the spectrum of electron density fluctuations that would be responsible for scintillations. Current results, however, indicate no serious limitations except for pulsars or other possible coherent sources with angular sizes $\lesssim 10^{-6}$ sec of arc at decimeter and centimeter wavelengths.

A natural extension of the network of earth-based VLBI terminals would be the development of a satellite terminal that could be placed in orbit around the earth. It is desirable to have such a satellite terminal operate over a wide wavelength range to study as wide a range of problems as possible. In order to overcome limitations imposed by interplanetary scintillations, operation at wavelengths of 3 cm and shorter is necessary. The aperture for such a telescope should probably be 5 to 20 m. The orbit of a VLBI space terminal would be eccentric to provide a wide range of vector baselines with existing ground-based terminals. In addition to pointable aperture, a fair amount of highly reliable time standards and electronics would be required.

A feasibility study of the VLBI space terminal for radio astronomy should be undertaken as soon as possible for future planning in this area.

VIII. SOLAR SPACE ASTRONOMY

A. THE PRESENT STATUS OF SOLAR SPACE ASTRONOMY

A preliminary survey of the electromagnetic spectrum of the sun from the near ultraviolet to the hardest x rays and gamma rays has been completed as a result of the efforts of numerous investigators during the past decade. We now know, in a rough sort of way, what the spectrum of the sun looks like and how it varies in time and with the solar activity cycle. Very briefly, the results have shown that the quiet sun emits radiation down to about 100 Å, active regions over sunspots radiate energy down to perhaps 10 Å, and when these active regions produce a flare the spectrum below 10 Å is enhanced by several orders of magnitude in intensity and extended into the hard x-ray region.

The solar spectrum has now been explored with sufficient spectral resolution to identify over a thousand emission lines and to establish at least relative intensities for many of the stronger lines. However, due to instrumental limitations, primarily the length and weight of a spectrograph that can be carried on present space vehicles, it has not been possible to obtain profiles for emission lines below 1200 Å. Line profiles contain an enormous quantity of information regarding the gradients of temperature, density, velocity, and magnetic field through the atmosphere.

Images of the sun have been obtained in relatively narrow bands of energy from 1300 Å on down below 8 Å. These images are important in delineating how the temperature and density of the largest structures within the atmosphere of the sun vary with depth, position, and time. The angular resolution attainable on the Orbiting Solar Observatory (OSO) vehicles has so far been insufficient to resolve smaller-scale structures in the solar atmosphere, which are known from observations in the visible portion of the spectrum to be the sites of important activity.

During a flare, the solar x-ray spectrum increases markedly in intensity and is extended into the hard x-ray region. Rapid progress in the study of solar flares has resulted from the use of spectrometers and proportional counters borne in space. However, partly because of the limitations of data storage and telemetry capabilities of existing vehicles, it has not been possible to obtain the very high time resolution that is required for an adequate description of dynamic spectra in flares. The next generation of vehicles must provide this necessary time resolution. One of the overwhelming advantages of a satellite is its capability for monitoring the fluctuations in the electromagnetic spectrum of the sun independently of weather on the ground. A long series of monitoring experiments have been carried on OSO's and Solar Radiation Satellites, which have provided crude, but nevertheless very valuable, information on the overall fluctuations of the solar spectrum.

It may appear from the foregoing statements that observations from space vehicles will eventually completely supplant ground observations. This is unlikely to be true—ground observations and space observations are complementary to one another. At the present time, and for the foreseeable future, ground observations will retain the advantages of relatively low cost and the possibility of obtaining high spatial and spectral resolution with very large equipment that could not be carried in space.

During the past decade, solar space astronomy has received generous financial support. A series of six orbiting solar observatories has been launched and a flight for ATM-A (Skylab) approved. One of the questions facing this Panel, and the Astronomy Survey Committee as a whole, is the extent to which solar space astronomy shall be supported in the 1970's. A prime criterion in making this decision must be the degree to which solar space research complements the efforts in other subdisciplines in astronomy.

Research on the sun supplies information crucial to the interpretation of stellar and galactic phenomena. The history of solar research shows that concepts are developed, methods tested, and phenomena discovered that have a direct bearing on stellar and galactic astronomy.

Stellar chromospheres (and coronas) owe their existence to the mechanical energy flux that originates in their hydrogen convection zones. This energy maintains the radiative and conductive losses of the outer stellar atmosphere. Thus, a close correlation is to be expected between the vigor of a star's convection zone and the strength of its chromosphere, as revealed by its emission lines. Such correlations have, indeed, been discovered from ground-based observations of the K-line of ionized calcium: the line width correlates with absolute magnitude, and the line intensity correlates with stellar age. The study of stellar chromospheres, by means of their euv spectra, should lead, therefore, to important conclusions concerning stellar interior structure and evolution.

In the past ten years, solar studies, from both ground and space experiments, have revolutionized our concepts of the structure and energy balance of the solar chromosphere. In particular, we have recognized that the chromosphere outside active regions is concentrated almost entirely in a network that overlies the supergranulation boundaries, where the photospheric magnetic field is enhanced tenfold. There are indications, still to be explored, that all the chromospheric material resides in the spicules (1-sec of arc fine structures). The average temperature gradient at the chromospheric–coronal interface, determined from space-averaged euv spectra, is larger than expected. As a result, it appears that heat conduction from the corona controls the temperature distribution in the interface. Since a sizable fraction of the energy deposited in the corona appears to return, by heat conduction, to the chromosphere and photosphere, the primary flux of mechanical energy from the convection zone must be correspondingly larger in order to maintain the corona. These solar results will clearly influence the interpretation of stellar euv spectra and will contribute to the explanation of the effects observed.

Some of the most exciting fields of present-day astronomy are concerned with the acceleration of charged particles and the radiation that these particles emit while interacting with thermal plasmas and magnetic fields. Synchrotron radiation from the Crab nebula, x-ray bursts from the Scorpio X-1 source, and nonthermal radio emission from galactic sources are but three examples of the prevalence of these processes.

Observations of solar flares in the xuv and euv spectral regions offer us the unique opportunity to study these nonthermal phenomena at close range, with higher spatial, time, and spectral resolution than is possible for most galactic sources. Moreover, we can follow the subsequent thermalization of accelerated particles by observing the solar xuv line radiation, which is highly temperature-dependent. In this way, the solar observations can contribute to the study of high-energy phenomena of wide astronomical interest.

In the past decade, xuv observations from rockets and OSO's have begun to yield data of this kind. Nonthermal x-ray bursts were observed in the early phases of flares from OSO-3 (with photon energy between 2 and 20 keV) and OSO-5 (20 to 200 keV). These bursts are associated with (and presumably are produced by the same electrons as) type III radio bursts.

Following the initial nonthermal phase, the flare temperature rises rapidly. Xuv lines attributed to heliumlike calcium and iron have been observed from OSO-6 and -3 that are consistent with the x-ray continuum and suggest plasma temperatures in excess of 40 million degrees. This value exceeds, by a factor of 10, temperatures derived from ground-based observations.

The energy released as xuv and particle emission is thought to originate in the annihilation or distortion of magnetic fields in the corona. No direct method of measuring coronal magnetic fields has yet been devised, however. Therefore, it was of extreme interest when, on June 8, 1968, a rocketborne x-ray telescope obtained solar images with 5-sec of arc resolution that showed looplike coronal structures that connect neighboring active regions. These observations followed a small flare and suggest that the flare instability is, indeed, related to the configuration of coronal magnetic fields. Such x-ray images emphasize, as never before, that the fundamental character of these regions lies in their fine structure and gives strong impetus to observing the corona on a continuing basis with higher spatial resolution.

Nonflaring active coronal regions have been observed from rockets in the density-sensitive forbidden transitions of heliumlike ions. These lines lie in the 6–26-Å band for ions of O III through Si XIII. The data show that coronal knots having densities 10 to 100 times the normal value and diameters of 5 sec of arc) exist prior to a flare. Are these knots the site of the flare instability? Further observations with high spatial resolution are needed to answer this question.

B. OBSERVATIONAL PROGRAMS FOR THE 1970's–PROGRAMS

1. The Chromosphere

Models of the average chromosphere in quiet and active areas on the sun have been derived from euv line fluxes, observed with 1 min of arc resolution from OSO-4 and -6. These models give only lower limits to the temperature gradient (hence the conductive flux) in the transition to the corona. In order to determine observationally the energy balance of the chromosphere (including radiative, conductive, and nonthermal terms), and to correlate local variations with the photospheric magnetic field, the supergranulation boundaries must be studied with 5-sec of arc resolution or better. Observations with such resolution in coronal lines will permit limits to be set on the coronal magnetic field.

Since mass transfer to the corona probably occurs in spicules, 1-sec of arc observations in the euv (with spectral resolution of 10^4) is necessary to investigate the mass-balance of the chromosphere and corona.

Energy is probably supplied to the corona in some form of wave or shock motion. To determine the character of these waves, we need time-resolved velocity measurements in the transition region both in quiet and active regions. A combination of moderate resolution in all three coordinates is required: angle (5 sec of arc), wavelength (1 part in 30,000), and time (30 sec).

The depth variation of the chromospheric temperature and density could be explored further with line profiles if a spectral resolving power of 10^5 were available.

2. Coronal Active Regions

We know from optical observations and from a few unique xuv observations that loopy or knotted structures with sizes below the resolution of current OSO experiments and with very high density exist prior to, and after, coronal flares. We do not know their relation to the coronal magnetic field or whether they are the sites of the flare instability. Broadband xuv images with at least 5-sec of arc, and preferably 1-sec of arc resolution will be needed to correlate these coronal structures over the disk with magnetic-field measurements in the underlying photosphere and to follow their evolution at the time of a flare. With such observations it will be possible to study the flow of coronal material after a flare (i.e., the formation of loop prominences and coronal downfalling "rain").

In the presence of strong temperature and magnetic-field gradients, heavy elements in coronal knots may be stratified by diffusive or gravitational forces. As a result, the chemical composition may vary significantly over small spatial scales within an active region. If so, the solar wind may fluctuate in composition as the trapped material gradually emerges. Xuv spectra of moderate resolving power (10^3) but high angular resolution (<5 sec of arc) are required to investigate this question.

3. *Flares*

Time-resolved xuv spectra have revealed a wealth of new information regarding the processes by which electrons are accelerated and thermalized during a flare. We are only beginning to acquire the data necessary to study these phenomena, however. Because coronal flares have been shown to possess fine structures, we must improve the angular resolution of present images and spectrometer scans to better than 5 sec of arc, and preferably 1 sec of arc, in order to determine the true nature of the flare instability. Spectral resolving power must be improved to 10^4 with concurrent spatial resolution of 10 sec of arc in order to separate blends of K-shell and hydrogenic emission lines of highly ionized iron atoms in the 1 to 2 Å range. These lines are the most sensitive indicators of the temperatures above 10^7 K generated in the flash phase. Present observations of x-ray bursts in the 20- to 200-keV energy range are hampered by the restricted temporal resolution (2 sec) of the instrumentation. Pulse time profiles with 0.1-sec resolution are required to study the degeneration process of fast electrons and to correlate x-ray and microwave bursts.

Polarimetry of hard x-ray bursts at several wavelengths in the continuum between 0.1 and 5 Å can be used to study the collimation of fast electron beams in flares. The first preliminary experiments, with 30-min of arc spatial resolution, have proved successful. Coronal magnetic fields could be measured directly from the polarization of xuv emission lines. Much development is required here, but the rewards, in terms of advances in understanding flares, would be enormous.

C. OBSERVATIONAL PROGRAMS FOR THE 1970's–EQUIPMENT

The Solar Astronomy Panel report discusses recommendations for solar space experiments. It recommends, as part of its minimum program in the 1970's, an improved OSO series through OSO-L, -M, and -N, and a strong rocket program, as the backbone of the solar space effort. Its optimum program also recommends the start of the design of a heavy-payload 1-sec of arc pointing capability vehicle to be flown during the next solar maximum. We refer to the Solar Astronomy Panel report for details.

IX. PHYSICS OF THE SOLAR SYSTEM: THE CONTRIBUTION OF OBSERVATIONS FROM SPACE

A. INTRODUCTION

The subject matter of solar-system investigations includes the nine major planets, their satellites, the comets, the asteroids, and the interplanetary medium. One would also like to discover whether other systems of planets are associated with stars in our vicinity. Both sets of investigations can be carried out to great advantage from a position outside the earth's atmosphere.

Motivation for these studies lies deeper than a simple desire for additional information. If we are sufficiently careful in our choice of investigations, we may anticipate that we will acquire some insight into problems considerably more subtle and complex than those posed by the immediate subject matter. On the one hand, we will sharpen our ideas about the mode of origin and the manner of evolution of our solar system and, beyond that, about star formation generally. Surely this process has left important evidence in the form of the characteristics of planets that we can now observe. At another extreme in generality, we are aware that life itself is a remarkably subtle aspect of the natural development of the outer layers of the earth and its atmosphere. If we can fully understand the processes in this development, we will be a long way toward understanding the origin and nature of life.

To illustrate the advantages of observations from space for an attack on these problems, we may consider the capabilities of a large telescope in orbit, unhindered by the airglow, molecular absorption, and seeing effects that limit the efficacy of ground-based investigations. Such an instrument would permit a search for trace planetary atmospheric constituents of fundamental importance to questions of the origin and evolution of life and also an evaluation of important major constituents such as helium, whose abundances are critical to an understanding of the origin and evolution of the solar system itself. One could determine the total heat balance of the planets with ease, thereby contributing important information on their interiors and evolutionary history. It would also be possible to evaluate variations in both absorption and emission as a function of position on the planetary disks over a very broad range of wavelengths, thereby gaining information on the scattering properties of the aerosols and hence a better clue to the determination of atomospheric composition.

The spatial resolution obtainable from a large space telescope in the uv would permit a determination of the distribution of temperature in the upper atmospheres of the planets, leading to estimates of atmospheric stability against thermal escape over the lifetime of the solar system. Maintaining the telescope in orbit for long periods of time would permit the monitoring of planets known to exhibit temporal variations in cloud structure or surface albedo. A bright comet would probably be observed, providing the first opportunity of resolving the nucleus, thereby obtaining its diameter and information on composition from infrared spectroscopy.

With a 120-in. telescope with a resolving power of 0.04 sec of arc one could expect to resolve craters on Mercury; to obtain good diameters (and thus densities) for Pluto, Triton, and the satellites of Saturn; and to study some surface detail on the larger asteroids and the Galilean satellites of Jupiter.

Another problem that could be attacked with a Large Space Telescope would be the search for other solar systems. Assuming a separation equal to that of the sun and Jupiter, one could resolve objects of equal brightness out to a distance of 425 light-years. This is obviously an unrealistic figure; the essential difficulty is the low intensity of light from the planet compared with its primary. The best way to compensate for this discrepancy when using direct methods would be to work in the thermal infrared, where the ratio of flux from a planet to that from its star is highest. Other methods may also be employed that would permit detection of an object like Jupiter at low frequencies if a suitably large radio array were available.

B. THE CURRENT STATUS OF OBSERVATIONS FROM SPACE

The opportunity to pursue these investigations unhindered by the presence of the earth's atmosphere and ionosphere has only been exploited in a preliminary way. Below 3000 Å, spectroscopic observations have been made by means of balloons, sounding rockets, spacecraft, and the Orbiting Astronomical Observatories. Above 10,000 Å, observations have been made from high-flying aircraft and spacecraft. While most of these have been made at wavelengths below the radio range, the first successful planetary probe, Mariner II, carried infrared and microwave radiometers. The brighter planets have been studied with varying success to wavelengths near 1200 Å. The data below 2000 Å are particularly uncertain, however, usually consisting only of the measurement of Lyman-α emission. The prospects for extending the wavelength range of low-resolution spectroscopy and for improving the photometric precision of the observations of the brighter planets are poor after the failure of OAO-B.

In the infrared, the only observations of solar-system objects from outside the earth's atmosphere were made from rockets and spacecraft; the most successful of these being the 1969 Mars flybys. On the other hand, numerous successful attempts have been made to minimize the effects of telluric water-vapor absorption observation from high-altitude aircraft. Again the results are restricted to the brighter planets, and the resolution and wavelength coverage leave much to be desired. The need for an orbiting telescope to complete even a preliminary survey of the infrared spectrum seems inescapable.

It should be noted that we still lack observations of asteroids or planetary satellites from space (although the satellites of Mars were detected by the Mariner), we have only rudimentary uv data on comets (almost none in the infrared), and we lack angular resolution, so that it has not been possible to look for variations over the disk of a planet, except from spacecraft. It remains to be seen how much OAO-C will be able to contribute to detecting the Galilean satellites of Jupiter and partially resolving the Jovian disk and distinguishing the rings of Saturn. In the infrared and low-frequency radio regions, there are presently no prospects for high-angular-resolution observations.

C. THE SIGNIFICANCE OF PRESENTLY AVAILABLE OBSERVATIONS

The availability of the uv spectral region has permitted the first detection of electronic spectra of atmospheric constituents. Since the absorption cross sections for uv ground-state electronic transitions are orders of magnitude higher than those for vibration–rotation transitions in the near infrared, this opportunity has permitted an extremely sensitive search for minor constituents in planetary atmospheres. Thus it has been possible to set upper limits of a few parts per million on several possible constituents of the Martian atmosphere, a sensitivity comparable with that of an *in situ* analysis with a mass spectrometer. Similarly, from the detection of weak features in the uv representing only a tiny fraction of the infrared abundance of ammonia in Jupiter, it has become evident that the effective opacity of this planet's atmosphere is strongly wavelength-dependent. One also has the opportunity to observe atomic resonance transitions with high transition probabilities, which are also the most favored transitions, given the low temperatures in planetary atmospheres. In this way, atomic hydrogen has been discovered in the upper atmospheres of Mars, Venus, and Jupiter, as well as in the comas of Comet 1969g (Tago-Sato-Kosaka) and Comet 1969i (Bennett).

Observations of the total brightness of Mars in the region below 3000 Å obtained from a rocket played an important role in the controversy about the surface pressure of the Martian atmosphere. This was related to the method that had been used to obtain the earlier value of 85 mbar from the ground but now applied to a region of the spectrum where Rayleigh scattering from the atmosphere is much stronger. Even assuming no contribution from the planet's surface, it was found that the reflected sunlight from Mars would only permit an atmosphere with a surface pressure less than 25 mbar, and a value less than 10 mbar seemed more likely. These values agreed with new results from ground-based spectroscopy and with verification from the radio occultation of Mariner, which showed that the Martian atmospheric surface pressure was in the 6–9-mbar range or less.

In the infrared, observations from aircraft gave the most definite indication of the presence of water vapor in the atmosphere of Venus currently available. The high-altitude vantage point permits an investigation of water-vapor absorption bands hopelessly blocked by telluric absorptions at ground level.

In much the same way, the best evidence we have that Jupiter and Saturn are radiating more energy than they receive from the sun was obtained from high-altitude flights. Indications that this situation existed had already been derived from ground-based studies, which suggested that the effective temperatures of both planets were higher than the values to be expected from a simple equilibrium with incoming solar radiation. This deduction was based on a knowledge of the planetary albedo and the measurement of the thermal radiation from the planet at those wavelengths not blocked by terrestrial atmospheric absorption. There is obviously some uncertainty in such procedures, since one is not measuring the *total* thermal radiation. A much closer approximation to this ideal is made possible by observations from a high-altitude aircraft, and this work was done at even higher altitudes than the Venus investigations mentioned. Despite the general acceptance of these results, it is still desirable to repeat these experiments with no interference from the atmosphere to eliminate calibration uncertainties.

D. THE RELATIONSHIP TO GROUND-BASED STUDIES

There is a close connection between observations made from space and from the ground. Identification of atmospheric constituents and characteristic parameters from the ground leads to the formulation of models that predict the appearance of the planet in other spectral regions. These models may then be verified by observations from space, which in turn lead to new data that must also be incorporated in the models.

On a somewhat different level, the new observations from an extraterrestrial vantage point may indicate distinct gaps in various areas of knowledge. These in turn may require a redirection of effect and even the development of new facilities in order to keep pace with new information. For example, the absence of an oxygen corona about Mars and Venus has led to a serious re-evaluation of the reaction kinetics in the photolysis of CO_2. Similar problems exist with the interpretation of relative intensities of molecular features in the Martian uv dayglow, indicating a need for additional laboratory and theoretical investigations that are currently under way.

The high-altitude infrared observations have led to the production of "normalized" spectra that have most of the telluric absorptions removed and may be used to eliminate the telluric components effectively from ground-based observations. The detection of unusual solid-state absorptions in some of these spectra (the rings of Saturn, the polar caps of Mars) has led to a large number of laboratory studies of condensates at various temperatures and grain sizes.

E. LOOKING TOWARD THE FUTURE–LOW-LEVEL SUPPORT

In the absence of funding for major new starts, a large amount of valuable research can be carried out with existing and about to be completed facilities. This statement applies to ground-based and laboratory investigations, as well as to programs carried out from space. In the latter category, we have already referred to the NASA airborne infrared telescope, which will soon become operable. It will unquestionably lead to results of great significance and should be utilized accordingly.

There is a definite need for a large telescope in orbit that could be used for infrared observations, but if it appears that this need will not be met, it is by no means necessary to abandon infrared observations as a way of obtaining information about the planets. The high-altitude aircraft and rockets have much to contribute, and even ground-based sites with low humidity should be fully exploited. For example, the remarkably powerful Fourier interferometer spectrometer has still not been used at a suitably dry, high-altitude observatory. The additional information provided by such an effort (presently under way) will be comparable with that obtained by low-resolution instruments taken to higher altitudes by aircraft—and at a fraction of the cost. Concomitant development of infrared-sensitive detectors, spectrometers, and photometers should receive strong support and would require only a low level of funding. The same is true of laboratory investigations of atmospheric constituents and theoretical studies of model atmospheres.

In the ultraviolet, the situation is not so favorable. One simply cannot obtain observations of celestial objects below 3000 Å from the ground. A replacement for OAO-B is thus urgently required to complement the rather narrowly defined capabilities of OAO-C. The Small Astronomical Satellite SAS-D, presently in the planning stage, also appears to have many desirable characteristics from the standpoint of planetary observations. Some additional work can be done with sounding rockets if the experiments are carefully conceived and successfully executed, but there is no hope of achieving high spectral or spatial resolution over a broad bandwidth with these techniques. Instead, the emphasis should properly be on the detection of certain specific features, such as a given emission line or the intensity or degree of polarization at certain wavelengths, to complement the OAO results. As in the case of the infrared observations, supporting activities, such as instrument development, and relevant laboratory and theoretical investigations should be pursued.

Finally, there are present limitations imposed on the information content of planetary uv spectra by inadequacies in our knowledge about the solar spectrum in this region. We thus strongly endorse observations designed to improve full-disk solar photometry below 3500 Å, especially observations made over a long period to investigate possible variations in the uv flux at different wavelengths.

F. LOOKING TOWARD THE FUTURE—HIGH-LEVEL SUPPORT

If we exclude deep-space probes from consideration, it should be apparent from the preceeding discussion that the most pressing current need in solar system science is for a large space telescope (LST) capable of both uv and ir observations. It may prove desirable and more economical to have two telescopes, one especially designed with suitable long-lived cryogenics, for wavelengths above 3 μm, and the other for shorter wavelengths. There is no substitute for the capabilities of a telescope of large aperture. At the same time, there is no doubt that smaller instruments can contribute significantly toward the solution of many problems. Thus in an era when our concepts of extragalactic astronomy are undergoing serious revisions as a result of observations carried out by telescopes with apertures in excess of 100 in., useful research is simultaneously being carried out with 16-in. reflectors suitably equipped for photoelectric photometry. Both approaches are necessary for the balanced advancement of the science, but the extraordinary significance of the LST for opening new frontiers should not be compromised by an awareness that smaller telescopes are capable of performing useful observations. Examples of the results to be expected from such an instrument have been given in the introduction.

At radio wavelengths, we may expect a substantial increase in our knowledge of the Jovian decametric radiation from a suitable low-frequency array deployed in space. To be fully effective, such an array should not only permit an extension of the emission spectrum beyond the limit imposed by the terrestrial ionosphere but should have sufficient angular resolution to provide information on the location of the sources of radiation on the planet's disk. Higher-frequency observations can be made effectively from the ground until one reaches the millimeter region, where water-vapor absorption becomes important. There is a transition region here, between infrared and microwave techniques, that would again benefit from the accessibility of a large radiation collector in space.

G. SUMMARY

The recent surge of new information about the solar system has resulted from a comprehensive program of ground-based, near-earth, and deep-space investigations. If progress is to continue at the present rate, we must continue to exploit all three modes since each provides opportunities not available to the others.

The near-earth research program that is the main subject of this report is reaching a critical stage. There are presently no firm plans for any new starts in this area beyond the Small Astronomical Satellite program. In order to take full advantage of the opportunities provided by observations from space, we make the following recommendations:

1. A program leading to the deployment of a Large Space Telescope should be initiated. The versatility of such an instrument fully justifies giving it top priority.

2. A logical first step in this program is to replace OAO-B with an equivalent satellite as rapidly as possible. Development of SAS-D as a follow-on to the OAO program is strongly encouraged.

3. Strong support should be given to a program to obtain highly accurate spectrophotometry of integrated solar light in the region below 3500 Å, including a search for temporal variations. The far-infrared spectrum of the sun should be explored at high resolution to permit the detection and identification of absorptions occurring in this region. We cannot provide an unambiguous interpretation of planetary spectra without these data.

4. Further development of infrared detectors and cooling systems for space application should be encouraged. The combination of a suitable infrared detection system and the proper type of Large Space Telescope may be expected to provide a large amount of new information about our own solar system and possibly generate the first real capability for direct detection of other solar systems.

5. The development of a large antenna array for low-frequency studies would provide valuable information on the low-frequency ($\nu < 3.5$ MHz) characteristics of the Jovian decametric radiation in addition to localizing the source(s) of that radiation.

One final remark is in order concerning these recommendations. In no case are we proposing that the facility in question be used exclusively for planetary research. However, in accepting the principle that such a restriction would be extremely uneconomical, we are forced to rely on the good will of the scientific community to make these facilities available for solar system research. We would therefore add a sixth, implicit recommendation that considerable care be taken in providing intelligent and equitable planning and administration of the programs that we have endorsed.

CHAPTER FIVE

Solar Astronomy

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I. INTRODUCTION

Solar research is related to the human environment, to general astronomy, and to physics. In addition to providing us with a rich source of physical phenomena for study, the sun is unique as the prime source of energy to support life on earth. We discuss the general aims of solar research in the following section.

In Section III, we focus on four major problem areas in solar research, chosen for their impact inside and outside solar research; they may soon lead to answers to crucial questions; advances in these areas would organize and synthesize many individual and now isolated phenomena.

The Panel believes it essential for a program in solar research to be carried on in a variety of subdisciplines. The subdisciplines are listed according to techniques. The final section of this chapter presents a list of recommendations concerned with an overall solar research program and with each subdiscipline. On the basis of these recommendations, the Panel concludes by outlining a minimum and an optimum program for solar research.

II. AIMS OF SOLAR RESEARCH

A. THE NATURE OF SOLAR RESEARCH

Like all of astronomy, solar research is an observational science basically different from an experimental science such as laboratory physics. Definitive observations, analogous to crucial laboratory experiments, will always be limited by the inaccesibility of the sun. Nevertheless, the sun offers a variety and range of physical conditions that provide an extraordinary natural laboratory, which can be observed in greater detail than any star.

The sun long ago provided evidence for new species of atoms, for thermonuclear generation of energy, and for the significance of plasmas on a cosmic scale and illustrated many concepts in the dynamics of rotating stars. Many of these discoveries are applied today to circumstances so different from those of the sun that their historical origins may well be forgotten.

Solar physics is a rich discipline of physical science and astronomy. Major current research is concerned with such diverse phenomena as neutrinos, cosmic rays, the solar wind, and solar radio bursts. Long-standing questions still awaiting solution include the large-scale fluid circulation in the sun, the solar helium abundance, and the origin of the solar activity cycle. New observational technology is crucial for investigating these problems. Examples include advances in photoelectric spectroscopy, applications of computer technology, interferometer arrays for radio studies, new detector technology for all parts of the electromagnetic spectrum, sophisticated grazing-incidence optics for imaging the sun in x rays, and vacuum telescopes and spectrographs for high-resolution, ground-based observations.

The sun, as the closest star, is the prime source of energy to support life on earth, giving solar studies a directly practical aspect. The attainment of some of man's objectives for a more stable and comfortable environment may, in the long run, depend on his knowledge of the sun.

Involvement in solar research, like most basic research, is partly motivated by the beauty of the subject in its own right. An essential characteristic of man is his ability to appreciate and to speculate on such features of the world about him as its natural beauty, even when the applications of science provide external reasons for its adequate funding. There is a strong motivation for solar astronomers in the magnitude, power, symmetry, and strong intricacy of solar structure and the extraordinary way that the whole picture fits together.

B. RELEVANCE OF SOLAR RESEARCH FOR GENERAL ASTRONOMY

The sun, as a typical star, fulfills an important function in astrophysics by providing close at hand phenomena often only vaguely sensed or speculated to occur in stars in the distant reaches of the universe. A few examples are the solar wind (as an example of mass loss), the chromospheres (as an archetype of stellar chromosphere), radio bursts in the solar corona, and the existence of sunspot magnetic fields.

Recently, we have learned that the sun has an order of magnitude less neutrino production than had been suggested from theoretical analysis of the energy production process. What this extraordinary experiment implies for solar physics is quite unclear. There is little doubt, however, about the importance of this experiment for the study of stellar structure.

Inhomogeneities in the solar atmosphere, e.g., density, temperature, and radiation contrasts from one portion of the atmosphere to another, and the dynamic phenomena in the solar photosphere and atmosphere provide basic insights into many phenomena throughout astronomy. Solar flares, for example, may be a model for phenomena occurring in T Tauri stars during star formation. Unraveling the physics of solar flares may help to provide the understanding of processes of particle acceleration that occur throughout the universe.

The dynamic spectral properties of the radio and x-ray emissions of the solar atmosphere are the basis of predictions of what must be occurring in very different physical environments. Hints of radio and x-ray emissions by dynamic processes in other stars already exist. These are poorly studied because of technological problems of making such observations of faint sources. The better observed solar phenomena must eventually provide a rich tool for understanding the atmosphere of other stars.

The interpretation of solar radio events extends to other regions of astronomy, for example, planetary nebulae and supernova envelopes. Plasma frequencies there are high enough to produce radio phenomena such as occur in the solar corona. The predicted frequencies for such radio events on a galactic scale are below the range of the spectrum observable from the surface of the earth. A clear understanding of the physics of solar bursts must precede interpretation of dynamic phenomena that will be discovered as space radio astronomy develops.

Another important result of solar studies for astrophysics is the observation of the solar oblateness. If this observation is confirmed by other techniques, it will have an enormous impact throughout astronomy. The coupling of the rotation of the interior and exterior of the sun via magnetic forces has played a vital role in the interpretation of this result. It had been assumed that the magnetic properties of a stellar interior, however important they may have been in influencing surface phenomena, would not substantially affect the overall structure. This now seems to be too limited a point of view.

C. RELEVANCE OF SOLAR RESEARCH FOR PHYSICS

The sun provides challenge and opportunity for the physicist, as well as for the stellar astronomer. The most current example of this is plasma physics, in which plasma turbulence and its coupled radiation field present important problems. For a decade it was realized that understanding these phenomena was essential to understanding the physics of solar radio bursts, arising from rapidly moving particles. The stability and confinement of such particles within the complex structure of the corona continue to be a mystery. The corona represents a natural plasma laboratory on a scale enormous compared to any terrestrial laboratory, within which phenomena occur whose interpretation will be an important test of plasma physics at a most sophisticated level.

The solar atmosphere provided a major stimulus for the study of the physics of radiative transfer. Neutron diffusion in nuclear reactor piles is mathematically identical to certain problems of solar physics. There has been a long-standing profitable interchange of ideas between the two fields. The radiative-transfer theory of the formation of line spectra is at its most sophisticated level in theoretical studies of the spectra of the solar photosphere and chromosphere. A wide range of solar phenomena still remain to be interpreted by the most modern tools of radiative transfer. These studies stimulate the development of radiative-transfer methods applicable to the understanding of terrestrial and planetary atmospheres.

The emission of neutrinos from the sun is a problem in atomic and nuclear physics. One possible (but improbable) interpretation of the low observed solar neutrino emission is that the theoretical cross section for the basic hydrogen plus hydrogen reaction in the solar core, which involves the theory of beta decay, is in error. Because this reaction cannot be measured in the laboratory at the appropriate low energies, the solar measurements provide a unique opportunity to test fundamental physics.

The measurement of the oblateness of the sun is also a potential contribution to fundamental physics. The experiment arose from the thought that a significant part of the precession of the orbit of Mercury could result from a gravitational quadrupole moment of the sun. If this existed, differences between the observed and the classical precession rates suggest a modification of the Einstein form of general relativity.

D. RELEVANCE OF SOLAR RESEARCH FOR THE UNDERSTANDING OF THE SOLAR SYSTEM

The sun's mass, which holds all the planets in their orbits, dominates the entire solar system. But without the sun's energy the system would be vastly different, and, of course, life on its planets would be impossible. Essential keys for the understanding of the origin of the solar wind, of solar cosmic rays, of the long-term variability of the sun in relation to planetary climatic variations, and of the overall origin and evolution of the solar system lie in solar astronomy. The solar wind and solar cosmic rays affect the planets and the space between them in many important and subtle ways discovered and investigated within the past decade. The pattern, in direction and in time, of the arrival of cosmic rays from outside the solar system requires a knowledge of how the sun loses its outer atmosphere and magnetic field to space. The earlier notion that the corona evaporates uniformly in all directions is clearly too simple. The streamer structure revealed by recent eclipse photographs involves the magnetic-field patterns of the solar surface, modulating cosmic rays, both from the galaxy and from the sun itself. This structure controls gaseous tails evaporated from comets as they approach the sun. Within the optical coronal structures, a complex fine structure exists, revealed by experiments aboard spacecraft moving around the sun and by measurements of signals from radio stars passing through these irregularities in the interplanetary plasma.

Solar gases interact with planetary magnetic fields throughout the solar system to an as yet unknown outer limit, probably beyond the orbit of Jupiter. This interaction has been historically a subject for geophysicists rather than astronomers, while the magnetic phenomena in the planets, e.g., Jupiter, remain a subject studied mostly by astronomers.

The sun contains most of the mass but only a small fraction of the angular momentum of the solar system. If the solar system was an integral subunit of the galactic gas and dust, and evolved as an isolated unit from that original state to its present form, a major problem arises.

The angular momentum per unit mass has not been conserved throughout the solar system; the sun appears to have lost most of its angular momentum. This angular momentum transfer is essential to the study of the origin and evolution of planetary systems elsewhere.

E. RELEVANCE OF SOLAR RESEARCH FOR HUMAN ACTIVITIES

The sun, as the source of almost all of our energy, is of overwhelming importance to our existence. Without it, life on earth would be impossible. This by itself, however, does not make *solar research,* as contrasted to *the sun,* relevant to our environment. The relevance of solar research stems from the variability of the interaction of the sun with the earth.

The variation of the sun is a prime candidate as the source of planetary climatic variations. Within the last 10,000 years or less, mankind has witnessed the appearance of land bridges between Asia and North

America, the growing hostility of the environment in the northern third of North America and near all elevated mountain ranges, and the possible modification of climate in those parts of the globe habitable by man. The time scale, whether it be the onslaught of new glaciation or further reduction of the remnants of the last glacial epoch with flooding of lowland areas of the continents, is too long to be of interest in planning a single decade of astronomical research. However, the mechanisms are as mysterious today as when the ice ages were discovered a hundred years ago. The nature of these changes, and whether there has been a change in the overall solar energy output, are still unknown.

A smaller but perhaps related problem is the earth's reaction to corpuscular radiation from the sun—the input of energy to the earth's magnetosphere and auroral zones and the solar cosmic rays that on occasion penetrate the earth's atmosphere through the shield provided by the earth's magnetic field. Sophisticated data analysis has been used to attempt to demonstrate a short-term correlation between variations in solar activity and terrestrial atmospheric dynamics. It would be unwise to ignore the possible relevance of solar variability to daily or longer-term variations in the earth's weather.

For other, more immediate practical problems, understanding the sun is critical. One is related to space-weather forecasting, monitoring, and prediction of solar radiation changes in connection with the VELA project. VELA satellites monitor the radiation from man-made sources such as nuclear explosions. As such, they are important in the supervision of existing test ban treaties; they also provide technical backup for future treaty negotiations. Solar and man-caused disturbances are, however, very similar viewed from satellites, so that prediction and monitoring of solar events is essential.

Another currently important problem relates to long-term manned spaceflight, where men are exposed to the full gamut of solar radiations. So far there has been no damaging exposure of our astronauts to solar radiation, particularly to the relatively low-energy cosmic rays that the sun infrequently produces in huge numbers. This is partly due to luck, but also because a major portion of our manned space program occurred during the minimum phase of solar activity from 1960 to 1966. Furthermore, the last solar maximum during which we have placed vehicles on the surface of the moon has for unknown reasons been a significantly less hazardous maximum than that of the late 1950's. We must understand what man's future in space will be when his trips are longer and when the sun reaches another peak of activity again.

III. SOLAR RESEARCH TODAY

A. INTRODUCTION

The past decade has seen an enormous increase in our knowledge of the sun, as a result of very rapid development of observational and theoretical techniques. Access to the solar electromagnetic spectrum has been expanded from a few octaves in the visible and radio region of the spectrum to all regions of the spectrum—emission from the sun at wavelengths from as short as 0.1 Å to as long as 10^{13} Å has been studied. Observing techniques in the visible region of the spectrum have been improved so that, for example, atmospheric oscillations and large-scale convection patterns have been discovered on the sun. Theories of radiation transfer in stellar atmospheres have been refined to give a much improved interpretation of the solar spectrum. A rapid development in the theory of magnetohydrodynamics is of great significance for our understanding of sunspots and coronal heating. The solar corpuscular flux has been studied, and attempts have been made to detect solar neutrino radiation.

New techniques have significantly altered our understanding of the sun. New problems have arisen, and some of the old ones remain. In this section we discuss four major problem areas of solar physics today, outlining the present status and limitations of our knowledge and suggesting a research program likely to lead to better insight.

In choosing major problem areas the following criteria were applied:

1. The problem areas are of interest not only to solar studies but also to general astrophysics and basic physics.
2. Advances in the area organize and synthesize many individual phenomena.
3. The problem areas require techniques and theories available now, or shortly, to permit the investigation to be carried out.

On the basis of these criteria the following four problems were selected:

1. What is the flare instability, and what is the mechanism of particle acceleration?
2. What are the energy-generating processes in the solar interior?
3. What are the energy transport processes in the sun?
4. What is the large-scale circulation in the sun?

These problems refer to some extent to the study of the sun *per se*, but they also relate strongly to other disciplines, for example, the origin of cosmic rays, plasmc instabilities, solar-terrestrial relations, stellar structure and evolution, nuclear cross sections, stellar chromospheres and coronas, sunspot cycles on stars, and long-term climatic variations.

B. MAJOR PROBLEM AREAS OF SOLAR RESEARCH

1. *Flare Instabilities and Particle Acceleration*

The generation of cosmic rays by the sun seems so improbable an event that it might never have been imagined had it not actually been observed. The conversion of the disordered slow motions of large quantities of gas in the convective layer of the sun into the ordered motion of a few particles with relativistic velocities stands as one of the most incredible natural events in the universe. Solar physicists are fascinated by the complex physical processes that accelerate protons and electrons to cosmic-ray energies. The study of particle events on the sun opens the possibility of understanding similar but more energetic phenomena throughout the universe. Interaction of these fast particles with the plasma and imbedded magnetic fields produces a wide variety of phenomena, interesting in their own right, from the standpoint of plasma physics, and offering us the opportunity to understand similar events in more distant and less well-resolved objects.

The past decade of solar research has led to a working model for the events that immediately follow the flare instability. According to this model, a very sudden release of energy takes place in the inner corona because of an instability in the magnetic-field configuration. Three main phases follow this explosion. In the *first phase*, solar protons and electrons are accelerated to subrelativistic velocity. The electrons radiate x rays and microwave radiation by nonthermal bremsstrahlung and gyrosynchrotron radiation. Downward-moving electrons collide with atoms in the chromosphere to produce the classical H-α flare. The plasma in the vicinity of the instability is heated to a temperature of the order of 40 million degrees and emits strongly in the soft x-ray region and in the centimeter-wave band. A plasma blast wave is excited, which, propagating through the corona at speeds in excess of 1000 km/sec, radiates Type II radio bursts. Plasma oscillations, excited by the upward-moving electron beam, radiate Type III bursts. In the *second phase* of the flare, the plasma cools rapidly and recombines. The magnetic-field lines in the high corona are pushed outward and perhaps lead to a further acceleration of the electrons and ions to relativistic energies. Synchrotron radiation is emitted by the relativistic electrons spiraling in the magnetic field in the high corona. In the *last stage,* the blast wave moves out into the solar wind, carrying with it relativistic particles in a tangled magnetic field.

Although this overall scheme is probably correct in many of its broader aspects, we are ignorant about many of the detailed physical processes at various stages in the scheme. It is known, for example, that solar flares are rooted in the chromosphere and typically extend as high as 20,000 to 30,000 km into the inner core. There are general indications regarding the site of origin of the flare with regard to the magnetic field of the underlying sunspot configuration, but we still cannot point to *the* physical structure or coronal magnetic-field configuration in which the instability occurs. General considerations point to the coronal magnetic field as the most likely source of the energy ultimately dissipated in radiation, particles, and mass motion. Moreover, in order to dissipate the existing magnetic field rapidly enough, the instability must occur in a small structure. Thus, we are driven to examine the smaller-scale structures that we can observe on the sun, in the vicinity of regions known to produce flares.

There are several promising possibilities. Space observations of the permitted lines of helium-like ions in the 1- to 20-Å region suggest the possible existence of extremely dense and hot knots of coronal material overlying the active region prior to the flare. Diameters of these regions have been determined indirectly from the observations to be of the order of 3000 km or less. Another possible site of origin of flares is the x-ray loops that were once seen in a unique observation with a rocketborne x-ray telescope immediately after a small flare. These loops are strongly suggestive of the loops and arches in the corona seen by observations at the limb, in H-α, and in the optical coronal emission lines. They probably outline individual magnetic flux tubes, which, for reasons not yet understood, are filled with hot coronal material.

To determine the site of origin of flares, we need high-resolution images of the sun in many wavelength regions, including hard and soft x rays, euv, and microwaves, in addition to H-α. Spectra in all wavelength ranges are also essential, in particular in the x-ray and microwave region, but not excluding very broad wavelength coverage even in the H-α region. Moreover, we need measurements of the coronal magnetic field. Probably the best way to locate the site of origin of a flare is to observe its initial stages in a broad band of x rays, with sufficiently high resolution in space and time.

Once we have located the actual site of origin, we can proceed to investigate the heart of the problem, i.e., the nature of the instability that produces the flare and of the mechanism that accelerates the particles. Several types of instability have been suggested, such as a tearing-mode instability, pinch effect, an instability at the front of a shock wave, or the so-called Sweet mechanism for the resistive cancellation of oppositely directed fields. At the moment, however, no particular mechanism is the outstanding candidate, nor can we name the particular measurement to determine uniquely the nature of the instability.

The essence of the instability may well reside in the initial shape of the magnetic field; details of photospheric, chromospheric, and coronal magnetic fields must be measured with high time and spatial resolution. A number of ground-based polarimeters working in visible light are being developed for this purpose. In the microwave region, continuous emission from the thermal chromospheric and coronal plasma has polarization properties depending on the magnetic-field strength. In the submillimeter range, possible recombination lines should have large Zeeman splitting that will be sensitive to coronal magnetic fields. In the visual range, the Hanle effect remains a sensitive (though difficult to interpret) indicator of the presence, strength, and orientation of ambient fields.

The initial acceleration of particles may occur within a few seconds during the onset of a solar flare. On the other hand, it seems likely that particles continue to be accelerated for hours or even days following the initial event. We have only the beginnings of an idea of how the particles may be accelerated. We need, therefore, to employ the full range of electromagnetic and particle spectrum observations to investigate how the particle energy spectrum varies with time and, in particular, with charge and mass.

We have several promising leads. Semiperiodic x-ray and radio bursts have frequently been observed during flares. The radio bursts most closely associated with x-ray events are at centimetric wavelengths and are extraordinarily bright as well as circularly polarized. By combining observations of x-ray and microwave bursts, we hope to advance substantially our understanding of particle accelerations in the sun.

It is vital to have good measurements for the energy and rigidity spectra for the heavy nuclei released from the solar atmosphere at billion-volt energies. Only a few preliminary measurements have been made for elements heavier than helium; these can give important information on how energy is delivered to heavy particles. To estimate the true energy spectrum of ions at the site of acceleration, the distortions produced by propagation through the interplanetary medium must be accounted for.

The site of initial particle acceleration is very likely quite low in the sun's atmosphere. Millimeter radio observations of the sun penetrate more deeply than centimeter ones and should prove valuable. Despite the relative paucity of data, some major flares are accompanied by millimeter bursts.

The middle corona represents a vast laboratory in which the plasma physicist can study the interaction of particles with plasma and magnetic fields. Through the study of nonthermal x-ray and radio bursts at decimetric through hectometric wavelengths, he can study the physics of these interactions and even discover new effects that are minor or unsuspected in laboratory sources. A good example is the influence of coronal plasma upon synchrotron radiation emitted in Type IV bursts following a large solar flare. Some of these bursts show a sharp cutoff at high and at low frequencies, which cannot be interpreted by any electron energy spectrum in classical synchrotron theory. Indications are that these bursts involve particles in the transition-energy range between classical gyroradiation and relativistic synchrotron radiation.

Shortly after the explosive stage of a flare, the plasma appears to be heated to a temperature as high as 10^7 to 10^8 K. Part of this heating may result from shocks, part from resistive losses in currents flowing in and near the flare, and part from collisions between the plasma and nonthermal energetic particles. By following the thermalization process—especially in its soft x-ray spectrum—we gain important information about the total energy input of the original flare instability, the energy spectrum and flux of low-energy nonthermal particles, and the chemical composition of the hot region.

To trace the time variation of temperature and density in the flare plasma, we need diagnostic techniques to interpret its x-ray spectrum. A new and powerful method now exists that determines electron densities from the line intensities of helium-like heavy ions, such as Ne IX, Mn XI, Si XIII, and Ca XIX. The interpretation of the observed line intensities depends on collisional cross sections and transition rates supplied from theoretical calculations and laboratory investigations. The development of this method illustrates the importance for the interpretation analysis of solar data of supporting theoretical and laboratory experiments.

In summary, the following kinds of observation are required during the next decade to investigate a central problem in solar physics, flare instabilities, and particle acceleration:

1. X-ray images of the corona throughout the duration of a flare with high spatial and time resolution with moderate spectral resolution.
2. Euv images similar to (1), with spectral resolution sufficient

to separate individual emission lines; preferably simultaneous observations in lines of different characteristic temperature.

3. Similar microwave images of the flare throughout the development and decay of many flares, hopefully strongly overlapping the data of (1) above.

4. X-ray spectroscopy of the postflare plasma with time resolution of the order of a minute and spectral resolving power of the order of 5000 with moderate spatial resolution.

5. Coronal magnetometry based on observations of the visible, millimetric, and centimetric wavelength regions.

6. Associated measurements of the energy and rigidity spectra of heavy nuclei throughout the flare event.

7. Radio studies of flare-associated bursts with high time spatial and spectral resolution at decimetric and longer wavelengths.

8. Radar observations of the corona directed toward the coronal conditions in regions through which flare particles and waves travel.

2. *Energy-Generating Processes in the Solar Interior*

The energy radiated by the sun and stars originates in their interior regions. Until a few years ago, all of our knowledge was inferred from theoretical solar models that reproduced the observed bulk properties of the sun: total energy radiated, mass, radius, and surface chemical abundances. Until recently no independent check on the inferred conditions in the solar core seemed possible. However, neutrinos created in the nuclear processes responsible for the solar energy supply escape unimpeded from the central regions where they are produced and provide a direct measurement of conditions in the sun's interior. The fact that neutrinos can escape so easily from the sun also makes them difficult to capture on earth.

This upper limit, however, is well below the value of the flux predicted on the basis of most current models of the solar interior. There are a number of uncertainties in these predictions due to uncertainties in the model and in various required parameters. The flux predictions are in particular influenced by (a) relative abundances and their distribution throughout the sun (especially helium and beryllium), (b) the efficiency of various mixing mechanisms in the sun, (c) uncertainties in cross sections for nuclear reactions at energies and under plasma conditions prevailing near the center, and (d) uncertainties in opacity estimates. Even by stretching as far as possible within the uncertainties, no single one of these factors appears capable of reducing the predicted neutrino flux to the observed upper limit. Their combined action may just bring the predicted flux down to the limit. What is clearly needed is an increase in the sensitivity of the neutrino detector and much laboratory and theoretical work to improve our knowledge of the quantities going into the predictions. If the discrepancy remains, other physical effects, for example, very large magnetic fields ($\sim 10^9$ G), might affect the structure of the solar interior. A solution of the neutrino flux discrepancy will influence models of interiors of other stars and possibly alter our concept of stellar evolution.

The upper limit thus far established indicates that we have not estimated correctly the flux of high-energy neutrinos produced in the $^8B \rightarrow {}^8Be + e^+ + \nu$ reaction. This reaction is thought to be a rare one, occurring once for each thousand proton–proton fusions. The current upper limit tells us that the ^8B decay occurs five or ten times less frequently than supposed. Davis hopes to improve the sensitivity of his counting technique in the $^{37}Cl\ (\nu, e^-)\ ^{37}Ar$ reaction by perhaps a factor of 5. The observed counting rate will then test our ideas about the production of lower-energy neutrinos via the reaction $^7Be + e^- \rightarrow {}^7Li + \nu$, which is thought to occur one fifth as frequently as the $p + p \rightarrow d + e^+ + \nu$ reaction. A negative result will require other detecting reactions to test the frequency of the proton–proton reaction itself, the nuclear theory, or the solar interior model.

Other detecting reactions, like neutrino–electron scattering, $^7Li(\nu, e^-)^7Be$, $^{87}Rb(\nu, e^-)^{87}Sr$, and $^{71}Ga(\nu, e^-)^{71}Ge$ should be considered even if the $^{37}Cl(\nu, e^-)^{37}Ar$ experiment is successful, since they would permit, by "neutrino spectroscopy," a direct evaluation of several individual nuclear reaction rates in the sun. Each detection technique has its own energy dependence; a series of them might discriminate between the frequency of neutrinos coming from the (p, p), $(^7Be + e^-)^8B$, ^{13}N, and ^{15}O.

3. *Energy Transport in the Sun*

The energy generated by nuclear reactions in the solar interior is propagated through the body of the sun by convection and radiation. One of the major remaining uncertainties in specifying interior characteristics is the precise relationship between the temperature gradient and the local energy flux. What is lacking is a full understanding of the absorbing and scattering capabilities of atoms in a dense plasma. We are not even sure whether the sun possesses a convective core.

At the solar surface, forms of energy transport other than convection and radiation become important. Wave generation and conduction are but two of these additional forms. These forms determine, along with radiation and convention, the structure of the solar atmosphere, especially the corona and chromosphere. Study of these transport processes is important not only because of their relevance to the understanding of the sun but also because similar processes might apply elsewhere (stellar coronas) and because the study of them under conditions existing on the sun is impossible elsewhere.

In the outer shell of the sun, both convection and radiation contribute to the energy transfer; near the photosphere, radiation takes over and convection plays only a minor role as far as total energy flux is concerned. The radiation, however, escapes from the photosphere unobstructed, and it is therefore the small amount of convective energy that determines the structure of the higher layers. This convection is believed to have two types of effect: (a) it provides the "mechanical noise" source for the generation of gravity, acoustic, and hydromagnetic waves; and (b) it distributes the magnetic field across the solar surface, thereby providing for an inhomogeneous distribution of energy transport by waves and conduction in higher layers where the magnetic energy density dominates. Specifically, the magnetic field channels the nonradiative heat conduction from the hot corona toward the "cold" photosphere and chromosphere into regions where the magnetic-field lines converge, thus strongly modifying the solar chromosphere. The corona loses its energy outward through heat conduction, radiation, and, ultimately, through the solar wind (which one might consider as still another form of convection).

The transport of energy outward from the sun manifests itself in a number of observable features at different heights in the solar atmosphere. Examination of the properties of these give direct tests of the energy-transfer mechanisms present. One of the most significant quantities is the temperature and density variation with height in the solar atmosphere. This temperature variation is well known in the solar photosphere; the variation at greater heights is, however, still very uncertain. In recent years, we have obtained somewhat more detailed information on the photosphere–corona transition region temperature structure from the observation of far-ultraviolet emission lines of different ionization and excitation potentials. These show, for the average sun, an incredibly sharp rise to coronal temperature, which is of great consequence for energy transfer. The variation of temperature above the very inner corona is poorly known because no related observable quantities can be reliably interpreted.

In addition to the temperature structure, we can observe a number of details on the solar surface that are directly related to the energy-transfer processes. In the lowest, photospheric layers, the granulation and supergranulation cells are undoubtedly related to the underlying hydrogen convection zone and therefore provide information about the properties of the convection energy transport. The granulation elements are very small ($\leqslant$1000 km), which makes them hard to study with existing telescopes. The question: "What are granules?" is still unanswered. They may be related to Bénard cells, although most granules show a behavior that is very different from such well-organized convection cells.

A significant improvement in spatial resolution (to ~100–200 km) is needed for both temperature and velocity measurements of individual granules. Knowledge of granule properties can be used to check theories of convective energy transport below the photosphere and of the nonconvective, mechanical energy input to layers above. The granules are the prime candidates for the source of this mechanical energy. They provide a crucial link in the photosphere–corona transition.

The supergranulation is much easier to study observationally because of its large size (30,000 km), and its properties are fairly well known. It is not at all clear, however, why the outer layers of the sun produce convection cells of such a well-defined scale. The function of the supergranulation in the energy transport is twofold: (a) as do granules, it reflects the properties of the hydrogen convection zone and possibly of the He ionization zone; and (b) it causes a large-scale inhomogeneity in the surface distribution of magnetic fields.

The photosphere–corona temperature transition region is also the region where the magnetic and kinetic energy density change in relative significance. In the photosphere, the convection is strong enough (except in sunspots) to modify the magnetic field. The supergranulation, and probably the granulation, redistribute the field and concentrate it on the cell boundaries. The granulation and the field also interact to create various hydromagnetic waves. In the layer of decreasing mass density just above the photosphere, the magnetic energy density dominates, and therefore regulates, much of the energy transfer processes.

The granulation may be a source of wave energy for the heating of the solar corona. Despite intensive work, we still do not know the relative importance of the various kinds of wave energy. Sound waves are easiest to deal with, but magnetoacoustic waves, ion-cyclotron waves, and gravity waves are all plausible. The 5-min oscillation of the photosphere is probably related to the wave energy input to the upper solar atmosphere but remains a separate, and as yet unrelated, peculiarity of solar phenomenology. Euv Doppler shift observations may answer the question: "How does this oscillation propagate upward and dissipate?" while euv emission-line measurements may settle the question: "How is the temperature structure related to the oscillations?" Previously unknown wave motions in the corona have appeared in recent high time and high spectral resolution radio observations in the metric and decametric wavelength ranges. These apparently are waves of speeds ranging from Alfvén velocities up to the ion-acoustic and electron-acoustic velocities. Their relevance to coronal heating is still unknown. Study of the effects in magnetic fields will bear on the question: "What is the temperature structure near active regions and in the magnetic enhancements at the supergranule boundaries?" We know already that both the corona and the chromosphere are very different in these magnetic regions. This is not surprising because of the dominance of the magnetic energy density. The largely unanswered questions of why and how they are different require better spatial and spectral resolution in the visual, ultraviolet, x-ray, and radio region of the spectrum.

The regions of magnetic enhancements associated with the supergranulation are the sites of the chromosphere spicules and of the bright chromospheric network observable in the xuv emission lines and in the cores of Fraunhofer absorption lines. The role each of these plays in the coronal heating process is not clear. Spicules are the jet-like structures that seem to constitute most of the upper chromosphere. Their kinetic energy flux compares with the energy flux carried downward by thermal conduction from the corona. This conductive energy is transported along the magnetic-field lines and is therefore concentrated in the regions where the spicules and network occur. If the origin of the spicules is this heat conduction, they are only secondary phenomena, not contributing to the coronal heating. In other models, spicules are part of the coronal heating process. The mass transport in spicules, in contrast to the energy transport, is truly gigantic. The upward moving matter of 10^{16} atoms cm^{-2} sec^{-1} is sufficient to replenish the corona in a few minutes and exceeds the mass flux in the solar wind by several orders of magnitude. Most of the mass, therefore, must return to the lower chromosphere. Spicules are undoubtedly the prime candidate for the mass source that eventually produces the solar wind. Their properties are hard to determine since they are, like granules, so small that our existing telescopes, both in the visible and the uv regions, cannot resolve them well. In addition to these observational problems, it is difficult to interpret the observed parameters in terms of physical properties. The radiative-transfer theories for complicated, time-dependent geometrics like spicules is still in its earliest stages. High-resolution observations in the far-ultraviolet emission lines ($\leqslant 1$ sec of arc) hold most promise for spicule studies. From these it is possible to study spicules, as well as the network, on the solar disk where they do not overlap as they do on the limb. The photospheric background is not a complicating factor as it is in the visible. The availability of many emission lines of widely differing excitation potentials will permit a detailed determination of the temperature and velocity structure, thus probably answering the questions: "What are spicules? What role do they play in the coronal heating process and in the mass transport in the corona?"

The solar wind itself appears to arise in the outer corona, where the magnetic energy density becomes a secondary factor. Our knowledge of this region is based on eclipse observations of the electron scattering corona and on low-resolution radio and radar observations at low frequencies. The height at which the solar wind starts probably varies with position on the sun. In the streamer regions above strong bipolar regions, the wind is likely to be different from regions in the quiet corona, since the magnetic energy density is less in the latter. The most powerful tool with which to study the origin of the solar wind appears to be radio and radar observations of the sun. The latter have already given valuable data on the mass outflow and perhaps on traveling waves in this part of the solar corona. So far, these observations have little spatial resolution. Radar observations with even low resolution ($\sim$5 min of arc) would provide an important addition to the study of the outer corona and to the origin of the solar wind.

Most of the discussion above has been centered on the quiet sun, undisturbed by the strong magnetic fields associated with sunspots. The energy transport in the active regions is very different. Its study can, therefore, provide additional insight into the role of very strong magnetic fields. In active regions (plages and the sunspots), phenomena like granules, oscillations, and spicules are modified or absent. Other phenomena arise like umbral dots, umbral flashes, arch prominences, and coronal arches. Observations of the extreme ultraviolet emission-line spectrum show an even faster temperature rise toward coronal temperatures than in quiet regions, possibly because of a modification of the conductive downward flux or a modification of the heating mechanism. A comparative study of the behavior of active and quiet regions is therefore highly relevant to the problems of energy transport.

In summary, we anticipate the following developments to be necessary to make major progress in the understanding of the problem of energy and mass transport in the solar atmosphere:

1. A strong effort in the theory of magnetohydrodynamics and plasma physics (convection, wave generation, propagation and dissipation, etc.).
2. A continued examination of the radiation transfer in the solar atmosphere since spectroscopy is our major diagnostic tool.

3. Increased spatial resolution of telescopes. In the visible region one should aim for 0.1–0.2 sec of arc for the study of granules and spicules; in the xuv and x-ray regions for 1–2 sec of arc for the study of spicules, the chromosphere network, and differential coronal heating; in the radio and especially the radar region (~30 MHz) a resolution of 1–5 min of arc will greatly increase our understanding of the origin of the solar wind.

4. Increased spectral resolution ($\lambda/\Delta\lambda \approx 10^5$) so that line profiles, Doppler shifts, and magnetic fields can be measured. This should be combined with good spatial resolution. This requirement has already been fulfilled in the visible; it therefore refers mainly to the euv, infrared spectrum, and microwave (millimeter) spectrum. If the predicted coronal recombination lines in the millimeter regions of the spectrum are confirmed, one will have a very valuable tool with which to study the lower coronal magnetic field, temperature, and density structure.

5. Improvements in two-dimensional image acquisition and storage devices.

4. The Large-Scale Circulation in the Sun

An outstanding problem in solar research is, and has been for a long time, the origin of the sunspot cycle. Recent views relate the solar activity cycle to the general circulation of mass through differential rotation and through the supergranulation. Babcock has suggested that differential rotation with latitude amplifies the subsurface magnetic fields, which, after attaining sufficient strength, are transported to the surface by buoyancy effects, thus creating sunspots. Leighton proposed that the diffusion of the sunspot fields by the supergranule convection cells is responsible for the 22-year cycle in the large-scale solar magnetic field. Understanding of the large-scale circulation in the sun and stars is essential for the study of stellar evolution. We require knowledge of the mixing of elements in the interior (by convection and by currents set up by the rotation) of the angular velocity variation with depth resulting from the interaction between the convection and the rotation and of the mass loss and spindown resulting from the stellar wind.

Most of the large-scale circulation in the sun occurs in the region between the core and the photosphere, inaccessible to direct observation. Even more than the problem areas discussed above, the study of the large-scale circulation of the sun therefore relies heavily on theoretical considerations particularly in the field of magnetohydrodynamics. The few observable properties of the sun associated with the general circulation serve, however, as important checks for these theoretical considerations.

The rotation at the surface layers of the sun has been observed directly and is therefore known with considerable accuracy, although some questions still remain. Recent observations of the solar oblateness, if correct, suggest that the rotation rate of the interior of the sun may be considerably higher than that of the surface layers. Because of its implications, both for the solar circulation and for the understanding of the precession of the orbit of Mercury in terms of the different relativity theories, the question: "What is the solar oblateness?" should be reinvestigated observationally and the results critically examined.

In stars with spectral type later than F4, convective zones are present. These are thicker the later the stellar type. The slowing down of the rotation of the outer layers is enhanced in these stars and in the sun by the angular momentum carried away by the convection-induced solar wind. This poses the questions: "What is the rotation rate of the solar corona, and how is it related to mass exchange in spicules, eruptive prominences, and solar wind?" Because of the magnetic fields anchored in the photosphere, the corona should corotate with the solar photosphere up to the point where the kinetic energy density becomes dominant. The most promising technique in studying coronal rotation remains the examination of the coronal structures (streamers); spatially resolved radar observations of the sun may also prove to be a useful tool.

The latitude variation of the solar angular velocity is thought to be the result of anisotropic turbulent stresses or of the angular momentum transport from pole to equator by meridional currents, by Rossby waves, or by both. Indications that such currents exist resulted from the study of sunspot motion; a study of the associated Doppler shifts is, however, much needed. Pole–equator temperature differences are another indicator for meridional currents. Numerous existing observations of this are, however, inconclusive or contradictory.

The mixing of elements by convection and meridional currents is of major importance for models of stellar interiors and red-giant stellar evolution. The theory of convection can be tested in various ways on the sun. The first, discussed in the previous section, is by the interpretation of the granule and supergranule observations. The surface abundances of certain elements, like lithium and beryllium, are another test. These elements may be created by spallation of C, N, and O nuclei in presolar matter and perhaps by the very energetic protons resulting from solar flares. They are destroyed by the Li(p,α)He reaction at the base of the convection zone if it reaches temperatures above 10^6 K. The lithium and beryllium abundance on stars is actually taken as an indicator of the depth of stellar convection zones. It is therefore important to know the solar abundances of these elements.

In summary, we stress the importance of a strong theoretical effort in the study of the large-scale currents introduced by the rotation, to improve convection theories and to study the interaction of rotation and convection. Refinement of the observations is needed of most of the quantities related to the large-scale circulation. The only new major instrumental development in this context is possibly a spatially resolved radar system for the study of the rotation of the solar corona.

C. CONCLUSION

The four problem areas discussed above include, to different extents, most of the efforts in solar research today. By dividing solar research as was done in this section, we hope to provide a framework within which the various research efforts can be seen in perspective. We give strong support for those efforts that will most likely lead to the most rapid advance in these problem areas. We do *not* assign priorities to the four problem areas, since each of them is indispensible for the understanding of the sun. The four problem areas do distinguish themselves, of course, in various aspects. When viewed, for example, from the point of stellar-energy generation and evolution, problem areas 2, 3, and 4 stand out. Problem areas 1 and 3 are dominant from the point of view of solar-terrestrial relations and stellar atmospheres, respectively. They also distinguish themselves in the relative emphasis on theory and observation, areas 1 and 3 being much more oriented to the latter. The practical difficulty associated with the solar cycle makes problem area 1 a hard one to study during the solar minimum years (1973–1976).

The following section describes the anticipated research programs required to make progress in these problem areas. The understanding of solar phenomena is based on the observation of their properties and their theoretical interpretation. Both theory and observations belong therefore in a *balanced* program in solar research. So do the various components of *theory* and *observations*. Observational research in a *balanced* program should be pursued in *all* regions of the electromagnetic and particle spectrum, since each of these conveys a different message about the solar phenomenon under investigation.

IV. A PROGRAM FOR THE 1970's

A. GENERAL COMMENTS

In formulating a program for solar research for the 1970's, we must bear in mind that we are planning both for the short-term and the long-term future. At this point in the development of solar research a number of problems stand out as most pressing for solution, as discussed in the previous section. We list a sample below:

1. What is the nature of the instability that produces a flare?
2. How are solar particles accelerated to suprathermal energies?
3. How do energetic flare particles interact with the gas and magnetic field to produce radio and x-ray bursts?
4. Where and how is the solar corona heated?
5. How does the large-scale structure of the corona influence the solar wind?
6. How does one account for the low flux of neutrinos from the sun?
7. What is the solar helium abundance?

Several of these questions have been with us for more than a decade and, indeed, since the beginning of modern solar research. However, the last decade has contributed more to their solution than any previous one, and we are encouraged to think that a continuing inventive effort will lead to their eventual solution.

It is upon questions like these, or some aspect of them, that the majority of solar investigators will work, at least for the next three to five years. We must provide adequate general facilities for this work.

In addition, we must encourage exploration beyond our present conceptual limits. It is far harder to do this than to design facilities to answer questions that are clearly in mind. However, we can capitalize on our past experience; a major advance in the precision or resolution with which a solar measurement can be made usually leads to the opening of a new field of research or the clarification of an old problem. We must, therefore, plan a gradual refinement in the power of our instrumentation over the coming decade. In this conclusion, we strongly support the general approach to program planning of the Astronomy Missions Board.

A proper balance among the various components of the solar program must be encouraged. Rocket and satellite experiments cost more, by far, than anything else in the solar program. In a time of budgetary restrictions, there is a strong temptation to cut back this component. However, as we have pointed out in the previous section, some of the most stunning discoveries in recent years have been derived from the rocket and satellite program. We look forward to continued progress from space observations. In addition, observations from balloons, aircraft, and the ground can be expected to complement space observations in an essential way, and at relatively low cost, in the coming decade.

The importance to space observations of concomitant observations from the ground has increased enormously in the past few years, as the angular resolution obtainable from space has become comparable with that obtained from the ground. This new development permits space observations to aid in the interpretation of ground observations, as well as vice versa. We react with the utmost approval to NASA's announced intention to fund ground-based solar research in support of the Skylab Apollo Telescope Mount experiments and hope that the principle so established will be extended to other space experiments.

Finally, theoretical and experimental studies in atomic and molecular physics, spectroscopy, radiative transfer, plasma physics, hydrodynamics, and other areas relate directly to the interpretation of solar observations and are vital to the ultimate success of the entire program.

RECOMMENDATION A–GENERAL

Each of the major components of the solar program–ground-based, space, theoretical, and experimental–is indispensable to the entire program. We recommend that plans for future expenditures in solar astronomy take account of the needs for a well-balanced program in these interrelated fields.

B. TECHNIQUES

We will try to outline the type of instrumental developments necessary for the exploration of the problem areas described; while mostly directed to the issues raised, they also represent a need to explore unknown areas. Both goals contribute to progress.

A natural subdivision exists in techniques. We rely heavily on the position paper of NASA's Astronomy Mission Board (AMB) ("A Long Range Program in Space Astronomy") and on the report of the Geophysics Research Board's Committee on Solar-Terrestrial Research (*Physics of the Earth in Space: The Role of Ground-Based Research,* National Academy of Sciences, Washington, D.C., 1969).

Section IV. C lists the expenditures associated with the instrumentation.

1. *Xuv and euv* (< 3000 Å)

The Solar Panel of the AMB (pp. 127–176) thoroughly discusses this range of the solar spectrum. It proposes to improve the three presently limiting factors of spatial, spectral, and temporal resolution. Our Panel views the improvement of spatial resolution as the most significant. The following facilities for space observations of the sun are or will be shortly available: rockets, Orbiting Solar Observatories (OSO's), and the Skylab Apollo Telescope Mount (ATM). These serve different functions. Rockets carry small payloads (50–100 lb) for a few minutes at a relatively low cost. They cannot be used to study variations of the sun longer than a few minutes. Orbiting Solar Observatories study solar variations at time scales up to more than a year. They carry limited payloads (250 lb) and until now have been limited to a spatial resolution of 30 sec of arc. The ATM-A in Skylab is a man-operated spacecraft, whose main advantage over the OSO's is its larger scientific payload (~2000 lb) and high pointing accuracy (5 sec of arc).

The AMB recommends for the next decade: (1) a continued strong rocket program (12–15 rockets/year); (2) continued use of the OSO's (one each 18 months); (3) development of a 5-sec of arc, high-payload spacecraft of the ATM type, possibly unattended by man (one every 2 to 3 years); (4) design the flight of a 1-sec of arc solar space observatory. The average yearly expenditure was estimated at $57 million to $113 million for low and high funding programs, respectively.

The OSO-I, -J, and -K missions have been authorized, and OSO-I is presently funded for launch in late 1973 or early 1974. The OSO-I and -J payloads are designed primarily to study phenomena on the sun during solar minimum, whereas OSO-K will probably combine studies of the quiet sun with observations of active regions and flares during the rise of the new active cycle in 1976 or 1977. We visualize this program, and its continuation through OSO-L, -M, and -N during the 1977–1982 sunspot maximum, as the backbone of the solar space effort in the 1970's.

From recent experiments with the OSO spacecraft, the fundamental design may provide substantially higher pointing stability and accuracy than anticipated. Table 5.1 summarizes some of the basic characteristics of the OSO series projected by NASA. A pointing stability approaching 1 sec of arc later in the OSO-I, -J, -K series is not unreasonable.

TABLE 5.1 Revised OSO Characteristics

	Old OSO's	OSO-H (1971)	-I, -J, -K (1973–1976)	-L, -M, -N[a] (1977–1982)
Pointing stability (sec of arc)	±20	±2(?)	±1(?)	±0.5(?)
Telescope resolution (sec of arc)	30–60	10–20	1	?
Length, pointed section (m)	1.1	1.5	1.5	3(?)
Weight, pointed section (kg)	50	100	100	250(?)
Area, pointed section	8" × 8"	12" × 12"	15" × 15"	30" × 30"(?)
Data rate (bits/sec)	200–800	800	6400	25,000(?)

[a] The figures in this column only indicate in which direction the OSO series can and should develop. They do not refer to firm specifications.

This unexpected development has far-ranging implications for the solar space program. It means that some shorter (< 2 m) experiments projected by the AMB for the 5-sec of arc observatory can now be accommodated by an OSO in the I, J, and K series, except that these OSO's cannot carry a heavy payload (>100 kg). To exploit the full capabilities of the OSO spacecraft, we suggest that NASA study, for the OSO-L, -M, and -N series, the development of a large shroud to accommodate instruments 3-m long and increase of the data-handling capabilities of the OSO series.

The OSO could carry successively refined instrumentation for the investigation of chromospheric and flare structure and dynamics. Line profiles in the extreme ultraviolet, with moderate spectral resolution, could be obtained as a function of time for small regions on the sun. In addition, high-resolution and narrow-band euv spectroheliograms could be derived to investigate the chromospheric structure, the supergranulation network, and fine structure of prominences. The improved OSO's would also measure xuv fluxes from regions smaller than ever resolved before. They will study dynamic flare spectra and coronal active regions in the xuv. In addition, the absolute photometric accuracy of measures of the xuv spectrum can be improved. It will be possible to map the solar spectrum to shorter wavelengths and make reliable intensity estimates of fainter lines.

OSO-L, -M, and -N, to be flown during the next solar maximum 1977–1982, should carry payloads to study simultaneously specific objects of solar research. Three examples of payloads for which detector arrays should be considered to study different aspects of the sun are

(a) *Active Region Payload*

(i) X-ray spectroheliograph (5–20 Å, $\lambda/\Delta\lambda \approx 10^4$, spatial resolution better than 5 sec of arc, time resolution ≈ 30 sec)

(ii) Far uv spectroheliograph (170 Å and up) to study line profiles and Doppler shifts ($\lambda/\Delta\lambda \approx 2 \times 10^4$, spatial resolution better than 5 sec of arc, time resolution ≈ 30 sec)

(b) *Flare Payload*

(i) High temporal resolution (<0.2 sec) hard x-ray (20–500-keV) observations with little spatial resolution and $\lambda/\Delta\lambda \approx 10$

(ii) Imaging hard x-ray telescope (40 keV) with best possible spatial (5 sec of arc?) and temporal (5 sec?) resolution

(iii) Hard x-ray polarimeter (10–100 keV) with good time resolution (<0.5 sec)

(iv) Soft x-ray (5–20 Å) spectrograph for the study of a few selected spectral lines with high time and spatial resolution (≈ 10 sec, 5 sec of arc)

(c) *Quiet Region Payload*

(i) 20–30-in. telescope in pointed section used as a feed for various experiments mounted in wheel (or other OSO section)

(ii) Auxilliary instrumentation fed by the main telescope to measure line profiles, velocities, intensities, and perhaps magnetic fields in the spectral region 1000 Å up to the visible temporal resolution ~ 15^s, $\Delta\lambda/\lambda \approx 10^5$, spatial resolution ≈ 1 sec of arc

Skylab ATM-A, scheduled now for mid-1973, will obtain solar xuv images, spatially resolved spectra, and profiles of fainter lines and lines at shorter wavelengths than any now available, all as functions of time. These observations with a spatial resolution of 5 sec of arc and time resolution of a few seconds, are among the most exciting prospects in solar physics today.

As pointed out in Section II, the investigation of xuv dynamic flare spectra will ultimately require large and heavy telescopes in space. In addition, we can foresee the scientific need for investigating 1-sec of arc coronal and chromospheric structures in the euv and xuv. We must plan for a 1-sec of arc observatory to be flown later in the 1970's capable of carrying a heavy payload. Because it will carry large-aperture (>30 in.) telescopes, it removes the flux limitations for xuv and euv observations. It will be possible to make measurements of line profiles and their temporal variations from small regions on the sun and to obtain high-resolution spectroheliograms in order to study the fine structure. These capabilities are ideal for attacking problems of flare fine-structure, coronal active-region structure, and the dynamics of chromospheric structure, including the chromospheric interface.

In addition, the 1-sec of arc observatory will provide the capability of exploring entirely new solar phenomena with simultaneous spatial, spectral, and time resolution beyond anything available or foreseen. Freedom from flux limitations would open up the possibility, for example, of x-ray polarimetery, leading to the measurement of coronal magnetic fields.

The AMB report has discussed the tradeoffs between manned servicing of such an observatory and reflight of additional observatories. We feel that it is more economical to operate solar instruments in an unmanned mode, with control by a scientist on the ground. However, if astronauts become available to maintain an observatory, its operating life could be extended and it could be modified progressively to accommodate changes in instrumentation.

We foresee a continuing need throughout the 1970's for a strong and expanded rocket program to supplement the satellite program. Rockets have many advantages. They have the flexibility to carry out exploratory experiments inexpensively, allow quick response to new discovery, and permit wide participation of the scientific community. Rocket payloads are recoverable, usually in reusable condition, allowing photographic recording with its enormous information storage capacity, not available in OSO experiments. A recent example was the flight by a team of scientists from the Culham Laboratories, University College of London, University of York, and Harvard University during the March 7, 1970, solar eclipse. They obtained data on the chromospheric flash spectrum at a resolution (supplied by the lunar limb) of about 0.5 sec of arc in the extreme ultraviolet. We strongly endorse the AMB recommendations for experiments most usefully carried out from rockets during the next decade.

RECOMMENDATION B–SPACE

We *recommend* the following priorities in the solar space program:

1. A strong rocket program and a continuation of the present OSO series through OSO-L, -M, and -N even at the lowest funding levels for the program.

2. A start of the design of a heavy-payload, 1-sec of arc pointing capability vehicle to be flown during the next solar maximum (~ 1980). The existing ATM hardware (ATM-B or Skylab II) could perhaps be used in a one-time mission of this type.

2. *Optical* (>3000 Å, <1 μm)

Recent years have seen the completion of a number of new ground-based solar telescopes with improved spatial resolution that is a result of better site selection and improved telescope construction. The new Aerospace and Sacramento Peak telescopes have eliminated internal seeing, often a limiting factor in solar telescope performance. First results from these telescopes show a significant improvement in performance. The remaining limit is the external atmospheric seeing. The logical development in ground-based optical instrumentation is a full exploitation of these new instruments and of the Kitt Peak National Observatory's solar telescope, with its associated modern auxiliaries, and a strong effort to better understand the physics of atmospheric seeing. What can the ultimate resolution of a ground-based telescope be? Can a large ground-based vacuum telescope with 50-in. aperture be expected to give or occasion diffraction-limited images? This question is urgent and important because of its relevance to future efforts toward optical exploration from space. In case it turns out that such a resolution can, indeed, be obtained, we most strongly *recommend* the construction of a large new optical vacuum telescope.

However, the best resolution obtained so far, after many years of strenuous efforts, by ground-based instruments corresponds to the diffraction limit of a 10-in. aperture telescope. We suspect, therefore, that we must use balloon or space observations to make a significant improvement in image quality. The AMB suggests that a 25–30-in. aperture optical telescope may be part of one of the follow-on 5-sec of arc missions after ATM-A. The short exposure times to study the photosphere would make pointing accuracy of 5 sec of arc acceptable. Internal guiding to correct for pointing variation should also be considered. Such a telescope would give a resolution between 0.1 and 0.2 sec of arc and be an enormous forward step from the 0.5 sec of arc only rarely obtained today.

Continuous development of new ways of image analysis is necessary to make the fullest use of the existing observing facilities. Some examples are (a) the application of Fourier spectroscopy techniques even in the visible region of the spectrum, (b) the development of video systems and other two-dimensional image detectors with a much increased sensitivity, (c) the construction of new types of narrow-band (<0.1 Å) filters–some fully, rapidly tunable over many thousands of angstroms, and (d) the improvement of magnetographs and the introduction of very sensitive Stokes polarimeters. These developments are to be strongly encouraged.

RECOMMENDATION C–OPTICAL

1. Major scientific problems require 0.1–0.2-sec of arc resolution in the visible for their solution. We *recommend* parallel efforts to acquire this resolution in space (from the 1-sec of arc vehicle discussed in Re-

commendation B) and on the ground (with newly designed telescopes). To ensure the best utilization of a new ground facility, a survey to find the best solar site should be started immediately.

2. The capability of existing telescopes should be enhanced by the development of new techniques for storing and analyzing images and for measuring magnetic and velocity fields.

3. Specialized, relatively small and inexpensive telescopes often make major contributions to optical solar research. The construction of these instruments should therefore be funded.

3. Infrared (1 μm $< \lambda <$ 1000 μm)

The large Kitt Peak solar telescope has turned out to be an extremely useful instrument for the study of the near-infrared solar spectrum (1–20 μm). Its large collecting area and its high optical quality make it a beautiful instrument for high spatial and spectral resolution studies of the sun, of the solar granulation, and of sunspots. We *strongly recommend* that much of the time on this instrument be dedicated to infrared studies and that a strong development program for image analyzing equipment be pursued.

Beyond 20 μm, up to 1000 μm, the earth atmosphere severely absorbs the solar radiation. There are four ways to combat this absorption: rockets; high-altitude balloons; aircraft; and high mountain, dry observations.

(i) High-altitude (25-km) balloon observations with a 16-in. telescope are needed for absolute radiometry in the 20–800-μm region with moderate spectral resolution. The resulting data will help in the definition of the photosphere–chromosphere transition region; in particular, we expect to see the temperature minimum near 300 μm.

(ii) The NASA 36-in. telescope planned to be flown on the NASA-Ames C-141 aircraft at 12–17 km will be an excellent tool for high spectral resolution studies of the sun in the infrared. We *recommend* that solar research plans for this instrument be supported.

(iii) The atmosphere has some windows in the microwave region (300 μm, 450 μm, 700–2000 μm) that can profitably be used for solar and stellar studies. We suspect that in this region on the sun there are a number of lines, especially in the solar corona. A 2-m or larger telescope, located at a high, dry mountain site could observe these features. This type of telescope would probably be very similar to instruments being planned for stellar infrared studies. We *strongly recommend* that in the design of telescopes for the stellar infrared a reasonable effort be made to increase their capability for solar observations.

The next decade will see much progress in infrared instrumentation, the development of new detectors, Fourier spectroscopy (both spatial and spectral), two-dimensional detectors, and Fabry-Perot filters.

The near infrared can profitably be used for observations now made in the visible: (a) At 4 μm we expect the sky brightness to be very much lower than in the visible, and an infrared K-coronameter for this wavelength might be desirable; (b) Zeeman splitting increases as λ^2; some solar absorption lines at 2 μm would, therefore, give magnetic splittings ten times those in the visible for magnetographic observations; (c) atmospheric seeing is better and higher resolution can be obtained with large apertures and better detectors.

RECOMMENDATION D–INFRARED

1. We *strongly recommend* that support for observations in the infrared from rockets, balloons, aircraft, and ground-based telescopes (e.g., KPNO solar telescope) be continued at the present level or higher.
2. Research and development of new techniques for examining infrared radiation should be supported strongly.
3. The wavelength region around 1 mm offers an exciting new frontier for solar astronomy. In order to exploit this new regime, a large (>2 m) submillimeter (0.3–2 mm) dish should be constructed at a high-altitude site.

4. Radio

Important advances in equipment now provide high resolution at all radio and radar wavelengths, of high time resolution especially at long wavelengths, and complete polarization observations of solar activity at all wavelengths. In particular the photoheliograph array at Culgoora (Australia) is innovative technologically and extremely rewarding in terms of its first results. This instrument operates only at a single frequency, 80 MHz. Similar instrumentation operating simultaneously at a variety of radio frequencies, like the multifrequency radioheliograph presently under study at the University of Maryland, should be supported.

Radar detection of the sun is possible only at decametric wavelengths. At shorter wavelengths, at frequencies higher than ~40 MHz, the radar signals are absorbed in the coronal plasma. The only synoptic program has operated at the frequency of 38 MHz. This series of observations has unfortunately been terminated. These programs were extremely limited in spatial resolution of the reflected pulse structure. For solar radar astronomy, to a much greater extent than for planetary radar astronomy, it is essential for a proper interpretation of the returns that direction of arrival be measured continuously. This implies a new kind of solar radar in which multiple beams resolve the sun on a spatial scale small relative to the sun's overall radar diameter of $\sim 1°$.

Such a radar array has not been proposed because radar astronomers have not felt that the necessary funding would be available. It is difficult to estimate the costs involved; a project would probably cost $10 million or more.

At very short solar radio wavelengths, the sun is an extraordinary object, especially at times of solar flare activity. In the range shortward from 10 cm, solar bursts show variations similar to those of the extremely hard x rays at the other end of the electromagnetic spectrum. High-resolution studies of solar radio emission on spatial scales of 5 sec of arc or better with time scales of 1 sec or better should reveal the structural properties of the regions where solar particles are accelerated. While the cost of such an array is high compared with most radio-astronomy projects, with the exception of the Very Large Array ($30 million to $40 million), the scientific results and costs are comparable with those for a study from space in the extreme x-ray spectrum. Such a radio telescope consists of 1100 ten-foot dishes spaced at 40-ft intervals around a circle 4 km in diameter. The ratio of the size of the sun to the beamwidth of individual dishes at 10 cm is 4:1. The dishes should have polarized receivers and be capable of operating at 5 cm and 2.5 cm. The short-wavelength capability is inexpensive because of the small size of the dishes.

RECOMMENDATION E–RADIO

1. The study of the interaction of energetic particles, solar plasma, and the solar magnetic field can be carried out effectively (and cheaply) by means of metric and decametric observations of moderate spatial resolution (1–5 min of arc) and high time resolution. We *recommend* the construction of a radio array to undertake such a study.

2. From a scientific standpoint, radar observations hold great promise. We regret that financial considerations have led to elimination of this subdiscipline. We *recommend* that facilities enabling spatially resolved radar observations (5 min of arc) be funded.

3. The acceleration of particles in the solar atmosphere may be studied at comparable cost with x-ray observations from space and with high-resolution (5-sec of arc) polarimetric observations in the centimeter wavelength band. These data are naturally complementary, and therefore both techniques should be pursued simultaneously.

5. *Neutrinos*

The logical development is to increase the sensitivity of the existing neutrino telescopes. The present experiment by Davis using the $^{37}Cl(\nu,e^-)^{37}Ar$ reaction can be increased in sensitivity by a refined detection of the ^{37}Ar nuclei. Other experiments are possible that will detect the less energetic but more numerous neutrinos produced in the other reactions in the solar interior (e.g., the p–p neutrinos). Such experiments could use the neutrino–electron scattering or the $^{7}Li(\nu,e^-)^{7}Be$, $^{87}Rb(\nu,e^-)^{87}Sr$, or $^{71}Ga(\nu,e^-)Ge$ reaction. Each of these has a particular energy sensitivity so that different experiments will result in an estimate of the neutrino energy spectrum, which, in turn, is related to the rates of the different energy-production processes in the solar interior. The neutrino–electron scattering is of interest because it is capable of giving both the neutrino energy spectrum and neutrino direction.

RECOMMENDATION F–NEUTRINO

We most *strongly recommend* continued support for neutrino observations of the sun and for attempts at a theoretical interpretation of these observations. These experiments and the concomitant theoretical effort promise an exceptionally high scientific return for the modest financial investment required.

6. *Particles*

To understand more fully the mechanism of solar particle acceleration, the propagation of solar cosmic rays, the nature of the solar wind, and the relationship of the composition of energetic solar particles and the solar wind to that of the sun, it is necessary to know the time dependence of the energy spectrum of electrons, protons, and heavier nuclei *as a function of position in the solar system.* Variations in intensity and spectra are expected as a function of position angle with respect to the sun, distance from the sun, and the conditions of the interplanetary magnetic field, as well as time.

An ongoing program for the study of solar particles should concentrate on these aspects. In particular, particle detectors with a combined energy range extending from the lowest possible detectable energy to at least 10^3 MeV/nucleon and with the ability to separate particles of different types should be flown simultaneously on different satellites. These satellites could be in the Pioneer, IMP, or outer-planets series. Good aspect and magnetic-field data should be provided by these satellites. Very detailed measurements on nuclear composition of energetic solar particles and of the solar wind, extending current measurements to a wider range of atomic mass, should also be made on some satellites and with instruments on sounding rockets. A full coverage of the sun and of solar flares in the radio, optical, and x-ray range of the spectrum should be obtained, since such a full coverage of a particle event is of the greatest importance in the interpretation of the particle acceleration and propagation mechanisms.

RECOMMENDATION G–PARTICLE

The study of both particle generation and propagation is very relevant to both solar physics and solar-terrestrial relations. The present vigorous effort in the study of solar particle radiation should, therefore, be continued.

7. *Laboratory and Theoretical Studies*

The facilities required for laboratory and theoretical investigations in support of solar observations are relatively inexpensive compared to large-scale solar instrumentation. Nevertheless, they must be included in any balanced program. We have seen in Section II how the interpretation of solar observations of every kind depends eventually on a theoretical framework and the results of laboratory measurements.

Some examples of areas in which theoretical research is needed and should be funded are radiative transfer, hydrodynamics (including convection), plasma physics, and theoretical atomic physics. The theorist needs primarily access to large computers, and he needs contact with observers. Funds for computer costs and travel should be provided. Laboratory programs devoted to fundamental spectroscopy, the determination of atomic transition rates, and measurement of plasma properties supply the basic atomic parameters that are needed in the interpretation of solar data. Experimental programs such as these require support for equipment and staff.

Finally, there is a large class of solar problems that may only be satisfactorily explained after more laboratory experimentation on the fundamental physics has been carried out. We think in this connection of convection, shock formation, radiation from shocks, and the interaction of plasma waves with fast particles. Granting the fact that it is often extremely difficult to scale solar physical conditions into the laboratory, we believe that far more could be done than is being attempted–mainly for lack of support. We, therefore, *strongly recom-*

mend that innovative programs in laboratory astrophysics in these areas be encouraged and supported.

RECOMMENDATION H–THEORY AND LABORATORY ASTROPHYSICS

Solar research proceeds through a strong interaction between theory and observations. We have up to now emphasized the latter because the facilities they require form the major expense of the program. However, we recognize the importance of a strong theoretical effect in the future. We, therefore, *recommend* that stronger support to this, and to related laboratory astrophysics, be given.

C. A PROGRAM FOR SOLAR RESEARCH

On the basis of Recommendations A through H, the solar panel recommends two programs for solar instrumentation: the so-called minimum and optimum programs. The minimum program contains only those developments that are required to keep the different subdisciplines of solar research in active existence. Elimination of any parts of this program would inevitably lead to a serious stagnation or, at worst, an elimination of a subdiscipline. The optimum program contains the developments that the Panel believes to be essential for a full, aggressive exploration of the sun.

1. Minimum Program

The minimum program includes (*not* in order of relative priority):

(a) A space program consisting of a strong rocket program (~$4 million/yr) and a continuation of the present OSO series through OSO-L, -M, and -N (~ $30 million each) to be flown during the next solar maximum (1977–1982).

(b) A strong effort to increase the spatial resolution in the optical region of the solar spectrum. This should consist of a survey to find the best solar site (~$0.5 million) followed by the design of a large (≈50-in. aperture) facility (~$0.5 million) only if this site promises to permit significantly better observations than those made with existing telescopes.

(c) A continuous updating of the capabilities of existing telescopes for optical and infrared observations, as well as the construction of small specialized telescopes for this spectral region. The updating includes the incorporation of new techniques to detect, store, and analyze images (~$2 million/yr).

(d) The use of existing or nearly completed aircraft telescopes and balloons for observations in the infrared (~$0.5 million/yr).

(e) Construction of a radio array for the study of metric and decametric solar radiation with moderate spatial resolution simultaneously at various frequencies and polarizations (~$1.5 million).

(f) A continued strong effort to measure solar neutrino radiation (~$0.1 million/yr).

(g) A study of solar particle radiation, especially with reference to the simultaneous observation at two or more positions in interplanetary space (~$3 million/yr for minimum program, ~$5 million/yr for an optimum program).

(h) A continued strong support for theoretical and laboratory astrophysics.

2. *Optimum Program*

The optimum program (again *not* in order of relative priority) includes in addition to the items discussed in the minimum program:

(a) The development of a large ($\gtrsim$40-in.) diffraction-limited space vehicle capable of carrying a heavy payload (>1000 lb) and of accurate pointing (~1 sec of arc directly, ~0.1 sec of arc with secondary guiding) (~$200 million).

(b) The construction of a large ground-based solar telescope (~50-in. aperture) if the site characteristics discussed in the minimum program can be obtained (~$7 million).

(c) A large-aperture solar telescope (~2 m) for the far infrared (0.3–2 mm) with spectrometer attachment (~$2 million).

(d) A spatially resolved radar (resolution ~3 min of arc) to study the structure of the outer solar corona and the solar wind (~$10 million).

(e) A high-resolution (~5 sec of arc) radio telescope for the centimeter region to study the sources of particle acceleration by means of their gyrosynchroton radiation (~$30 million).

CHAPTER SIX

Theoretical Astrophysics

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I. INTRODUCTION

Astronomy is an observational science, and superficially it often appears that, at this stage in our exploration of the universe, theory plays a comparatively minor role. We show here that this is not the case; in order for progress to be made in exploring and in understanding the cosmos, the role of the theoretician is of major importance.

We do this first by discussing some of the ways in which theory and observation play complementary roles in different areas of astrophysics. We then single out *one* complex area of research—that concerned with our understanding of the stars and the origin of the chemical elements—to illustrate in more detail the role of theory in astrophysics. We have *not attempted to cover all branches of theoretical activity,* because such a catalogue would inevitably be out of date before it was published.

In Section IV we address ourselves to some organizational problems. Finally, in Section V, we make certain recommendations concerning requirements for theoretical astrophysics in the decade ahead. Appendix A, on laboratory astrophysics, is based on material supplied by the contributors to the panel.

II. THEORETICAL METHODS

Once upon a time, the optical astronomer observed the sky because he was interested in the strange objects to be seen there. This type of activity produced a variety of catalogues of facts, usually accompanied by only a minimal understanding of the facts themselves and of their relations to one another. Only with the development of theoretical stellar astrophysics has it been possible to achieve an understanding of most of these objects—stars. Today similar observational activity is taking place at radio, infrared, and x-ray wavelengths and, to a certain extent, in extragalactic optical astronomy. Our present understanding of these sources, however, remains woefully inadequate and clearly requires a major theoretical breakthrough of the type witnessed in the 1920's and 1930's in the study of stellar structure and stellar atmospheres. Despite our lack of understanding of these new phenomena, the data are being used for a variety of purposes. Much of the current activity in extragalactic astronomy at optical, infrared, and radio wavelengths is directed toward deciding which (if any) of the theoretical models of the universe is correct. We wish to emphasize that (a) this is a rather dangerous approach to make without any understanding of the physical processes involved and (b) the whole effort would have no meaning whatsoever outside the framework of the theory of general relativity and the associated cosmological hypotheses. We shall now attempt to analyze the ways in which theory interacts with observation to advance our knowledge of the universe.

In certain cases it is possible to develop the consequences of the known laws of physics and to make theoretical predictions that are subject to observational test. The prediction of stable stellar configurations at nuclear densities—neutron stars—is a classic example of this type of theory. Another is to be found in the field of cosmology.

Starting from Einstein's general theory of relativity and postulates of isotropy and homogeneity, various classes of cosmological models were proposed by Einstein himself, Friedmann, and Lemaitre. These all assumed baryon conservation and, as a consequence of Hubble's discovery of the universal expansion, require evolution from an initial high-density phase. Indeed, Gamow predicted the existence of a microwave background and a substantial primordial helium abundance on the basis of these models. A radically different approach—the steady-state universe—was proposed. This cosmology dispensed with the idea of an initial singularity and substituted the concept of continuous creation of matter to maintain an approximately constant mean density. It is largely within the framework of these theories (hypotheses) that observational work on radio source counts, red shifts of quasi-stellar objects and galaxies, helium abundances in old stars, the x-ray background, and, above all, the microwave background radiation has found both its inspiration and its justification.

Theoretical predictions of the type outlined above are of particular importance in astronomy, which is an observational rather than an experimental science. Predictive or abstract theory that is concerned with the consequences of physical laws is thus immensely productive in an astronomical context and can lead to lively interaction with the observational aspects of the subject.

It would, however, be unfair to suggest that theory usually plays such a guiding role in astronomical endeavor. Indeed, more often, theories are motivated by discoveries rather than vice versa. As a result, the second major role of theory in astronomy is that of interpretation. For example, with the advent of stellar spectroscopy in the latter part of the last century, immense projects of spectral classification were undertaken (the Henry Draper Catalogue, for example) without any real understanding of how these spectra were formed. Only much later did the work of Karl Schwarzschild, Eddington, Milne, and others on radiative transfer in the surface layers of stars clarify the physical interpretation of the assigned spectral types and point the way to the possibility of determining element abundances from stellar spectra. Out of the original qualitative interpretation has grown the subject of quantitative abundance analysis, which, in turn, has provided us with most of our detailed knowledge on the cosmical distribution of the elements. A second example comes from the field of stellar evolution. The general features of the Hertzsprung-Russell diagram (essentially a plot of luminosity versus temperature) had been known for many years before a theoretical interpretation in terms of nuclear processing in unmixed stars could be given. Again, this qualitative explanation opened the way for using the results of stellar-evolution calculations to provide us with quantitative information about stars and clusters. In principle, the method can be used to determine at least overall abundance characteristics, stellar masses, and hence stellar ages. Indeed, the stellar-evolution theory provides us with the only widely applicable method of dating stellar systems. In each case a qualitative understanding of existing data has led to exciting prospects of using the new knowledge as a tool for studying further properties of the cosmos. The latter diagnostic application may be considered as the third major role of theory in astronomy.

For many astronomical phenomena (quasars, infrared and radio sources, x-ray objects, and maser action in interstellar clouds, to name but a few), we cannot at present claim even a satisfactory qualitative interpretation of the nature of the objects involved. We are confident, however, that as our understanding increases these objects will likewise offer prospects of probing still further the mysteries of the universe.

III. SOME ACTIVE AREAS OF INVESTIGATION

Much of modern astrophysics is concerned with three major problems of cosmogony. They are the origin and formation of galaxies, the origin of the chemical elements, and the problems of star formation and evolution, with special application to the origin and evolution of the solar system and the origin of life.

The problems associated with an understanding of the origin of the galaxies are particularly difficult because they cannot be separated from cosmology. Unless we know how the universe began, it is exceedingly difficult to make progress in this area. While it is an active theoretical field of research at present, it suffers from the complication that the major cosmological questions still remain unsolved.

We shall therefore concentrate here on the problems of the formation and the evolution of the stars and the related problem of the origin of the chemical elements. These topics have been some of the most productive and interesting themes in astronomy over the last several decades, and we believe that further substantial progress is likely in the next decade, when modern observational techniques (infrared, optical, and radio) will be combined with theoretical work in stellar, interstellar, and galactic astrophysics.

A. STELLAR ASTROPHYSICS

Here we review some of the theoretical problems in stellar astronomy, including under this heading studies of stellar atmospheres, interiors, and pulsation. These are fields in which a reasonable theoretical understanding has been achieved, at least for the normal stars, and in which theory is increasingly being used as a diagnostic tool. In each case information can be obtained on the distribution of the elements. In addition, studies of stellar interiors are vital if we are to understand how the elements are produced. Similarly, investigations of mass loss, both explosive and steady, are necessary to understand how processed material can be recycled to the next generation of star formation.

The most direct way of determining stellar abundances is through the study of stellar atmospheres. The basic assumption is that the surface abundances are typical of the star as a whole before nuclear processing commenced in the interior. A considerable degree of success has been achieved in this field, at least for moderately hot main-sequence stars, for which the simplifying assumption of local thermodynamic equilibrium holds reasonably well. The rapid advance in this area must be attributed, at least in part, to the advent of large computers. The cooler stars, however, still present a major problem, both because of the complications produced by the huge number of lines in their spectra and because convection, which is not well understood, carries a substantial amount of energy, at least at moderate and high optical depths. Even for the hotter stars, significant problems remain. In the more luminous stars, local thermodynamic equilibrium no longer obtains, so that computation of the local populations of atomic energy levels becomes immensely complicated. In addition, many of these stars suffer radiation-driven mass loss. The less luminous stars, among them white dwarfs and certain main-sequence stars, seem to show evidence of element segregation in the outer layers, which may not therefore give an accurate indication of initial abundances. Such an interpretation has recently been offered for the peculiar A-type stars, which originally provided much of the impetus for studies of nucleosynthesis. Clearly, a major effort is required not only to obtain abundances for stars in stages of evolution other than the upper main sequence but also to take into account the various additional problems such as mass loss, element segregation, and deviations from local thermodynamic equilibrium that are of importance in individual stars. A rejuvenated program of abundance determinations, based on the most modern observing, reduction, and theoretical techniques, should be supported. The major requirements for such an advance, apart from adequate personnel, are accurate observations of line profiles, good computing facilities, and, still more important, laboratory and theoretical determinations of relevant physical quantities.

The basic laws determining the structure and evolution of stars are well known. The purpose of further studies in this field is to determine qualitative observable consequences of these laws for a star during all stages of evolution and, if possible, to invert the process and deduce the interior properties of individual stars from their external appearance. In particular, crude evidence on element abundances, especially helium, may be obtained from the mass–luminosity relation for binary stars, from evolutionary tracks, or from pulsation properties. Such studies also offer hope of understanding how elements are processed and returned, at least in part, to the interstellar medium.

The remaining problems of qualitative understanding occur largely in the very early and very late stages of evolution; they frequently involve dynamic as opposed to the quasi-static states. The whole question of star formation and evolution toward the main sequence is in an unsatisfactory state. It should now be amenable to study, particularly with the vast amount of new observational data from infrared and microwave astronomy on conditions in interstellar clouds, globules, and regions of known star formation. In the very early phases, once pressure support ceases, dynamical, nonspherical collapse will occur, even in the absence of rotation and magnetic fields, thus rendering detailed computations difficult. The problems of angular momentum and magnetic flux must also be resolved if we are to understand how stars are to form at all. Some advance has already been made in these directions, but it is now time for a concerted effort. Once the collapsing object becomes opaque, the subsequent adiabatic contraction increases the density until pressure support returns, and then slow spherical contraction again becomes a reasonable assumption. Accordingly, this part of evolution has received some attention, although it can hardly be regarded as a well-understood phase. It does appear, however, that the conditions on the zero-age main sequences are more or less independent of details of the contraction phase. This is of importance, particularly in studying quantitative effects.

Studies of the early phases of stellar evolution will undoubtedly be much concerned with the origin of the solar system and of life itself. In this context the direct chemical information obtained from meteorites and from lunar samples gathered by the Apollo astronauts may prove invaluable. The role of interstellar molecules and dust in the original solar nebula and the intriguing question of whether such complex configurations survive until the planets are formed will also be a most exciting and rewarding field of research.

The later stages of evolution have been followed by numerous investigators, probably because the initial models are fairly well defined. In particular, substantial progress has been made in understanding (1) supernova outbursts and the distribution of elements produced therein—there is little difficulty in this situation in returning material to the interstellar medium and ultimately into a new generation of stars; (2) mass exchange in close binaries and the formation of white dwarfs and blue stragglers; (3) evolution of planetary nebulae and the production of white dwarfs in single stars; (4) the helium flash and the nature of the horizontal branch. Results are still in a most preliminary state and are only slowly being subjected to observational test. Inevitably refinement will be required. On the other hand, many fields remain virtually untouched; for example, formation of neutron stars and pulsars, angular momentum problems in dense objects (white dwarfs and neutron stars), crystallization, superfluidity and superconductivity in dense objects, the existence and properties of "black holes," and the role of supernovae and pulsars in producing cosmic rays. In those fields that have received more attention, it is becoming clear that even the *qualitative* results depend crucially both on input physical data and the previous evolution of the stars, in some cases back to the main sequence.

With the exception of supermassive objects, the approximate course of evolution of normal stars over most (>90 percent) of their lifetimes is both well known and well understood. This includes the main-sequence phase and the subsequent evolution through at least core helium burning. The effects of composition changes are also fairly well known. In addition, some advance has been made in calculating the effects of distorting forces, for example, rotation, during these phases. The results do not depend crucially on pre-main-sequence history. The study of stellar evolution thus offers the prospect of determining at least the gross properties of stars, particularly in clusters, from observations of their position in the color-magnitude diagram. Such methods yield information on stellar mass, age, and composition. While masses can be obtained directly from binary orbits and composition can be determined more accurately for certain bright stars from detailed studies of stellar atmospheres, the evolutionary theory is far more widely applicable, in particular to the fainter objects. The theory also provides a method of determining the helium abundance in cool stars. Old stars may be expected to reflect more closely the primordial helium abundance and thus provide a test of conditions in the initial phases of the big-bang universe, but because of their low surface temperatures these stars are not amenable to atmospheric helium abundance determinations. Finally, with the exception of radioactive decay dating for material in the solar system, the evolutionary theory currently offers the only prospect of determining the age of stars or clusters. With such a rich harvest in astronomical information apparently in view, these methods have already been applied in a preliminary way, and valuable data on relative cluster ages and compositions have been obtained.

The theory of stellar pulsation, at least as far as Cepheid and RR Lyrae variables are concerned, is now well understood. On the other hand, the long period and irregular variables, which have rather lower surface temperatures, have so far received little theoretical attention. The β Cepheid variables also require further study before even a qualitative interpretation may be regarded as sound. On the quantitative side, the models for Cepheids and RR Lyraes can, in principle, be used to determine the masses, luminosities, and compositions of the stars involved. Preliminary results on luminosities have even been used to determine the distances to certain globular clusters and, hence, to help to establish a cosmological distance scale.

Recently, it has become possible to apply a number of quantitative tests to the theoretical results. These suggest that the present computations cannot be used to yield reliable quantitative results in any absolute sense. These tests include (a) the limits to the actual solar neutrino flux, which fall substantially below those predicted; (b) the determinations of Cepheid masses from pulsation theory, which are approximately one half of those obtained from evolutionary calculations; (c) the mass–luminosity relation in the Hyades, the observations of binary stars yielding lower masses than indicated by theory; and (d) the ages of certain star clusters that appear, on the basis of theory, to be older than the universe. Improvement is required if the vast potential of stellar evolution and pulsation theory is to be realized in determining quantitative results from observational material.

The origin of these difficulties lies partly in the uncertainties of the observations but mainly in the accuracy of the input physics; that is, in the equation of state, nuclear-energy generation rates, convection theory, and opacity, in ascending order of importance. During the main-sequence phase, the results are fairly insensitive to these parameters—and hence to errors therein—but the derived quantities are correspondingly more difficult to determine with precision. In the later stages of evolution, the calculations show ever-increasing sensitivity to input data so that even qualitative features (for example, the existence of a Cepheid phase during core helium burning) may be affected. While relative changes in a given parameter can be determined accurately from the observations, there is little prospect that these are free from considerable systematic error. In certain instances, even qualitative behavior may be altered. We therefore *recommend* that support be given to further studies, both theoretical and experimental and by independent groups, of such fundamental matters as opacity, nuclear-energy generation rates, convection, and equations of state with a view to the full realization of the potential of stellar evolution and pulsation theories. We also *recommend* that work (necessarily of a more qualitative nature) on both early and late stages of evolution should be supported. This should include dynamical and nonspherical effects and will inevitably require more computing power. We would also *recommend* a continuing support for experimental and observational tests of these theories, in particular (1) the attempt to detect solar neutrinos and (2) attempts to derive independent physical parameters, for example, masses from eclipsing binaries and abundances from stellar atmospheres. It is essential, however, that these be coherent comprehensive research programs based on the most modern methods of observing and analysis. Obtaining approximate and isolated data is, in general, no longer viable or relevant unless part of a search for a much broader astrophysical picture. Above all, a stronger interaction between groups involved in interior, atmosphere, and observational studies of stars must be generated and supported. With certain notable exceptions, these endeavors have become isolated from one another—a situation that clearly does not make for rapid progress in our understanding of either the stars or the evolution of the chemical constitution of the galaxy.

B. ORIGIN OF THE CHEMICAL ELEMENTS

The problem of the origin of the chemical elements is one of the most profound in modern astrophysics. The aim is both to determine and to account for the distribution of elements in various celestial objects and in the intervening material. The means are manifold, embracing almost all branches of astronomy from theoretical studies of nucleosynthesis in model universes to direct chemical analysis of lunar samples gathered by the Apollo astronauts. A solution to this problem will involve answers to a host of others, among them the cosmological origins of matter, the conditions in the interstellar medium, the processes of galaxy and star formation, the major sites of nucleosynthesis, the way in which processed material is returned to the interstellar medium, the formation of planetary systems, and the conditions under which life can develop, to name but a few.

Significant strides have been made during the last decade toward constructing a theoretical picture of the early stages of element production in the universe and the changes to the initial abundances brought about by nuclear processing in stars and massive objects. Theoretical progress has come about from the study of detailed nuclear-reaction chains, including a numerical treatment of the time-dependent behavior of element abundances over a wide range of physical conditions.

The general picture that emerges from this study is that helium and possibly some lithium and beryllium may have been synthesized during the initial big bang that gave rise to our expanding universe. Subsequent additions to this initial nuclear material are thought to have been made in the interiors of stars and during stellar explosions. These additions then have been added to the interstellar medium, which then forms new stars. From observations of our own galaxy, it is generally, although not universally, believed that a large fraction of the metal enrichment of the interstellar medium took place rapidly during the latter stages of the collapse of our galaxy to its present disk shape. Subsequent enrichment appears to have taken place slowly over a period of 10^{10} years or so as stars evolved, processed material in their centers, and subsequently expelled this material into the interstellar medium either through supernovae explosions or through the course of less dramatic mass loss. In addition, the interaction of cosmic rays and the interstellar medium may have produced certain amounts of the lighter elements.

C. SUPERNOVA EXPLOSIONS AND EXPLOSIVE NUCLEOSYNTHESIS

One of the most important theoretical developments of the last decade has been the application of numerical hydrodynamics to the study of supernova explosions. When a star reaches an advanced stage of evolution, the central regions will become either very hot or very dense or both. If the star contains more mass than the maximum amount that can form a stable white dwarf star, an instability must ultimately result. This instability will involve the rapid contraction of the central regions, which will lead to a free-fall collapse of the interior, unless a thermonuclear explosion intervenes.

One theory for a supernova ignition mechanism that recently has been studied in detail applies to stars of 4 to 8 solar masses. Following hydrogen and helium burning in the central regions in such stars, a highly electron-degenerate core is formed. Because the mass of the star is greater than that which can be stabilized as a white dwarf star, the degenerate core eventually becomes sufficiently massive so that it must undergo a rapid contraction. This core will be composed primarily of the carbon and oxygen products of thermonuclear reactions involving the central helium. Rapid contraction of the core leads to compressive heating, which eventually ignites thermonuclear reactions in the carbon and oxygen. These thermonuclear reactions undergo thermal runaway, resulting in a detonation, in which the thermonuclear energy is suddenly released in the interior regions. This causes a detonation wave to race through the core of the star, which grows in strength as additional nuclear fuel is suddenly burned. This explosive energy release causes an overpressure in the central regions, which then creates a shock that ejects the outer layers of the star.

There has been some considerable discussion of the behavior of the internal layers in such an exploding model. At first it was thought that the star would be completely exploded, leaving no remnant behind. However, more recent studies tend to indicate that the central explosion will occur at a high enough density so that electron-capture reactions will lower the pressure in the expanding material, thus allowing the central material to fall back and reimplode to small size. This would then leave a neutron star remnant at the center of the exploding supernova.

An impressive advance in our understanding of the formation of the elements by stellar nucleosynthesis has emerged from these studies of supernova explosions, together with studies of the details of the nuclear reactions that occur during the thermonuclear detonation processes. At lower densities, carbon will be incompletely burned, leaving products with masses in the range from neon to silicon with relative abundances strikingly similar to those found in nature. In the same way, oxygen detonated at low densities will leave explosion products in the mass range from silicon through scandium, which also bear a striking similarity to the relative abundances of nuclei in the solar system in this range of the elements. More completely burned material can either form the elements in the mass range from scandium through iron in approximately the relative abundances found in the solar system, or, if the peak temperature reached in the supernova shock wave is somewhat higher, they will form the elements in the range from iron through germanium in approximately solar-system proportions.

Not only does this success of explosive nucleosynthesis theory point strongly to the basic accuracy of the conditions postulated for the production of these elements, but as a result the relative abundances of the elements in these different ranges will provide important observational data that must be satisfied by detailed theories of supernova explosions.

Further detailed numerical hydrodynamic studies of supernova explosions will be greatly assisted by the availability of fast computers with large amounts of storage capacity. This is particularly true if nonspherical effects are to be included in the calculations. One such study, recently completed, indicated that a wide variety of additional phenomena may be associated with supernova explosions in which the exploding star is rapidly rotating, so that it forms a flattened disk upon collapse. Such flattened disks contain differentially rotating material, and any contained magnetic fields can be wrapped rapidly around the rotational axis to form buoyant magnetic bubbles, which may then be rapidly ejected along the rotational axis, taking with them material from deep in the supernova interior. This may be the process in which very neutron-rich material is ejected from the supernova interior, leading to the formation of neutron-rich heavy nuclei by rapid neutron capture. Many more detailed studies with fast computers will be required to evaluate this possibility.

While the general character of element buildup in the production of the light elements, the iron group elements, and the heavier elements seems to be understood, there remain some important unanswered questions in piecing together our ideas of nucleosynthesis. Further progress will continue to depend on active interaction between theory and observation.

Calculations to date have shown that plausible models of element enrichment can be derived from *ad hoc* but seemingly reasonable models of stellar explosions and stellar interior conditions. However, the phases of stellar evolution that lead to instabilities and then to supernovae explosions have yet to be identified unambiguously. Hence the physical conditions during the explosion and the element mix prior to the explosion are still not sufficiently well determined.

Phases of stellar evolution in which gradual mass loss takes place have been identified in part, but to date the incorporation of hydrodynamics into stellar evolution calculations has been at a fairly primitive level. Currently, we have only order-of-magnitude estimates concerning the amount of material contributed to the interstellar medium during various phases of stellar evolution. Progress in interpreting stellar spectra showing evidence of mass loss depends on our ability to solve radiative transfer in spherical models of the stellar atmospheres with general velocity fields. Up to now, this difficult problem has not been tractable, but it appears that during the next decade enormous progress will be made in solving coupled hydrodynamic and radiative-transfer problems. With these solutions, the accurate interpretation of spectroscopic data relevant to the mass-loss problem will become possible. As a by-product of these studies, we should also achieve considerable insight into the physical conditions that prevail in regions of the stellar atmosphere or circumstellar shell that produce the interstellar grains.

Recent calculations of explosive nucleosynthesis have suggested plausible theoretical models for interpreting differences in element abundance ratios. We are thus at a stage where it is necessary to make detailed checks on various aspects of the theory of nucleosynthesis. Further progress requires determination of accurate element abundances for large numbers of stars having different metal-to-hydrogen ratios. This will require observations with high-resolution spectrographs coupled with the most efficient new detectors to obtain line profiles at far-ultraviolet, visible, and infrared wavelengths. In addition, accurate temperature determinations must be made. The much needed data on the composition of the interstellar medium will depend primarily on satellite observations of the atomic absorption lines in the far ultraviolet.

One of the most exciting recent developments in galactic astronomy has been the discovery of a wide variety of interstellar molecules, indicating that the chemical complexity of matter in the galaxy is far greater than had been previously thought. At the present time, different molecules have been identified, ranging in complexity from simple atomic systems, such as molecular hydrogen and carbon monoxide, to complex organic molecules, including methyl alcohol, methyl cyanide, and cyanoacetylene. Many of the molecular regions have been observed to be closely associated with infrared sources and regions of recent star formation.

Progress in our understanding of these objects, however, requires the development of a strong theoretical framework, which, at present, is almost entirely lacking. There are two important areas in which progress is imperative if we are to interpret the observations and to understand the importance of molecule formation and molecular line radiation in astrophysical problems such as star formation, the thermal balance of the interstellar medium, and stellar mass loss.

The extremely low density of the interstellar medium (less than 10^5

atoms cm^{-3}) means that the types of chemical reaction that will be important in the formation of interstellar molecules are substantially different from those involved in laboratory chemistry. The time scale for three-body gas-phase reactions under interstellar conditions is greater than the lifetime of the galaxy, and therefore the chemical abundances will differ radically from what is expected in thermodynamic equilibrium. The short lifetimes of molecules under interstellar conditions mean that they must be formed continuously.

The principal mechanisms for converting atoms to molecules in the interstellar medium are two-body gas-phase reactions and formation on the surface of interstellar grains. Pure gas-phase processes (radiative association along with chemical exchange reactions) can probably produce a substantial abundance of diatomic molecules such as CH, CN, and CO. Much progress is possible in this problem, using the existing techniques of theoretical chemistry to calculate reaction rates. However, formation of molecular hydrogen and complex polyatomic molecules almost certainly involves reactions on the surface of interstellar dust grains. A substantial effort should be made to advance our understanding of surface chemistry under interstellar conditions. Support should be given to encourage work in this promising new area, which combines the disciplines of solid-state physics, theoretical chemistry, and astrophysics.

Molecular radio lines are observed from a wide variety of sources including late-type stars, dark interstellar clouds, and concentrations near H II regions and infrared sources. In all of these cases the radiation originates from regions where physical conditions do not even approximate local thermodynamic equilibrium. In the most extreme cases, the observed lines exhibit the properties of maser radiation (OH and H_2O). The thermal equilibrium of dense interstellar clouds and possibly protostars may be determined by molecular line radiation. Theoretical work is therefore required in the areas of calculating relevant collisional cross sections, radiative excitation rates, and radiative transfer in the absence of local thermodynamic equilibrium. In addition, the physics of maser propagation under astrophysical circumstances must be examined, including the problems of coherence effects, polarization mechanisms, and the isotrophy of the maser radiation.

IV. ORGANIZATIONAL REQUIREMENTS

In the previous sections we have attempted to outline the approach and some of the needs of theoreticians working in some important areas of astrophysics. Clearly, this review is not exhaustive, but we hope nonetheless that it will give some idea of the scope, the importance, and the future prospects for this type of research. In this section we discuss organizational requirements if progress in astrophysics is to be optimized.

A. THE NEED FOR LARGE COMPUTERS

An important part of theoretical astrophysics consists of model building. The models may be quite simple or rather elaborate, as in the computation of a model of a star or of the hydrodynamic development of a supernova explosion. This type of research is basically a form of applied physics. Known physical laws are utilized to compute the behavior of a complicated system. Some of the more important astrophysical systems whose properties must be computed are quite complicated, either in terms of regions of differing composition or in geometry. A realistic representation of a star in an advanced stage of evolution may easily require several hundred separate zones. The same is true of a two-dimensional hydrodynamic calculation with cylindrical symmetry; the number of netpoints, which are equivalent to zones in a one-dimensional calculation, may typically be of the order of 10^4. Already such a calculation strains the largest computers both in terms of speed of computation and in terms of storage capacity. A hydrodynamic calculation in three dimensions without symmetry, to a comparable degree of spatial resolution, would require on the order 10^6 netpoints, which is well beyond any practical possibility on existing computers.

These are only some of the problems in theoretical astrophysics that require access to large and fast computers. Problems involving plasma physics and fluid dynamics require a similar type of approach. There are large numbers of problems in other areas of physics applied to astrophysics, involving the simultaneous solution of large numbers of differential equations, which may involve the repeated inversion of very large matrices.

Since computers are central to further progress in theoretical astrophysics, the Theoretical Astrophysics Panel recommends a major increase in support for this area. Since very few of the computers so far installed at universities are capable of handling the problems described above, special emphasis should be given to upgrading the computers at one or more institutions and making large fractions of the time available to astronomers. At the same time, funds are needed by individual investigators to use the smaller computers at their home institutions, both for the smaller problems that are more conveniently and efficiently handled by direct access to a computer and for the preparation of programs eventually to be run on the larger machines.

B. THE NEED FOR PHYSICAL DATA

The major purpose of laboratory astrophysics is the measurement of quantities required for the physical interpretation of astrophysical observations. The bulk of present activity in laboratory astrophysics deals with the measurements of spectroscopic and plasma-physics quantities. Also, activity in the measurement of nuclear cross sections is needed for theoretical investigations of stellar structure and nucleosynthesis.

There is a theoretical equivalent of laboratory astrophysics. This consists of the computations of cross sections for various nuclear and atomic processes, of various atomic and molecular reaction rates, and of the physical properties of various kinds of physical systems. Of extreme importance here are the computations of opacities and equations of state for stellar interiors. These activities are successful only when there is a reasonably precise theoretical formulation of specific fields of physical knowledge. For many of the relevant fields this is sufficiently true.

The Theoretical Astrophysics Panel would like to stress the importance of both types of activity to progress in our understanding of the universe. In particular, such information is essential if the great diagnostic potential of, for example, the theory of stellar atmospheres or stellar evolution is to be realized.

The present employment difficulties for physicists pose certain problems in carrying out these theoretical activities. A graduate student trained in some field of physics, in which he has obtained results of astrophysical interest, is not really trained as an astrophysicist. Upon obtaining his degree, he will usually seek employment in his chosen field of physics. Unless the field of physics is one in which employment opportunities are available, such graduate students will be inhibited from entering the field in the first instance. If there is to remain a healthy input of physical quantities into astrophysical theory, then the profession will have to find a way to provide employment for the people who carry out these important activities.

The Panel is also aware that much of our current physical data are produced by only a small number of independent groups. Cross-checking (and competition) are thus at a minimum. We feel that the number of independent investigators should be increased and suggest that the best way to achieve this is to give support to physicists in universities who are interested in doing such work, preferably in conjunction with strong astrophysics groups. While gathering together such activity into a single institution may be administratively attractive, it can easily lead to complacency in accepting results and in the rate at which these are produced.

C. UNIVERSITY DEPARTMENTS

In our view, most theoretical astrophysics research in the foreseeable future will be, and should be, conducted in the universities. While the problems of immediate interest are continually changing, the basic requirements for progress remain unchanged. They are

1. The maintenance of a close relationship with physicists, mathematicians, and other scientists in order to take advantage of new developments and to encourage astrophysically relevant research in these fields;
2. The maintenance of a close relationship with observers;
3. The continuation of student training in astrophysical research.

These requirements can be met in a natural and flexible way in the universities.

Because astronomical phenomena are so complex and involve the interaction of so many physical effects, it is inevitable that the astrophysicist should become a "jack-of-all-trades" as far as physics is concerned. He will also require a reasonable applied mathematical competence and, increasingly, a knowledge of physical chemistry. Close contact of the theoretician with other branches of physical science and with observational astronomers therefore seems to us essential. Various administrative arrangements for bringing this about are now being tried out in the astronomy–physics community. Some institutions find it practical and desirable to treat astronomy and astrophysics as part of the physics department; others prefer to have astronomy separated from physics and mathematics but related academically. We encourage close cooperation and the establishment of joint academic programs for graduate students in these areas and urge that astronomy students be required to have a thorough grounding in modern and classical physics.

D. NATIONAL THEORETICAL INSTITUTE

The importance of theoretical work has been recognized in a number of European countries; in practical terms this has usually involved the establishment of Institutes of Astrophysics, loosely attached to universities. Examples are the Institutes in Cambridge and Sussex, England; in Paris, France; in Munich, Germany; and in Oslo, Norway. There has been much discussion both inside and outside of this Panel on whether such a National Center for Theoretical Astrophysics should be established in the United States. Before examining the proposal in more detail, we wish to emphasize (a) that further support for theoretical astronomy is essential if the United States is not to lose the initiative to the Europeans, and (b) that any funds allocated to such a center should not preclude an increase in support to the university astronomy departments.

The Panel believes that the principal purposes of the National Center for Theoretical Astrophysics should be

1. To provide a focal point for theoretical research in the United States;
2. To promote a continuing and lively interchange between astronomers in other departments and institutes;
3. To promote similar interaction with overseas astronomers;
4. To enable first-class postdoctoral research workers to spend a couple of years in a highly stimulating environment before taking more permanent appointments elsewhere.

The Panel is, however, well aware of the dangers in setting up such institutes. In particular, we are most anxious to ensure that no large, inflexible, and permanent institution be set up, replete with huge administrative sections and large continuing expenses. Such an institution would run totally counter to the spirit necessary if a theoretical center is to succeed. For this reason we believe that it would be most unwise for such an institute to provide, at the same time, a national astronomy computing center, although, of course, adequate computer facilities should be available to the staff.

The Panel therefore *recommends* that a center for astrophysics be set up on an experimental basis (e.g., 7 years). It should contain a small more or less permanent staff, perhaps 6 comparatively senior people, with 10 or 12 short-term (up to 2 years) appointments available. In addition, it should have the capacity to accept short-term (up to 3-month) visitors from the United States and abroad. Travel funds for staff and visitors should be readily available. The Director should have a renewable term contract and should be a person of great scientific stature. To avoid the necessity of employing large numbers of support staff and in order to promote maximum interaction with other scientists (and students), the institute should be set up near an existing research university. The site should also be chosen to promote maximum interaction with observational astronomers. If possible, cultural and physical attractiveness should also be taken into account to ensure that good scientists will visit, especially during the critical initial phases. The success of the Institute should be examined after a prescribed (7-year) period, and, if found wanting, it should be closed down. Funding should be via the National Science Foundation. We suggest an annual budget of up to $750,000 on a fixed-term (7-year) contract. In practice, the Director should be given a relatively free hand in running the Institute, and the actual administrative staff should be kept to an absolute minimum.

The Panel should, under no circumstances, support the establishment of a research center with any of the following properties: (1) large permanent staff, (2) a budget that resulted in a drain on university support, (3) a mainly computer-oriented character, or (4) an unwillingness to accept as its main purpose the promotion of research and interchange among astronomers both within the United States and from overseas.

E. THEORETICAL WORK AT THE NATIONAL OBSERVATORIES

The requirements for a possible national theoretical center, in the previous section, are clearly quite different from those at the Kitt Peak National Observatory (KPNO) and the National Radio Astronomy Observatory (NRAO), both of which are concerned with building and operating large and expensive facilities for outside users as well as promoting scientific research among their own staff. The Panel agreed, however, that both KPNO and NRAO would benefit considerably from the presence of a small but energetic theoretical group. This should contain a small permanent staff but should consist mainly of senior and postdoctoral appointments of short (a few months') to intermediate (up to two years') duration. It would have the advantages of bringing about a constant interchange between scientists and hence of reducing the theoretician–observer division in optical astronomy. KPNO possesses a large, rather underused computer, which could comparatively cheaply be upgraded into a first-class computing facility. This could then be placed at the disposal of visiting university scientists and would to a considerable extent relieve, at least temporarily, the pressure on those compelled to use inadequate university computing facilities. Certainly, such a solution is reasonable for a considerable fraction of normal computing needs and would be of great benefit to the scientific atmosphere and standing of KPNO.

V. RECOMMENDATIONS

We now summarize our recommendations, which are based largely on the preceding discussion. No significance is to be attached to the ordering of these recommendations.

1. Since theory plays a crucial role in interpreting observational data

and in suggesting new measurements, we *recommend* that theoretical work be expanded considerably in the next decade, both in absolute terms and relative to the total astronomical effort. As well as continued and increased support to the existing theoretical work, this means that funds should be made available to support promising new areas of theoretical research as they appear.

2. In the foreseeable future most of the activity in theoretical astrophysics will be, and should be, conducted in the universities. We therefore *recommend* that support for research workers in these institutions be continued and increased. We believe that the present system of grants to senior investigators, who, in turn, support postdoctoral workers and students, has proved reasonably successful and should be continued. In view of the considerable and increasing lag of theoretical understanding behind the acquisition of new observational data, it is clear that an increase in the number of theoretical groups and the number of people is desirable.

We *recommend* that the ways in which funds for theory are allocated be assessed in a more flexible way. We *strongly recommend* that small grants and grants for young research workers be made much more easily available than they are at present. We also *recommend* that research grants be renewed or not renewed on the basis of results obtained in the immediately preceeding years and that firmness be shown in reducing support to less productive investigators.

3. We *recommend* that a National Center for Theoretical Astrophysics be set up on an experimental basis. The main purpose of such an institute would be to act as a focal point for theoretical research and to promote interaction between astrophysicists. The institute should have a small permanent staff of approximately 6 members and 10–12 intermediate-term (up to 2 years) appointments. It should also be able to accommodate short-term visitors from the United States and abroad.

It should not be combined with a national computing center for astronomy. It should be located near a major research university and should be situated to facilitate interaction between observers and theoreticians. We anticipate that an annual budget of approximately $750,000 would be required.

4. We *recommend* that small but active theoretical groups be set up at the National Radio Astronomy Observatory and the Kitt Peak National Observatory. The permanent staff should be small, and most of the group should consist of postdoctoral fellows and visitors from universities.

5. We *recommend* that the Kitt Peak National Observatory (KPNO) computer be upgraded to at least the standard of a CDC 6600 and pro-

vide a national computing service for astronomy. It is envisaged that programs for major problems can be developed at the universities but that longer production runs would be carried out at KPNO.

6. We *recommend* that support be continued or increased for research aimed at producing physical quantities of astrophysical interest (such as *f*-values and nuclear cross sections). In particular the number of independent investigations should be increased.

7. On the basis of the arguments presented in Section III and in the reports of some of the other panels, we believe that in the near future observational results of the greater theoretical importance will be obtained in the wavelength range between about 2 mm and 5 mm. It is largely in this wavelength range that the most valuable information bearing on the problems of the interstellar medium, star formation, and the origin of the solar system will be obtained. These problems offer, in our opinion, the brightest prospect of a theoretical breakthrough in the next few years.

Thus, we particularly endorse the recommendations to construct

(a) At least one large 2–5-mm ground-based radio telescope;

(b) A large ($\sim$ 4 m) 2–20-μm ground-based infrared telescope;

(c) A large-aperture airborne submillimeter-wave telescope that is able to investigate objects emitting at wavelengths of $\sim$100 μm. It is also clear to us that the major bottleneck that remains in astronomy is due to the shortage of large-aperture optical telescopes in dark sites. Thus we *strongly recommend* not only that the efficiency of existing large telescopes be improved by the development of modern auxiliary equipment but that one or more new telescopes with apertures greater than 100 in. be constructed forthwith.

APPENDIX A: LABORATORY ASTROPHYSICS

Astrophysical observations, which normally deal with the behavior of matter on a large and complex scale, rely on theory for their interpretation. A major role of laboratory astrophysics is to provide necessary input data for theoretical interpretation of observational data. A second role is to simulate in the laboratory some large-scale astrophysical phenomena that are difficult to treat theoretically, for example, convection. The first role is the more widely known and receives the major fraction of effort in laboratory astrophysics. Examples of fundamental data obtained by laboratory experiments include wavelengths, transition probabilities, collisional excitation cross sections, molecular dissociation potentials, and nuclear-reaction cross sections. Considerable ingenuity has often been exercised in the measurement of such parameters, usually under conditions entirely different from those in the astrophysical situation to which they are applied.

The second role of laboratory astrophysics, simulation of large-scale phenomena, is qualitatively different from the first. Fundamental data do not generally emerge from such work; rather, insight into behavior of complex systems does. Experiments in fluid convection, rotating fluids, plasma instabilities, or hydrodynamic shocks are examples of this type of laboratory work. The early interpretation of the solar Fraunhofer spectrum in terms of the familiar cool envelope of laboratory gas discharges was also in a sense a result of laboratory simulation. Such an interpretation, although it seems primitive today, was useful at the time to comprehend a complex physical system for which the proper analytic tools (radiative transfer) had not yet been developed. Experiments in convection and magnetohydrodynamic instabilities may play a similar role today.

Interpretation of such experiments is often difficult, primarily because of the impossibility of scaling all physical parameters. Nevertheless, the theoretical problems involved in understanding plasma hydrodynamics on astrophysical scales are so staggering that one must welcome whatever insight can be obtained by laboratory experiments, even if they are relatively primitive. A great deal of information has in fact been obtained in recent years from laboratory experiments on convection, plasma instabilities, and other phenomena. Continued efforts in these areas are sure to provide important constraints on theories of large-scale behavior of astrophysical plasmas.

A. ATOMIC AND MOLECULAR PHYSICS

1. *Introduction*

Spectroscopic studies of astrophysical objects represent the chief source of our knowledge about the chemical composition and other physical characteristics of those objects. Such studies of the sun and other stars yield direct information from which stellar temperatures, gas and electron pressures, gas velocities, convection, spatial motions, and internal nuclear processes can be deduced. The results of spectroscopic abundance analyses reflect the nucleosynthetic history of the stars themselves and of the interstellar medium from which they were formed. The initial breakthrough in our understanding of nucleosynthesis and the buildup of heavy elements from lighter seed nuclei by neutron capture was based directly on spectroscopic abundance analyses of stars of abnormal chemical compositions, such as stars containing technetium and the so-called barium stars. Progress in the theory of stellar evolution and in the construction of theoretical stellar models goes hand in hand with progress of spectroscopic investigations into the physics of stellar atmospheres.

Similarly, such advances in astrophysics are ultimately and almost exclusively dependent on the progress of laboratory spectroscopy. The physical structure of astrophysical objects is most reliably deduced by comparing the positions, shifts, profiles, intensities, and other characteristics of their spectral lines with the corresponding atomic data observed in the laboratory. The astrophysical demands for spectroscopic data have always been in excess of the availability of such data from the laboratory.

Atomic and molecular physics and spectroscopy in particular have not, however, since the 1930's enjoyed the lavish support received by other areas of physics. This was partly due to the attitude prevalent among many physicists that all interesting problems in atomic physics had already been solved. Through these years, continued moral support has come from the astronomy community, with some fiscal support through a few government agencies.

The report of the NRC Committee on Atomic and Molecular Physics (B. Bederson, chairman)* shows that this field has suffered a substantial diminution of support in recent years (1968–1970), and that it is in real danger of losing viability. This curtailment occurs at the same time that the interpretation of astrophysical phenomena demands increasingly precise atomic and molecular data.

2. *Atomic and Molecular Data of Astrophysical Interest*

(a) Spectral Line Identification With the extension of astronomical spectroscopy down to the 1-Å region there has arisen a pressing need for line identifications in the part of the spectrum between 1 and 3000 Å. Many of the observed lines in both stellar and solar spectra have not been identified. On the sun these lines are known to arise from regions with temperatures varying from 10^4 K in the chromosphere to 10^7–10^8 K in flares, so that, for example, all stages of ionization of the iron atom up to Fe XXVI have been observed from space platforms.

Determination of accurate wavelengths of the lines of all elements and of all ionization stages in the far ultraviolet and x-ray regions is therefore of the greatest importance. Among the various methods of exciting and

*_Atomic & Molecular Physics_ (National Academy of Sciences, Washington, D.C., 1971).

ionizing gases, beam-foil spectroscopy appears to provide an exceptionally powerful method of studying lines of the highest stages of ionization in gases of very high chemical purity.

(b) Transition Probabilities and Oscillator Strengths The paucity of reliable data on transition probabilities (f-values) continues to be a severe problem to astrophysics, especially in the far ultraviolet and x-ray regions of the spectrum where few experimental f-values are available. The advent of large computers made possible extensive, accurate calculations of f-values for simpler atomic systems. The development of beam-foil spectroscopy and the refinement of techniques for measuring lifetimes of atomic states have attracted additional workers into the field and have extended the range of observation. This method and other direct lifetime techniques such as the Hanle effect, phase shift, or delayed coincidence methods have provided many accurate atomic lifetimes, which often have been useful for normalizing atomic f-values obtained by other means. But these experimental techniques determine only atomic lifetimes and often require additional data on branching ratios before individual f-values can be obtained.

Therefore much of the work of determining oscillator strengths must still be accomplished by the conventional emission or absorption techniques or from anomalous dispersion. Of these three approaches, emission measurements have been most frequently employed, and this will probably continue to be because this method offers the greatest flexibility and is in the most advanced state of sophistication.

For simpler atomic systems, reliable calculations of f-values are now possible and have been undertaken for such lighter elements and isoelectronic ions as the He, Li, Be, B, C, N, O, Na, and Mg systems. The extension of these techniques to more complex atomic systems is under way but will probably require some time. It is partly hampered by the lack of accurate experimental data for comparison. Meanwhile the application of recently detected regularities and systematic trends within isoelectronic sequences and spectral series has yielded accurate data for a large number of transitions.

Both the quantity and quality of many available data are far from meeting the needs of astronomy today. A good case is the accepted value of the abundance of iron in the sun and stars, for which the newer, more accurate f-value data led to an abundance about 10 times larger than previously thought. Similar changes may be expected for a number of heavier elements.

(c) Line Shapes and Line Shifts Line shapes and line shifts are also of

considerable importance in the analysis of astronomical spectra. Of special interest are data on the Stark broadening of hydrogen lines, including the line wings, and data on broadening of the lines of heavier elements by electrons, atoms, or ions, Considerable progress has been made during the last decade in the theory of Stark broadening of hydrogen and helium; line-broadening data for heavier elements are still required if we are to interpret solar and stellar line profiles measured by high-resolution spectrographs in the visible and ultraviolet.

A Data Center on Line-Broadening is being organized at the National Bureau of Standards. Due to the tight financial situation, it seems, however, that not more than a classified annotated bibliography can be expected in the near future. Critical evaluations of line-broadening data will be postponed unless sufficient support is obtained.

(d) Collision Cross Sections A characteristic feature of much astronomical work is the realization that departures from local thermodynamic equilibrium are significant in ordinary stellar atmospheres, as well as in objects such as the solar corona and interstellar gas. This has greatly increased the need for information on collision cross sections of many different kinds. Interest in the laboratories has extended to high energies, where the Born approximation is valid, but astrophysical requirements are critical in the region just above threshold.

There are a number of important types of transition that need study. Transitions induced by collisions with hydrogen atoms between close-lying energy levels of a single spectral term are important, as are electron collision-induced transitions between levels of different multiplicities (intercombination transitions). One also needs information on how these cross sections vary along an isoelectronic sequence of ions.

In only a few cases will it be possible to measure individual cross sections, because of the labor, expense, and technological limitations; it will be necessary to rely heavily on theory, verified and calibrated by means of a few elaborate and accurate experiments. The general aim should be to measure one or two examples for each of as many types of collisional processes as possible and then to develop extrapolation techniques that would permit estimates to be made, at least within an accuracy of a factor of 2, for many other atoms and ions.

As a parallel development it will be necessary to refine still further the mathematical techniques for handling complicated problems in the theory of radiative transfer, particularly to allow detailed treatment of atoms with many energy levels when individual radiative and collisional processes are considered. Finally, it will be possible to combine the atomic data with the transfer techniques and investigate the physical conditions and chemical compositions of stellar atmospheres and other objects with some assurance that the physical input data are correct.

Cross sections for radiative ionization, including their wavelength dependence, are necessary for calculating stellar continuous opacity and interstellar ionization. Measurements are now available on the ionization of most neutral atoms from the ground state; data on SI and ions such as N II, Al II, Si II, and S II are still needed from experiments or calculations. Also needed are transition probabilities from the ground state to autoionization levels and the rate coefficients for dielectronic recombinations.

(e) Molecules Much of what has been said above about atoms applies also to molecules. The detection of complex molecules in the interstellar medium was a great surprise. For example, ammonia, water, hydrogen cyanide, formamide, cyanoacetylene, formic acid, and methyl alcohol have been identified, in addition to diatomic molecules such as CO, CN, and CS. Almost nothing is known about the mode of formation of such molecules at low densities or even why any complex molecules should exist in interstellar space.

For many molecules, whose existence we might be led to suspect, we do not even know the frequencies of the possible microwave transitions for which searches should be made. There is thus a need for additional studies of selected molecules by methods of microwave spectroscopy, not only for transition frequencies but also for dipole moments, hyperfine splitting, lambda doubling, and other molecular data.

When more molecular lines have been identified, studies of the physical processes in low-density gases can be initiated. There has been some interest among chemists in trying to understand selected chemical reactions by means of detailed dynamical studies, and an extension of this work to molecules of astrophysical interest would be worthwhile.

It will probably be necessary to extend work on the effect of solid surfaces (interstellar grains) on molecular formation. We still have not been able to reach a generally acceptable identification of the broad interstellar bands seen in the spectra of hot stars, but processes involving interstellar grains are thought likely. Physicists and physical chemists now studying reactions on very clean surfaces should contribute significantly to the solution of this problem.

We do not yet know what part, if any, is played by electronic excitation of molecules in low-density conditions. Even in the older branches of molecular astronomy, such as the study of molecules in the atmospheres of relatively cool stars, there remain major gaps in our knowledge of dissociation and ionization potentials, oscillator strengths, and other basic data without which any physical interpretation must remain inadequate.

B. LABORATORY NUCLEAR ASTROPHYSICS

1. *Introduction*

Since our attempts to understand the structure and evolution of the universe are based on a combination of theory and observation, the former limited by our finite ingenuity and the latter limited by the inaccessibility of the objects under study, it is imperative to understand as well as possible those astrophysically relevant areas of knowledge that can be verified in the laboratory. One such area is nuclear astrophysics, where the needed physical parameters can be obtained from careful and imaginative study of the relevant nuclear reactions. Indeed, considerable progress has already been made in identifying the nuclear reactions that are the most probable sources of nuclear energy in stars and are responsible for the synthesis of the nuclei present in the universe. There remain, however, many unanswered questions of critical importance.

If we adopt the currently favored cosmological hypothesis that the present structure of our universe is the result of expansion from an early high-density, high-temperature phase (the big-bang hypothesis), the first question we encounter is how much nucleosynthesis occurred during the early stages of the expansion. The rates of the nuclear reactions leading to the synthesis of helium from hydrogen appear to be known already with sufficient precision, considering the much greater uncertainties in estimating the baryon density–temperature relationship and the net density of electron neutrinos. The latter quantity has an especially critical effect on the synthesis of helium; an appreciable excess of either neutrinos or antineutrinos strongly suppresses the synthesis of helium. The synthesis of elements heavier than helium appears to be negligible in the primeval big bang. The presence (or absence) of a universal minimum abundance of helium therefore becomes a crucial test of the big-bang hypothesis and of the conditions under which it occurred.

Following the primeval expansion, it is believed that inhomogeneities in the expanding material resulted in the formation of objects with masses ranging from a small fraction of one solar mass up to galactic mass. The more massive of these may have collapsed to very high density and temperature and then re-expanded in a smaller-scale version of the universal expansion, usually referred to as "little bangs." During these expansions, considerable nuclear activity is to be expected. Theoretical analysis of the dynamical and nuclear evolution of such objects suggests that nuclear abundances comparable with those found in the oldest stars could be produced in this way. There are, however, many nuclear reaction rates that have not been measured in the laboratory, and guesses for these rates have had to be used in the calculations.

Less massive objects (stars) are expected to evolve in a more orderly manner through a sequence of nuclear-reaction chains, at rates that depend critically on the mass of the object. The least massive stars are expected to evolve so slowly that they may still exist to the present day, with composition only slightly altered from the material that formed the star. Nuclear evolution is believed to begin as soon as the temperature at the center of the star becomes high enough to fuse hydrogen into helium, by the proton–proton chain of nuclear reactions or at slightly higher temperature by the carbon–nitrogen–oxygen bi-cycle. Following hydrogen burning, and at a higher temperature, helium burning will occur—the fusion of helium nuclei to form carbon and oxygen, perhaps with traces of neon and magnesium. At still higher temperatures, the processes known as carbon, oxygen, and silicon burning will proceed. Following, or perhaps merging with carbon, oxygen, and silicon burning, the star is expected to explode violently as a supernova, with the ignition of the unburned nuclear fuels in the outer layers of the star. It must be expected that a considerable amount of energy will be released by the nuclear reactions occurring during this terminal phase of a star's existence, and extensive nucleosynthesis will take place.

It is probable that some supernovae explosions will distribute only part of their mass throughout space and that a residual star will be left—a white dwarf, a neutron star, or a black hole with gravitational fields so intense that no radiation can escape.

Within this briefly outlined framework the present status of laboratory nuclear astrophysics and the probable directions for research over the next decade can be summarized.

2. *Status and Directions*

The reactions that lead to the conversion of hydrogen to helium seem to have been checked and rechecked in the laboratory with great care. The recent result obtained by Davis and his collaborators for neutrino emission from the sun, $(1.5 \pm 1) \times 10^{-36}$ capture per second per chlorine atom in the detector, confirms that less than 10 percent of the sun's energy is derived from the C–N–O bi-cycle of hydrogen-burning reactions. However, the measured neutrino flux is about a factor of 5 below the theoretically predicted value based on the proton–proton chain reaction rates. The source of this discrepancy is not known at present, but it seems more probable that it is to be found in the structural models of the sun, possibly in the assumed opacity, than in the nuclear parameters. However, nuclear physicists must be ready to respond if the situation changes with the improvement of experiment and theory in other areas.

The nuclear information for the subsequent phase of stellar evolution—helium burning—is in a less satisfactory state. Recent determinations of the resonance energy for the reaction $3\,^4\mathrm{He} \rightarrow {}^{12}\mathrm{C}$ have reduced the predicted rate of this reaction by more than a factor of 3. The rate of the next reaction, ${}^{12}\mathrm{C}\,(\alpha, \gamma)\,{}^{16}\mathrm{O}$, is uncertain by more than two orders of magnitude, and a major effort is currently in progress to determine this rate experimentally. The stellar rate of the reaction ${}^{14}\mathrm{N}\,(\alpha, \gamma){}^{18}\mathrm{F}$ remains uncertain because of the possible effects of low-energy resonances. It seems likely that the rates of these helium-burning reactions can and will be determined during the coming decade. Further progress cannot be made in stellar-evolution calculations until they are measured.

Our picture of the next phases of stellar nuclear evolution, quasi-static carbon and oxygen burning, remains incomplete. In addition to the great uncertainty about the relative amounts of carbon and oxygen formed by helium burning, stemming from the uncertainty in the ${}^{12}\mathrm{C}(\alpha,\gamma){}^{16}\mathrm{O}$ rate, there is still considerable disagreement among different experimenters as to the extrapolated astrophysical rates. Quasi-static silicon burning seems to be moderately understood, since the rate at which neutrons, protons, and alpha particles are made available to build nuclei heavier than silicon appears to be controlled by the photodisintegration of ${}^{24}\mathrm{Mg}$. This rate has been determined recently in the laboratory.

Calculation of energy production and nucleosynthesis in explosive carbon, oxygen, and silicon burning requires the rates for all proton, neutron, and alpha-particle capture reactions on nuclei up to $A \approx 60$, because nuclear-reaction rates and dynamical rates are comparable in many cases. Only a modest beginning has been made in determining these rates, and the next decade should see a major effort in this direction. For nuclear astrophysics this will be a period of great excitement and significance. The same can be said for nuclear physics, since the results may lead to an understanding of the interactions of photons, nucleons, and alpha particles with nuclei, in the intermediate mass range, where there are few empirical data and only rudimentary theories. The nuclei with $A \gtrsim 60$ appear to have been produced by neutron capture, on a slow time scale (the *s*-process) or on a rapid time scale (the *r*-process). A basic need is to delineate the sites and nuclear reactions responsible for the *s*-process neutrons. The rates of some possible neutron-producing reactions have been measured, but a better theoretical understanding of stellar evolution will be necessary before this problem will be resolved. The *r*-process neutrons must be produced in explosive situations, but we have at present only rough indications of the important nuclear reactions. Neutron capture cross sections have been measured for a great number of the stable nuclei, which lie along the path of element building by the *s*-process. Capture cross sections are still missing for some specially interesting nuclei such as the osmium isotopes, which would be useful in obtaining time-scale information for nucleosynthesis. For the *r*-process, the path followed in building heavy elements lies far on the neutron-rich side of the neutron–proton ratio corresponding to nuclear stability. The *r*-process is capable of building the heaviest known nuclei because the radioactivity of the nuclei with $A > 209$ is no barrier to rapid successive neutron captures. The crucial parameter needed in determining the path of the *r*-process is the nuclear binding energy for exotic nuclei far from beta-stability. This has been estimated from the binding energy of stable nuclei, but the estimates are particularly uncertain for nuclei in the recently postulated island of stability near $Z = 114$ and $N = 184$. Only a systematic study of neutron-rich nuclei in the various regions of the periodic table, and especially in the region of the superheavy elements, can lead to improved estimates.

The recent measurement of the xenon isotope ratios produced by the fission of ^{244}Pu confirms that ^{244}Pu is the progenitor of the neutron-rich xenon found in certain meteorites. Thus the short-lived isotope ^{244}Pu becomes another reliable "clock" for timing nucleosynthesis just prior to the formation of the solar system. The search for other extinct radioactive elements should be continued with renewed emphasis.

The occurrence in nature of certain light nuclei such as deuterium, helium-3, lithium, beryllium, and boron, which are easily destroyed in thermonuclear reactions, presents a problem—how and where were these nuclear species produced? Spallation of more abundant, heavier nuclei such as oxygen, nitrogen, and carbon is a possible answer to "how," and the answer to "where" may be the surfaces of magnetically active stars or in interstellar space by galactic cosmic radiation. Before a complete explanation can be provided for these highly reactive nuclei, spallation reaction rates must be measured from threshold (a few tens of MeV) to at least 200 MeV for all the likely target nuclei.

The complicated molecules found recently in interstellar space, and especially the isotope ratios of the elements that form them, provide us with a summary of the total nucleosynthesis up to the present. This evolved matter is the source material for a new generation of stars. The composition of this material provides new checks for our theories of nucleosynthesis.

It may not be too much to hope that a more complete understanding of the interplay between nuclear astrophysics and gravitation in very dense objects will assist in elucidating the correct theory of general relativity.

In selecting the specific aspects of laboratory nuclear astrophysics, above, attention has been focused on certain broad areas where information is desperately needed before reliable progress can be made in our understanding of the nuclear aspects of stellar and universal evolution. It hardly needs to be re-emphasized that all facets of nuclear physics have an impact on astrophysics—the way in which nuclear mass depends on N and Z, the kinds of symmetry indicated by nuclear reaction systematics, the occurrence of self-interactions in the weak interaction, to list but a few examples. It should also be emphasized that the astrophysical rate of each of the cycles of nuclear reactions depends on the relative abundances of the nuclei involved and thus on an accurate knowledge of the rate of the preceding chain of reactions.

CHAPTER SEVEN

Dynamical Astronomy

WORKING GROUP MEMBERS

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I. INTRODUCTION

The resurgence of celestial mechanics in the past two decades has come about through the advent of the space age, the capabilities of electronic computers, and the technological development of radar to the point where it can measure planetary distances (which had not previously been directly observed) with an accuracy several orders of magnitude better than the observations of angles can attain. A great deal has been accomplished in solving problems that were formerly intractable. The solution to these problems only set the stage for a wider range of more penetrating astronomical problems that remain to be solved.

An exposition of these problems cannot be organized into any logical sequence nor even into the form of a branching tree. It more nearly resembles a network, where each problem is tied to several others at various points. Furthermore, the theoretical problems of dynamical astronomy are inevitably intertwined with the practical problems of astrometry.

This summary has been prepared from suggestions contributed by 30 astronomers, each a leading specialist in his own field. A hundred more details could have been included, but the tenor of all their responses was a recognition of the necessity for continued support of work of a fundamental nature absolutely necessary for progress on the problems at hand in dynamical astronomy and for the means to attack the new promising problems that are the challenge for a sound, scientific future. The training of new scientists must be accelerated, both because it is exacting and because the instrumentation requested here will outlive one generation of astronomers.

II. SOLAR SYSTEM

One of the oldest problems in astronomy has been the description of the true motion of the planets about the sun. This involves the determination of the fundamental astronomical constants, especially the astronomical unit or the average distance between the earth and the sun and also the masses of the individual planets. It is necessary to derive theoretical formulas that represent the position of each planet separately under the simultaneous gravitational attraction of all the others as it moves about the sun. These formulas should be valid over as long a period of time as possible, say, ten centuries; each will contain several hundred terms. In the past, this problem has been attacked piecemeal, one planet at a time. With the exception of Mars, all the presently available theories were computed before the turn of this century. More observations of these planets have been accumulated since then than were previously available. The attack on this problem now involves several considerations. One possible plan would be to compute the trajectories of all the planets simultaneously by numerical integration over the entire span of years covered by all the observations. This must be balanced against two other possibilities. One could choose to recompute the general perturbations with numerical coefficients. This is what was done in the nineteenth century, but it may now be done more accurately and in a much shorter time. (The famous mathematician G. W. Hill spent seven years of his life computing the complete orbits of Jupiter and Saturn, and it is now known that there are some small mistakes in his work. E. W. Brown spent the better part of his productive life computing the orbit of the moon.) Alternatively, the logical-choice capabilities of electronic computers enable them to be used to perform mathematical manipulations on algebraic and trigonometric formulas. It is therefore possible to compute the formulas for each of the planets with algebraic symbols that are functions of the masses and orbital elements of all the planets, in order to obtain literal general theories for each planet. General theories offer the possibility that they will prove to be valid over a much longer time span into the future.

In whatever manner the above work is accomplished, it leads to a truly monumental task of comparing all the computed results simultaneously with all the observations. Such a global solution is realistically possible with the aid of electronic computers. It is necessary to undertake this enormous project if the true uncertainties of all the unknown quantities are to be assessed after the solution is completed. It furthermore offers the prospect that minute effects may be legitimately established from the remaining residuals, such as the relativistic precessions of the perihelia, which otherwise might be lost below the "noise level." To complete this task in the best manner possible, it will also be necessary in the future to integrate the variational equations for all the unknowns, so that their partial differential coefficients will have the most accurate values possible.

Literal expansions also need to be applied in several other areas, namely, in the lunar theory, in artificial satellite theory, and in a careful analysis of the motion of the earth. The main problem of the lunar theory (the gravitational effect of the sun upon the moon as it revolves around the earth) has recently been developed to a high degree of accuracy by machine calculation, but there is still no adequate treatment of the perturbations of the major planets upon the moon nor of the effects of oblateness. There have been important advances in recent years in the development of theories for artificial satellites, but the application of literal expansions has not yet been carried far enough. This offers the prospect of a more thorough treatment of singular cases, such as resonance or the critical inclination. Observations of the numerous artificial satellites in orbits, which have various inclinations to the equator of the earth, will provide much more detailed information about the interior structure of the earth than was formerly possible by purely geodetic methods on the earth's surface.

The slow, daily spinning of the earth on its own axis causes it to act as a gyroscope. The sun and moon are not in the plane of the earth's equatorial oblateness, and this causes a slow wobble in the axis of the spinning earth, known as precession. The moon, though much smaller, is also closer, and it causes an additional wobble, known as nutation, which is complicated in part by the more rapid motion of the moon around the sky. A more precise theory of precession and nutation is now possible, using machine-computed literal expansions. At the same time, there is the problem of the tides and the rotation of the earth as a somewhat less than rigid body. What local changes do observing stations experience as a result of these effects, and also from earthwide meteorological conditions? These problems must be analyzed more extensively and then carefully compared with observations.

Observations from a network of a half a dozen photographic zenith tubes (PZT's) spread over the surface of the earth have been used to determine the uniformity of its rotation and the wandering of its poles. Now two new techniques have become available that offer the prospect of much more accurate observations. Radio-frequency very-long-baseline interferometry (VLBI) is being developed, which, in effect, gives the astronomer synchronized telescopes several thousands of miles apart, observing the same fixed radio sources in the sky from known locations on the earth. The rapid internal motions of some of the exploding radio sources complicate the geodetic results only slightly, and the accuracy will be very high.

Although the number of suitable sources is more limited than for optical observations, the potential improvement in accuracy by three orders of magnitude with the radio-interferometry technique is vast. The main limitation on accuracy of earth-based observations appears to be the unpredictable component of the index of refraction of the atmosphere.

Even without benefit of averaging over repeated measurements, this limit will be only at the centimeter level of equivalent fringe-phase uncertainty. The potential for the formation of an ultraprecise, although sparse, primary reference system with this technique appears very promising, and such an observing program should be supported. Observing programs with astrometric instruments will be required to determine the positions of the radio sources with respect to the fundamental reference system and to determine whether systematic differences exist between the optical and radio techniques.

Meridian transit observations and accurate recording (particularly by photoelectric techniques) of occultations of stars by the moon are continuing to provide information on the position and motion of the moon. The new capability of laser ranging to the moon, using a network of three or more retroreflectors, will provide additional important contributions. Such observations carried out systematically and densely over a sufficient length of time will reveal not only the dynamical characteristics of the earth but also the physical librations of the moon. Combined with observations of the artificial satellites that orbit the moon, these will provide information concerning the internal structure, mass distribution, rigid body motion, and gravitational field of the moon.

The oblateness of the sun is not well known, although it is an important factor in distinguishing among several different aspects of the theory of relativity. A start has been made in direct measurement of the oblateness of the solar surface. It is now technically feasible to place an artificial planet in a close orbit around the sun, to observe the effect of the sun's dynamical oblateness, thus removing the existing ambiguity. Such a space vehicle mission would also be engaged in making astrophysical observations in the close neighborhood of the sun.

The three-body problem is one of the classical unsolved problems of mathematics and is the simplest form of the more intricate n-body problem. Many famous mathematicians have studied this problem, which is nonintegrable in the ordinary sense. Recently, significant progress has been made with respect to the general problem of three bodies, directly applicable to the dynamics of triple-star systems. Much detailed information concerning the restricted problem of three bodies has been accumulated by means of numerical calculations, especially with respect to various new families of periodic orbits. One application concerns the evolution and stability of the orbits of the minor planets of the Trojan group. Recent observations indicate that perhaps with respect to the sun and Jupiter, 700 such objects exist in the neighborhood of the equilateral triangular points. A few years ago, only a dozen such objects were known. If this progress continues, it is reasonable to expect that for the first time in the history of celestial mechanics we shall have a rather complete understanding of an important problem for which no general solution exists.

The *n*-body problem is even more complex, but it has many special applications, from the minuteness of particle physics to the vastness of globular clusters. Large computers now offer a new avenue of attack in studying the evolution and the stability of systems of stars. This ties in closely with two other areas, which are in the nature of techniques for attacking such problems, namely, regularization and numerical analysis. Formerly, regularization was a theoretical device of the mathematicians to avoid singularities. In recent years, it has been applied as a computing device to improve the accuracy and the efficiency of numerical calculations, especially in problems of unusual difficulty, such as the temporary close approach of two of the bodies in a large system. This device can undoubtedly be exploited in an ever-expanding circle of astronomical applications. For example, a regularizing transformation may be found that will reduce the computing time for the orbits of rapidly moving objects, such as the moon or Mercury. In the past, the problems of dynamical astronomy have been a stimulus to progress in numerical analysis and statistics.

Current problems continue to offer challenges, and we benefit from improved methods of computation and analysis that are being developed. Much judicious experimentation is needed, for example, to ascertain the most expedient and efficient methods of programming that should be used in calculating the dynamical evolution of a simulated globular cluster.

The comets, as a group, have orbits that often extend to enormous distances beyond the farthest known planets, while the known minor planets are generally confined within the dimensions of Jupiter's orbit. There is no sharp dividing line between the unusual cases of each group, and this leads to many as yet unsolved problems concerning their origins, the dynamical evolution of each group, and the origin of the solar system itself. It has been known for nearly a century that some comets are influenced by other forces in addition to the gravitational attraction of the sun and planets, but just what these forces are has not yet been clearly established. The power of computers now provides more leeway for extensive numerical experimentations, and the power of larger telescopes and more sensitive detectors will provide more observations to solve these problems. The minor planets appear to occupy the location of a missing major planet in the solar system, and recent observations with the powerful 48-in. Schmidt telescope at Palomar Mountain have given a good statistical picture of the characteristics of this population of nearly 50,000 small objects. However, it is still not established whether these tiny objects, often less than a mile in diameter, are the fragments of a cosmic collision or whether we are seeing an intermediate stage of a long-term process of accretion. The progress of space technology offers the possibility (in some respects easier than landing on the moon) of retrieving samples of these remnants for first-hand study.

III. ASTROMETRY

The whole universe appears to the astronomer as if he were standing at the center of an enormous celestial sphere, where only the directions and angular distances from star to star can be measured. These directions and angular distances over any part of the whole sky must be expressed in a uniform and accurate system of coordinates, corresponding to geographical latitude and longitude on the earth. Such a system, called the fundamental coordinate system, is more difficult to attain than might appear at first sight. There is no way to implant permanent bench marks on the sky, as surveyors do on the earth. Each star is a freely moving member of our own galaxy, the Milky Way; this galaxy is slowly rotating like a giant pinwheel. Each star has its own relative speed and direction, so that, as seen from the earth, it is slowly changing its relative position in the sky. This angular change is known as proper motion.

The importance of highly accurate proper motions cannot be overemphasized. No angular motion of a star can be detected except by the comparison of accurate positions of the same star over a long interval of time. These positions are referred to other nearby stars, which may themselves be moving, although their motions will presumably be smaller if they are farther away. Ideally, the extragalactic nebulae are most distant and most suitable as bench marks, but they are fainter than the standard stars and diffuse in appearance. In the northern hemisphere, a program has been initiated to determine absolute proper motion based on external galaxies, but a number of years will elapse before this program will give final results.

Historically, the brighter stars have been observed for the longest time, and therefore the Fundamental System (FK 4) consists of about 1600 of the brighter stars. On the average, an area of the sky 150 times as large as is covered by the moon has only one such fundamental star. Especially in the southern hemisphere (where there have been fewer observatories in the past) the accuracy of this fundamental coordinate system is not up to the standards of modern observing accuracy. What is needed is an extension of FK 4 to two or three times as many stars, including fainter stars and with the goal that the errors of the absolute proper motions should be greatly reduced.

The stars of the FK 4 system, like the meter bar in Paris, are not used in the routine reductions of astronomical photographs. What is needed for large-field telescopes is a secondary system of about 200,000 stars. For the northern hemisphere, such a photographic catalogue of stars is just being completed, based on an observing program that began 40 years ago. For the southern hemisphere, the necessary work has not even been started. This unbalance is one of the most pressing problems of astrometry. Similarly, the program of referencing proper motions to the extragalactic nebulae has only been started. The earliest plates have been taken of the northern hemisphere; the program is just being started in the southern hemisphere, and its completion is another long-term pressing problem.

For the largest telescopes, even the secondary system of 200,000 stars is inadequate because the field of view is small and the brighter stars are overexposed for accurate measurement. At the end of the nineteenth century, an international, cooperative, astronomical project produced the *Astrographic Catalogue.* It has the position measurements of literally millions of stars, including essentially all stars down to 1000 times fainter than the naked-eye limit. It was designed for use with hand computing methods. At present, pilot studies are under way to see how it can be improved for modern use. The results of these pilot studies should lead to recommendations for the modernization of the *Astrographic Catalogue.*

The nature of our Milky Way, its rotation, and its evolution, can be known only if we know the space positions and the space motions of the stars that compose it. This is a requirement that, if taken literally, is beyond the physical capabilities of the astronomers. We must settle for representative samples of as large a number as is reasonably possible to obtain. The complete space motion of a star requires a knowledge of the proper motion that was discussed above, the motion of the star along the line of sight, called the radial velocity, which is obtained by spectroscopic observations, and the distance to the star determined from its parallax. Parallax determinations for the closer stars by triangulation require accurate measurements with very-long-focus telescopes. Each star requires about 20 plates on the average, spread over several years. Only about a thousand stars are close enough to the earth for their parallaxes to be measured in this way, but they are the backbone of distance scales in the universe. The accuracy of parallax determinations appears satisfactory when the internal errors of any one observatory are considered. Unfortunately, this is not the case when observations from different observatories are compared. The subtle origin of these differences remains to be explored, and every effort must be made to reduce them.

Similar to this type of observing is the observation of double stars. Stars that appear as close doubles in large telescopes usually have a period of approximately 100 years, so that they need to be observed every few years. Observations are especially important near periastron passage; this time is unknown until enough observations have accumulated to compute the orbit. An even more taxing observational problem is the discovery of binary companions too faint to be seen. The orbit of the visible star in such a system must be computed from its minute, periodic deviations from straight-line proper motion. This, in turn, requires a large collection of observations. Invisible companions may have masses like those of planets, rather than stars.

Double stars that are too close together to be separated in any telescope can often be observed spectroscopically by the shift in their spectral lines as they revolve around each other. Observations often made for other purposes reveal information about the physical characteristics of the individual stars. The orbit of the double star, Sirius, the brightest star in the sky, led to the discovery of the first white dwarf star: a very hot blue star that has a mean density of over 10^6 g/cm^3. Similarly, it is mainly by means of their parallaxes that the actual brightness of stars of various types can be established. Of two equally faint stars, if the one can be measured to be ten times as far away as the other, then it must have one hundred times the intrinsic brightness of the other. The subluminous red dwarf stars have been identified to us in this way. Their large proper motions suggested that they were close, and this has been verified by their parallaxes.

Aside from the considerations of other areas of astronomy, observations required for continued progress in astrometry dictate certain special instrumental requirements. For parallaxes, the 61-in. astrometric reflector, which has recently been put into operation by the U.S. Naval Observatory at Flagstaff, Arizona, provides a model that should be matched in the southern hemisphere. The modernization of other long-focus telescopes that are or have been in use for parallax work would greatly improve the efficiency of observing, as well as their productivity. In the past, radial velocity work has been done mostly by using general-purpose telescopes. Experience has shown that the systematic errors that develop between different instruments make it imperative to have the radial velocity programs in both hemispheres conducted on a systematic basis in order to attain observational results that are consistent and commensurate with present-day standards of accuracy. The eminent success of the Palomar 48-in. Schmidt telescope in so many areas of observing, and particularly in the discovery of faint proper motion stars, indicates that its counterpart should be constructed in the southern hemisphere. The operation of these instruments requires at least three automatic, precision, measuring machines, in order to keep abreast of the requirement of plate reductions. The improvement of the fundamental coordinate system requires the construction of at least three automatic transit circles (ATC), one in each hemisphere and one mounted near the equator, in order that each instrument may have an overlap area with its closest neighbor in latitude. Each ATC should also be accompanied by a PZT. Additionally, several Danjon astrolabes, larger than the experimental models that have been used in the past, should be built and operated because they provide fundamental observations that do not have the same systematic errors as the other types of instrument. Existing instruments will need to provide an assured schedule for the observation of faint comets and the unusual objects that appear sporadically. The prospects of observations from space vehicles or the moon must be closely followed. For example, at present, the limitations in accuracy of a secondary star catalogue of 200,000 stars arise from atmospheric refraction, the optics of a telescope (which bends under its own weight), and the creeping of photographic plate emulsions. If all these stars could be monitored in the vacuum of empty space with a weightless telescope, the overall inherent accuracy of the catalogue might be improved by an order of magnitude. The development of new technological capabilities offers unforeseen observational possibilities. When the first U.S. spacecraft flew to Venus, the radio signals that were accumulated over several months gave a more accurate determination of the mass of the moon relative to the earth than had been attained in the whole previous history of astronomy. The recent discovery of pulsars has placed an unprecedented requirement upon the accuracy of planetary orbits. The time scale of the pulsars is so short, typically 100 cycles per minute, that the errors in the earth's orbit due to planetary perturbations appear as fictitious changes in the timing of these fascinating objects.

The solutions of all the problems described above are not an end in themselves. They serve to provide an interpretation and understanding of the physical universe and its component parts. The theoretical and numerical calculations concerning the long-term behavior of dynamical systems reveal both the stability of the solar system and the evolution of our Milky Way. The rotation of our galaxy, its period, the total mass interior to the sun's orbit around the galactic center, and the angular velocity gradient are all described in terms of Oort's two constants, A and B. A is determined from radial velocity observations of the stars and of interstellar gas; B is found from proper motions from which the effects of precessions have been correctly removed. The characteristics of the stars in the galactic halo are of special interest, but what little is known about them depends on using their small proper motions statistically. Because of their great distances, their proper motions are smaller and relatively more severely affected by observational errors. Furthermore, because of their faintness, precautions must be taken to avoid any magnitude effect in relating their proper motions to the known motions of the brighter stars, such as the FK 4 standards.

The orbits of visual double stars provide a direct measure of the masses of stars, parallaxes provide the intrinsic brightnesses of the stars, and together they establish the mass–luminosity law. The cosmic ratio of hydrogen to helium as well as the theory of stellar interiors also depend heavily on these observations. Similarly, the zero point of the period–luminosity law, the distances of globular clusters, the luminosities of RR Lyrae stars, degenerate red stars, and the physical characteristics of all other types of extraordinary stars depend on accurate parallaxes. The astrometric binaries, with invisible companions, are our sole source of information concerning the least massive objects outside our solar system. They provide a clue to the number of other planetary systems that may exist and the likelihood of intelligent life elsewhere in the universe. All progress, not only in dynamical astronomy but also in the areas of astrophysics and cosmology, is dependent upon the fundamental observing programs described above.

IV. RECOMMENDATIONS

A. COMPUTER SUPPORT FOR DYNAMICAL ASTRONOMY

The basic overriding requirement is to provide large computer facilities and support for their use on the following problems:

1. General perturbation theories for all the major planets, except Mars (which is already done).
2. Planetary perturbations of the moon to be derived with an accuracy capable of interpreting the laser-ranging observations.
3. Development of a lunar theory with literal coefficients.
4. Redetermination of the theory of precession and nutation.
5. Determination of the elements and masses of the major planets from a simultaneous solution, using all available observations and coefficients derived from the numerical integration of the variational equations.

6. Development and application of methods to describe the motions in multibody systems.

Primarily, the support should be for computer time at the investigator's home institution; secondarily, there should be access to a very large computer elsewhere, when the magnitude of the computation requires it.

B. FACILITIES FOR GROUND-BASED OBSERVATIONAL WORK

Instrumental facilities, especially in the southern hemisphere, are essential to the balanced progress of observational programs. The following are recommended:

1. Construction and installation of three automatic transit circles, each accompanied by a photographic zenith tube and a large astrolabe (75 cm). For adequate overlap, one set is needed in each hemisphere and the third near the equator.
2. Construction of at least three automatic measuring engines, one with a capacity to accept plates up to 17 in. × 17 in.
3. Modernization and refurbishing of existing long-focus refracting telescopes devoted to intensive astrometric observing programs.
4. Astrometric observations must be carried out in the southern hemisphere. Pending availability of a reflecting telescope largely dedicated to this work, the next large reflector designated for the southern hemisphere should be designed with the special requirements of astrometry in mind.
5. Installation of a duplicate of the Palomar 48-in. Schmidt telescope in the southern hemisphere, to be devoted in part to proper-motion surveys of faint stars.
6. Provision for guaranteed observing schedules on reflecting telescopes (up to 225-cm aperture) in each hemisphere for systematic radial velocity observations.

These items are listed in only a slightly descending order of priority. All are needed if their respective observing programs are to move forward in a balanced manner.

C. SUPPORT OF GROUND-BASED OBSERVATIONAL PROGRAMS

The items listed here are in addition to those implied by Section IV.B. In general, the observational facilities are assumed to be available. Full and continuing support of these programs is recommended:

1. The fundamental meridian circle observing program of the sun, moon, and planets at the U.S. Naval Observatory.
2. Systematic and continuous radar observations of the major planets and laser ranging of the moon.
3. Systematic observations of radio sources with very-long-baseline interferometers.
4. Optical observations of radio sources in order to refer them to a fundamental system (FK 4 or later).
5. Observing programs that will permit the extension of FK 4 to approximately 5000 stars with improved proper motions, especially in the southern hemisphere.
6. Construction of a photographic star catalogue of the southern hemisphere, equivalent to the AGK 3 that exists in the northern hemisphere.
7. The observing programs to refer absolute proper motions to the extragalactic nebulae in the northern and southern hemispheres.
8. Provision for an assured schedule of observing time on existing large telescopes for the observation of faint comets and other interesting objects that appear sporadically.
9. The survey of proper motions of faint stars on the Palomar Schmidt plates by means of a new laser-beam scanner.

D. SPACE PROGRAMS

1. A spacecraft should be launched into close solar orbit in order to determine the dynamical oblateness of the sun.
2. The feasibility of improving positional measurements of a large number of stars relative to the fundamental reference stars, by observations outside the earth's atmosphere, should be investigated.

CHAPTER EIGHT

Astrophysics and Relativity

PANEL MEMBERS (Joint Panel with Physics Survey Committee)

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I. THE NATURE OF THE FIELD AND SCOPE OF THE REPORT

This Chapter considers those phenomena in the astronomical universe that require Einstein's theory of general relativity for their explanation. For example, a star composed of neutrons has such strong gravitational fields that the Newtonian theory of gravitation breaks down and the theory of general relativity is needed. The application of relativity to astronomical phenomena continues a tradition established earlier when other branches of physics, such as Newtonian mechanics, atomic and nuclear physics, and magnetohydrodynamics, were so applied.

Although the scientists engaged in astrophysics and relativity are principally physicists and astronomers, this subfield includes the whole of cosmology—the study of the origin and evolution of the universe—a subject of considerable interest to nonscientists as well. Not only does cosmology play a unifying role with respect to other evolutionary sciences, such as biology and geology, but it touches on fundamental questions of broader interest, such as the origin of matter and the nature of time, that are rooted in ancient religious and philosophical traditions. We have treated cosmology as the central theme of this Chapter, because by exemplifying the connections with other human endeavors, cosmology may be helpful to the nonscientist who wishes to assess the value of scientific effort in astrophysics and relativity.

After discussing the cultural impact of the field in Section II, we show in Section III how traditional relativistic cosmology has been revitalized by recent—and often unexpected—astronomical discoveries. Then in Section IV we sketch in some detail the main lines of advance at the present time, indicating those in which additional effort would be most rewarding. The relation to other branches of science is considered in Section V. Section VI discusses the testing of general relativity.

II. THE IMPACT OF COSMOLOGY ON CULTURE AND SCIENCE

Historically, only affluent societies have studied cosmology with sufficient intensity to produce definitive advances. Such study—like the rapid accumulation of capital—is one way that strong societies lay a foundation for future greatness.

How cosmology influences society is not well understood; it acts in concert with many other forces in subtle and complex ways. Herbert Butterfield, a historian who tried to evaluate the scientific revolution of the seventeenth and eighteenth centuries, the trademark of which was the cosmology developed by Newton, concluded that, "Since the rise of Christianity, there is no landmark in history that is worthy to be compared with this." The implication is that Newtonian cosmology influenced social forces even more powerfully than did the discovery of America. But it was only at the time of Newton that cosmology linked with the forces that revitalized world civilization. Other examples suggest that this confluence was no accident. There have been three great eras of progress in cosmology—that of classical Greece, that following Newton, and that following Einstein. One could hardly imagine much of our present civilization without the schools of thought that these eras symbolize.

The contribution of Greek cosmology was the idea of making theoretical models to explain what we see—the novel idea of sketching in the mind's eye pictures of things so grand that no human eye could ever actually see them. All men have looked at the heavens with wonder and curiosity, but most found a mythological explanation adequate and returned to mundane occupations. The Greeks were the first to develop scientific theories: The earth is a ball; the moon is illuminated by the sun; each planet moves around the earth in its own orbit. These descriptions were so clear and precise that their inadequacies were provocatively apparent even though they were more profoundly true than any previous explanation of the lights we see in the sky.

In the Newtonian picture of the world, which still concentrated on the solar system, Copernicus placed the sun at the center and Kepler showed how the planetary orbits could be described with mathematical precision. From this basis, Newton was able to show that Kepler's orbits could be derived from physical laws of universal validity, expressed in mathematical form. This achievement inspired the incubation of modern science; it showed the essential role of advanced mathematics and of observations and experiment based on advanced technology and craftsmanship. Furthermore (and especially by the time Newtonian planetary orbits had been confirmed in detail), it inspired and reinforced an unprecedented confidence and ambition to subject all aspects of the world to human understanding and control. According to Butterfield, "When we speak of Western civilization being carried to . . . Japan in recent generations, we do not mean the Christianising of Japan, we mean the science, the modes of thought, and all that apparatus of civilization that were beginning to change the face of the West in the latter half of the seventeenth century."

Cosmology following Einstein is currently producing a new picture of the universe that is as great an innovation as those of the Greeks or the Newtonians. The new picture is not yet complete, even in outline, but some parts are clear. Problems are studied on a scale that was previously inconceivable. The universe is no longer the solar system surrounded by a scattering of stars, for the stars are seen to be organized into our Milky Way galaxy, which is one among countless galaxies populating the universe. Furthermore, the galaxies are moving apart in a motion that cannot have been eternal, so cosmology must describe not only the present universe but also its history and evolution. The motion of the galaxies poses the problem of the origin of the universe in an entirely new way that could be directly related to the fundamental structure of matter and space. The problem of the motion of galaxies also forces physical science to take a historical and evolutionary approach to its subject.

Cosmology has a fascination for both laymen and scientists—we are all awed by the mystery of creation. The grandiose scope and fundamental nature of cosmology make it a favorite point of contact and communication between the scientists and the general public. Physicists and astronomers delight to see their own specialities applied to cosmological problems. In a broader sense, cosmology as an observational science now depends on the education of a broader public that is willing, through their elected representatives, to support the allocation of the large sums that are required to advance in this area. Not only is communication with the public a fascinating task for the scientist, it is vital for the progress of his science.

How is it possible to frame cosmology in scientific terms when the universe is unique? This question has particular force when we reflect that in astronomy one can only observe rather than perform controlled experiments as in physics. Usually the fact that various phenomena are observed in great multiplicity compensates in part for this deficiency. Having observed some spectral feature in one star, the astronomer checks many similar stars to see if the feature is typical; having suggested a hypothesis to explain the properties of this feature one can make predictions for stars of very different age, mass, or composition and then observe those stars. Cosmology, on the other hand, deals with the nature and evolution of the universe as a whole, and we know only one universe! It is not surprising that scientists should attempt to approach the mystery of creation by rational hypotheses, but it may be surprising (and gratifying) that such hypotheses can be tested on scientific and not merely aesthetic or philosophical grounds.

The most basic cosmological observations are that the universe is uniform when averaged over distances large compared to the dimensions of clusters of galaxies and that it is expanding. The uniformity is confirmed by the observed isotropy of the cosmic blackbody radiation. The expansion appears to be centered on the observer; however, it is of the type that would seem to all observers, wherever they are located, to be centered on them, so that all parts of the universe are expanding in the same manner. The expansion probably began about 10 billion years ago, and the most direct hypothesis assumes an origin of the universe in a short-lived, high-density, high-temperature phase—the so-called "big bang." In stark contrast to the big-bang model, the steady-state model assumes that continuous creation of new matter replaces the matter lost by expansion in such a way that the universe keeps a constant density throughout all time. Other models take intermediate positions, such as supposing an oscillating universe or a succession of "little bangs."

The scientific testing of various models depends on the fact that information arriving from distant parts of the universe is delayed by the finite speed of light, so that we see galaxies as they were at various times in the past. Thus, in the big-bang model, distant galaxies should appear to be young; while in the steady-state model, galaxies at all distances have the same distribution of ages. The demonstration that the age of a galaxy is correlated with its distance would be decisive evidence against the steady-state model.

Each cosmological model is designed to be compatible with the observed expansion of the universe. Thirty or 40 years ago few additional observations were available, and it was not possible to choose among a variety of models. New observational techniques and refinements of old ones during the last 25 years have converted cosmology into an observational science. That is to say, theoretical models are discarded and new ones invented by comparing their predictions with observations. This comparison, in turn, has stimulated more ambitious and practical-minded theoretical efforts (aided by electronic computers) and further innovations in observing techniques (see Section IV). Specific examples such as the microwave background radiation, which is better explained by the big-bang model than by the steady-state model, or optical data on bright galaxies, which seem to favor an oscillating universe, are found in Section III.

In addition to being a science in its own right, cosmology also impinges on physics in various ways. There are some areas, such as relativity, in which the two subjects overlap. The testing of general relativity, both of the original Einstein formulation and of more recent rival versions, is discussed in Section VI. In a few cases, astrophysics and cosmology have a direct impact on the physics of small-scale phenomena; more often, there is an indirect impact when fundamental physics is forced to deal in a concrete way with natural but unusual phenomena. This subject is discussed in Section IV.

III. PAST, PRESENT, AND FUTURE OF ASTROPHYSICS AND RELATIVITY

A. COSMOLOGICAL MODELS

To construct a theoretical model of the universe, we must know what mixture of matter and radiation it contains; we must know the field equations of gravitation; and we must specify the symmetries of the cosmos. Cosmologists are far from agreement on these matters, but the majority view is roughly as follows:

1. The energy content of the universe is now dominated by nonrelativistic matter having negligible pressure. Observations show that galaxies have random velocities of the order of only a few hundred kilometers per second, less than the speed of light by three orders of magnitude. The known populations of relativistic particles, the cosmic rays and photons, have energy densities much less than that of the matter of galaxies.
2. Einstein's equations of gravitation are correct. (The evidence for this view is discussed in Section VI.)
3. All spatial directions and positions are essentially equivalent. The best evidence for the first assumption (isotropy) comes from the observed properties of the cosmic microwave radiation, discussed below. If the universe is isotropic around us, then it is isotropic about every point (unless man is located at a special point—an assumption to be rejected), which implies homogeneity.

These three assumptions lead to a class of cosmological models, first examined in 1922 by Friedmann. In these models, the space–time geometry of the universe is curved, with a scale length that changes with time. As the scale length increases or decreases, galaxies move apart or toward each other, like dots painted on the surface of a balloon that is being inflated or deflated. Light from distant galaxies therefore undergoes a Doppler shift toward the red, if the universe is expanding, or toward the blue, if it is contracting, and these wavelength shifts increase with distance. The Friedmann models thus provide an explanation of the systematic red shifts of distant galaxies, first observed during the 1920's.

The Friedmann models are distinguished by just two numerical parameters, which can be taken as the present rate of expansion, known as the Hubble constant, H_0, and the present rate at which this expansion is slowing down, known as the deceleration parameter, q_0. In all these models, the universe began with an infinitely high density at a time in the past less than the Hubble time $1/H_0$, and it has been expanding ever since. If q_0 has a value greater than one half, then the scale of the universe is now expanding more slowly than the two thirds power of the time and will eventually stop expanding and contract to a state of infinite density. The deceleration parameter is proportional to the present mass density of the universe and is less than, equal to, or greater than one half, depending on whether the mass density is less than, equal to, or greater than a critical value determined by the Hubble constant. Also, if q_0 is less than or equal to one half, the universe is infinite, with negative or zero spatial curvature, although, if q_0 is greater than one half, the universe is spatially finite, with positive spatial curvature. Thus, as long as we adhere to the Friedmann models, the chief task of observational cosmology is to determine the critical parameters H_0 and q_0.

Before describing the progress made in this task by observational cosmology, we must indicate the various minority views that would modify the Friedmann models in one way or another. The least radical modifications are those that alter the material contents of the universe or the gravitational field equations but leave intact the assumptions of homogeneity and isotropy. Walker and Robertson showed in 1935 that all such models have essentially the same space–time structure as the Friedmann models, except that the cosmic-scale factor can have quite a different time dependence. Included in this class of models are the following:

1. Models with a Cosmological Constant When Einstein first turned his attention to cosmology in 1919, he assumed that the universe is static. However, no static solutions of his field equations could be found, so he modified them by adding a new term, which involves a new constant, the so-called cosmological constant. The original motivation for a cosmological constant was removed by the discovery of the expansion of the universe, but its existence remains a logical possibility. Models with a cosmological constant can have quite different histories from Friedmann models and need not begin with a state of infinite density.

2. The Steady-State Model Bondi, Gold, and Hoyle have gone beyond spatial isotropy and homogeneity and have assumed that the universe, although expanding, always looks the same (temporal homogeneity). To fill the widening gaps between galaxies, new matter must be continuously created, but the steady-state model is not specific as to how and where this occurs. In this model, the cosmic scale factor grows exponentially. The Hubble constant is really a constant of nature (unlike the factors of the Friedmann models, which decrease with time), and the deceleration parameter q_0 takes the value -1. The universe is spatially flat, but the space–time continuum is curved like the surface of a sphere of radius $1/H_0$ in a five-dimensional flat space.

3. Models with Massless Particles It is possible that the energy density of the universe is dominated not by galaxies that we see but by particles of zero rest mass, such as neutrinos or gravitons, which interact too weakly with matter to have been detected. Another possibility is that the field equations involve massless scalar fields, as in the theory of Brans and Dicke. The resulting models differ from the Friedmann models only in detail and, in particular, share the feature of an initial state of infinite density.

A more radical step is to give up the assumption that the universe is homogeneous and isotropic. Its space–time structure now no longer has the simple form derived by Robertson and Walter; everything is much more complicated. An example is the hierarchical model of Charlier. Stars are grouped into galaxies, galaxies form clusters, and there is even evidence that clusters of galaxies are grouped into superclusters. Charlier suggested that this hierarchy continues indefinitely. In such models there is not even any meaningful way of averaging out its properties to define isotropy or homogeneity. The weight of present opinion is that hierarchy terminates with clusters of clusters of galaxies and that the universe is isotropic and homogeneous on any larger scale.

With this wide range of possible cosmologies, why do astronomers usually analyze their data in terms of the Friedmann models? The reason is not that these models are surely right, but that we need a very restrictive theoretical framework to draw any quantitative conclusions about the geometry and history of the universe from the limited data now provided by observational cosmology. As the resources of astronomy improve, we may well find that the Friedmann models no longer fit the data. At that point, it will be appropriate to adopt a less restrictive framework, either by incorporating a cosmological constant into the field equations, by postulating continuous creation, by allowing the presence of a scalar field or a high proportion of massless particles, or by giving up the principles of isotropy or homogeneity.

The Friedmann models present a challenge to observational astronomy: to determine the Hubble constant, H_0, and the deceleration parameter, q_0. Once these two parameters are known, the Friedmann models will tell us how old the universe is, whether it will continue to expand forever or begin to contract again, and whether it is spatially infinite or finite. However, the effort to determine H_0 and q_0 could reveal that the Friedmann models are wrong; then we would be able to develop a more accurate cosmology.

The problem of determining H_0 and q_0 has called into play virtually every weapon in the armamentarium of astronomy. In rough order of importance, these include the following:

1. Red Shifts and Luminosities In a uniformly expanding universe, the velocity away from us of a moderately distant galaxy is its distance times the Hubble constant, H_0. H_0 is often expressed in years^{-1}. The velocity of a galaxy can be accurately measured by observing the red shift of its spectral lines, and the distance can be determined by observing the apparent luminosity of the galaxy, which decreases with distance according to the inverse-square law. We also need to know the absolute luminosity of the galaxy, or the energy that it radiates. This is accomplished by an elaborate chain of distance determinations, which is continually being checked and refined. In a recent determination of H_0, the chain consisted of five links. The value of H_0 is subject to change as the data are refined.

First, the distance to a nearby cluster of stars, the Hyades, is determined from motions of its stars to be 130 light-years. Many stars of the Hyades cluster belong to the main sequence, the class of stars that have not yet exhausted the hydrogen fuel at their centers. The absolute luminosity of such stars is known to be uniquely correlated with their surface temperature (spectral type). Knowing the distance to the Hyades, we can deduce for each spectral type the absolute luminosities of its main-sequence stars from their apparent luminosities.

Second, the distances to other stellar clusters and associations within our galaxy are determined by comparing the apparent luminosities of their main-sequence stars with the absolute luminosities known from the Hyades for the same spectral type. Six of these clusters and associations contain a total of nine giant variable stars, called Cepheid variables. The absolute luminosity of such stars is uniquely correlated with their period of variation and spectral type. Knowing the distance to the nine Cepheids in open clusters and associations, we can deduce their absolute luminosities from their apparent luminosities.

Third, the distances to the nearby galaxies within our local group are determined by comparing the apparent luminosities of their Cepheids with the absolute luminosity for the same period and spectral type. It is found that the distance to M31, the great spiral galaxy in the constellation Andromeda, is two million light-years. Knowing the distance to M31, we can determine the absolute luminosity of its globular clusters–great agglomerates that contain hundreds of thousands of individual stars.

Fourth, the distance to the nearest rich cluster of galaxies, in the constellation Virgo, is determined by comparing the apparent luminosity of the brightest globular cluster in the Virgo galaxy M87 with absolute luminosity of the brightest globular cluster in the Andromeda galaxy M31. If one assumes that these brightest globular clusters have the same absolute luminosities, the distance to M87, and hence to the Virgo cluster, is 50 million light-years. Knowing the distance to the Virgo cluster, we can determine the absolute luminosities of its constituent galaxies from their apparent luminosities.

Fifth, the study of a number of rich clusters of galaxies led Hubble to conclude that all their brightest members have about the same absolute magnitude. If one assumes that the brightest galaxy of any cluster has the same absolute luminosity as the brightest galaxy M87 of the Virgo cluster, then measurement of the apparent luminosity of the brightest galaxy in a cluster provides a measure of the distance to the cluster.

Using this particular chain of distance determinations, $1/H_0$ was found to be between 10 billion and 16 billion years. However, any change at any link in the chain of cosmic distance determinations would require a corresponding change in the Hubble constant, so the true range of uncertainty in H_0 is probably even wider; recent work suggests that $1/H_0$ is somewhat larger.

To determine q_0, it is necessary to push the measurement of red shifts and luminosities to such great distances that the graph of galaxy velocity versus distance is no longer a straight line. The curvature of this graph, together with the Friedmann models, then determines q_0. The most distant galaxy known is the radio source 3C 295, for which the fractional red shift of wavelengths is 46 percent. The available data for other galaxies out to 3C 295 indicate that q_0 is between one half and three halves. However, the measurement of q_0 has its own difficulties, including sensitivity to evolutionary and selection effects. The steady-state model behaves like a Friedmann model with $q_0 = -1$ and does not allow evolution, so it is in apparent conflict with the measurements of red shifts and luminosities.

2. Age Measurements According to the Friedmann models, the age of the universe should be less than the Hubble time, $1/H_0$. On the other hand, the age of the galaxy can be estimated from the known present relative abundances of the isotopes of uranium. If one assumes that uranium was created during a short period (lasting not more than a few hundred million years), with ^{235}U initially about 65 percent more abundant than ^{238}U (as indicated by considerations of nucleosynthesis), then for the ^{235}U to have decayed to its present low abundance, the uranium must have been formed about 7 billion years ago. If the uranium was created gradually, our galaxy must be older than that, and estimates based on the abundance of other radioactive species give ages ranging up to 20 billion years. The comparison of the observed distribution of globular cluster stars in luminosity and spectral type with the results of theoretical calculations of stellar evolution gives an age between 9 billion and 15 billion years. The upper range of these age determinations is larger than the lower range of determinations of the Hubble time, but no clear discrepancy has yet emerged.

3. Mass Density Measurements If the Hubble constant is (13 billion years)$^{-1}$ and the deceleration parameter is greater than one half, then the mass density of the universe in a Friedmann model must be greater than 9×10^{-30} g/cm^3. The masses of a few nearby galaxies are known from studies of their rotation velocities or the dynamics of pairs of neighboring galaxies. From the mass-to-luminosity ratios of these galaxies, and the observed density of luminosity throughout the universe, the mass density of the universe can be estimated to be about 3×10^{-31} g/cm^3. The study of clusters of galaxies gives a slightly larger result, but it still appears that the mass density of luminous matter is too small by an order of magnitude to be consistent with a q_0 as large as one half. It is possible that nonluminous intergalactic matter makes up the missing mass. If such matter were cool, it would produce absorption effects in the light from distant quasars, and such effects have not been seen. If it were hot, the absorption effects would not be expected, but then one might see emission effects, for example at x-ray wavelengths. There is inconclusive evidence that such effects exist. Therefore, the estimates of mass density that can be supported by direct observation suggest that q_0 is considerably less than the critical value of one half. This would imply an open, indefinitely expanding universe.

4. Angular Diameters, Number Counts The measurement of angular diameters of galaxies as a function of their red shifts could in principle serve to determine H_0 and q_0, but galaxies vary too widely in actual diameter to make this method practicable. The measurement of the numbers of galaxies up to a given red shift or down to a given apparent luminosity (or radio source strength) might also be used to determine q_0, but the radio source counts do not agree with any Friedmann model, unless we allow for evolutionary effects. Since the reason for such effects is unknown, this method is useless at present as a measure of q_0. Again, the steady-state model, which does not permit evolution, appears to be in conflict with the source counts. The influence of selection effects could be greater than we have assumed, in which case this method is useless.

In summary, the confrontation of the Friedmann models with different pieces of astronomical evidence leads to different pictures of the universe, of which the following are examples:

1. The universe is finite and the deceleration parameter is of order unity, as indicated by the measurements of red shifts and luminosities. Therefore, the bulk of the matter of the universe must be in some form that has not yet been discovered, possibly intergalactic ionized hydrogen with a temperature of order 10^6 K or greater. The universe is relatively young, with an age close to the lower limits allowed by the dating of radioactive elements and globular clusters.
2. The universe is infinite, with a very small deceleration parameter, as indicated by the observed density of luminous matter. The age of the universe is close to the Hubble time, in rough agreement with other estimates.

It is also entirely possible that neither of these alternatives represents the true Friedmann model, or even that the Friedmann models are altogether wrong.

B. NUCLEOSYNTHESIS IN THE BIG BANG AND STELLAR EXPLOSIONS

A challenging problem is to account for the observed abundances of the elements, which seem to be very similar in the solar system, in distant stars of the galaxy, and even in other galaxies. Gamow and his collaborators developed the original big-bang model because they thought that the elements could be formed under the intense heat and high density of the early phases occurring in Friedmann models. Although light elements can form in this way, all theoretical attempts to produce nuclei heavier than lithium have failed. In a steady-state model, in which there is no hot, dense phase, one is forced to consider nucleosynthesis in stars. Thus, for the heavy elements, both types of model require stellar nucleosynthesis.

For the light nuclei (hydrogen, deuterium, helium, and lithium), synthesis in the big bang is a possible explanation under certain simple assumptions. If Einstein's general relativity is the correct theory, hydrogen and helium would be produced in a ratio of about ten to one by number, a value approximately equal to that actually observed in most objects, and deuterium and ^{3}He would be roughly 10^{-4} and 10^{-5} of hydrogen, respectively. However, if the scalar–tensor gravity theory is correct, it is more likely that little helium would be produced. Astronomy still has not settled this most important question: Did the helium exist when galaxies began to form or has it been synthesized by nuclear reactions in stars within the galaxies? The steady-state model demands the latter, ordinary big-bang models demand the former, and scalar–tensor big-bang models could yield either. The most natural place to look for an answer, the composition of the oldest stars, offers some indications either way; thus the matter is not yet settled.

Evidence regarding the big bang could also come from detection of deuterium and ^{3}He by radio astronomy. Observation of the hyperfine transition in $3He^+$ in H II regions would allow its abundance to be determined. Similar searches for the hyperfine transitions of interstellar deuterium have been unsuccessful so far, but the corresponding upper limit is not decisive.

A series of papers by Cameron, Fowler, Hoyle, and the Burbidges developed the idea that nucleosynthesis of elements heavier than helium occurs in the interiors of stars. By surveying the systematic properties of nuclei, these papers proposed a set of thermal environments of various initial compositions that would produce, by thermonuclear reactions, the prominent features of the natural abundance distribution. Recently, it has become clear that the environments yielding the best reproduction of the abundances are not found in the slow evolution of stars but, rather, in dramatic last-second explosions–supernovae and "little bangs," which extensively alter the composition of stellar matter. It now seems possible that all the elements and their isotopes with atomic number greater than or equal to $Z = 6$ (carbon) have been synthesized during the explosions of massive stars (roughly 20 to 40 times the mass of the sun). The evidence that the elements were synthesized at the moment of explosion comes from comparing the nuclear abundances theoretically produced in the two cases with the actual nuclear abundances observed in nature. The key feature is that in an explosion nuclear fuels burn at temperatures considerably higher than those at which the same fuels burn in a static star. The final abundances are different because of the higher ignition temperature in the explosive case. Inasmuch as the big-bang model appears to be incapable of providing for the synthesis of elements heavier than lithium, it is gratifying to find that, at least theoretically, stellar explosions can produce them.

It has long been realized that the atomic nuclei cannot always have existed. The fact that radioactive ^{235}U, with a half-life of 0.7 billion years, still constitutes nearly one percent of the much longer lived ^{238}U assures us that much of the uranium on earth was produced in a few-billion-year period immediately preceding the formation of the solar system; if the uranium had been produced much earlier, the ^{235}U would have decayed by now. It is likely that the uranium was produced in a nearby supernova explosion. Presumably nucleosynthesis is still occurring, and, if it is, we can expect to detect the fresh radioactivity by techniques of gamma-ray astronomy. The best gamma rays to search for are those emitted when ^{56}Ni decays to ^{56}Fe after its ejection from exploding stars. The evidence is now quite good that abundant ^{56}Fe was synthesized in that manner, and the observed average density of iron in the universe suggests that the associated density of nuclear gamma rays also should be observable. We are faced with the exciting possibility that gamma-ray astronomy at photon energies between 0.5 and 4 MeV may provide hard experimental facts concerning the history of nucleosynthesis in the universe. A single observation of the ^{56}Ni gamma-ray spectrum will throw light on these subjects: (a) the occurrence of explosive nucleosynthesis in the universe, (b) the dependence of its rate on cosmic time, and (c) the validity of the steady-state universe.

The most direct way that the study of stellar evolution and nucleosynthesis provide information regarding the correctness of cosmological models is in the timing of the creation of the elements. In Friedmann models the age of the universe is less than the reciprocal of Hubble's constant, or 10 billion to 16 billion years. If we live in a Friedmann big bang, that age should considerably exceed the age of the globular clusters, about 9 billion to 15 billion years as estimated by customary calculations of stellar evolution. Because the globular clusters are metal poor, their age must considerably exceed the mean age of the elements as revealed by the radioactive probes. The most farseeing of these probes, ^{187}Re, seems at present to be also about 15 billion years old, although different ages have been advanced by different scientists. The uncertainty in each of these three ages is still sufficiently great that there is not necessarily a contradiction, but the suggestion of one has been lurking for years, and we early await several proposed measurements that will help to clarify these ages.

C. EXPLODING GALAXIES

Until about 25 years ago, practically all of our information about the universe was obtained from optical studies. With increasingly sophisticated techniques, optical astronomers were investigating the properties of our own galaxy and the expanding universe outside. Basic understanding of the energy sources and structures of the stars and the dynamics of the galaxy were obtained. Although 20 years before, the universe had been shown to be expanding, few data relevant to cosmology were available. The investigation of the electromagnetic spectrum in the postwar period—first by the techniques of radio astronomy and more recently in x-ray and gamma-ray wavelengths and in the infrared—and the increasing realization that classical cosmic-ray physics involving charged particles is closely interwoven with these parts of the electromagnetic spectrum, led to a new era of discovery. Major discoveries, largely unpredicted, have occurred throughout the last two decades.

Radio astronomers first showed that there are large numbers of sources in the universe that are radiating vast energies in the radio spectrum between about 10^7 and 10^{11} Hz. The form of the spectra, the polarization, and other evidence led quite early to the conclusion that these sources are emitting by the electron synchrotron process (fast electrons in magnetic fields). The theory of synchrotron radiation implies that they must be generating very large energies (up to 10^{61} ergs, equivalent to 10^7 solar masses in some cases) of relativistic particles. Some sources are in our own galaxy; others are extragalactic objects. The sources in our galaxy are remnants of exploding stars—supernovae—the most famous one being the Crab nebula. The early optical studies of this object showed that it was an old supernova remnant, and the discovery that it was a gigantic particle accelerator led to a revival of the idea that supernovae are the primary sources of cosmic rays. The extragalactic sources were even more remarkable. The vast energies present must be released in gigantic explosions, usually located in the nucleus of the object. Theoreticians are still trying to account for this vast energy release. The amounts of energy are so large that it appears that only the release of gravitational energy through collapse of a large mass in a very small volume, or the creation of matter, is able to account for these phenomena. The gravitational fields present are likely to be strong, requiring general relativity for their interpretation.

More recent discoveries have added to this picture. It has been shown that the nucleus of our own galaxy is a powerful source of nonthermal radio emission and also of infrared emission. The major source of activity is confined to a region with a size of only one or two light-years (compared with the size of the galaxy, which is 100,000 light-years). Similar situations have been found in galaxies of practically all types. Recent investigations of infrared emission in some galaxies show that it is much greater than all the radiation emitted by the stars and is generated in very small nuclear regions. Radio astronomers, using interferometers with very long baselines (approaching the diameter of the earth), have measured components with these very small sizes (as small as 0.001 sec of arc) directly in distant galaxies. Moreover, there is now ample evidence that the nonthermal emission in optical, infrared, and radio wavelengths, which is generated in the nuclei of galaxies, often shows variations in times of the order of years, months, and possibly days, indirectly confirming that the energy is generated within volumes less than a light-year across. The gravitational radiation detected by Weber, discussed in Section V, may be a manifestation of even greater activity in the nucleus of our galaxy, provided that the claimed detection is confirmed.

Thus, studies on radio, optical, and infrared wavelengths have led us to realize that stars and starlight are not the only energetic constituents of the visible universe. Violent activity in the nuclei of galaxies is probably commonplace and is not due to thermonuclear processes.

So far we have discussed only the previously unknown characteristics of galaxies revealed by investigations outside the optical wavelength band. Even more significant are the entirely new kinds of objects that have been found. The most important of these are the quasi-stellar objects (quasars) and the pulsars (see Section III.H). Prior to 1960, it was found that the optical counterparts of extragalactic radio sources were galaxies, objects having discernable angular diameters (unlike stars). In 1960, some radio sources were identified with pointlike objects indistinguishable from stars on direct photographs. At first it was thought that the sources might be like true stars, hence the name quasar (which is short for *quasi*-stell*ar*). A major breakthrough came in 1963 when it was found that the spectrum of the brightest quasar, 3C 273, has a large red shift ($\Delta\lambda/\lambda = 0.16$). In the ten years that have elapsed, extensive work on quasars has been conducted, but their nature still remains a mystery. Their red shifts extend from about 0.1 to about 2.9 (distances from 10^9 to 10^{10} light-years, if Hubble's law applies). Their optical, radio, and infrared spectra are in many ways similar to those of the nuclei of galaxies in which violent activity is taking place, and the simplest apparent interpretation of them is that they are indeed excessively luminous galaxies either in an early stage of evolution or in a very late stage. But there is so far no direct evidence that they are actually galaxies. If they are at cosmological distances, there are many severe problems associated with the idea that they are galactic nuclei with scaled-up energies. Some scientists therefore have proposed that they are not at great distances, so that their red shifts are not of cosmological origin. Attempts to establish conclusively that most quasars are at cosmological distances have failed, as have attempts to argue that they are comparatively close by, that is, at distances of less than 10^8 light-years. This situation is currently at an impasse, the majority of scientists tacitly assuming that they are at the distances suggested by their red shifts, so that they may be used for cosmological investigations; while the minority feel that, without an independent distance determination and an understanding of the physics of the objects, it is premature to apply them to test cosmological models in ways that might be completely irrelevant.

In any case, quasars are of extreme importance for relativistic astrophysics. If they are at cosmological distances, they are the only discrete objects that we can now use directly to investigate the universe as it was billions of years ago. If they are closer to us, then their existence means that comparatively stable configurations must exist that can give rise to large intrinsic red shifts, possibly casting doubt on the reliability of the red shift of ordinary galaxies as a distance indicator. In either case, we are very far from understanding the way in which the particles are accelerated, although large masses inside small volumes, hence strong gravitational fields, must be involved.

Although quasars were discovered by the radio astronomers, it is now clear that radio emission is sometimes a comparatively minor form of activity in such objects. Optical objects with similar properties but less radio emission are a major constituent of the universe. It is probable that the time over which they are active is short compared with 10^{10} years. This assumption implies that the total number of objects that are their progenitors or descendents could be comparable to the total number of luminous galaxies in the universe.

D. RADIO GALAXIES AND COSMOLOGY

The discoveries of radio galaxies and quasars posed enormous theoretical problems because of the huge energies involved. Moreover, because they are so bright, they offer the possibility of extending cosmological studies to much larger red shifts, where the effects of curvature also are large.

Surveys of the sky at radio frequencies show numerous discrete sources. The brightest of these are concentrated near the galactic plane and are clearly in our Milky Way. The others are nearly uniformly distributed about the sky and are extragalactic. Modern radio telescopes have sufficient sensitivity and resolution to isolate about one million such sources. Of the few hundred sources with radio positions good enough to permit optical study, about one third are identified with galaxies and one third with quasars. For the remaining third, no optical identification has been possible, sometimes because of heavy obscuration by dust, because the optical luminosity is relatively low, or because the galaxy is very distant.

Since the early 1950's, scientists have realized that many of the discrete sources are sufficiently strong to be detected by radio means, although they are far beyond the limits of even the largest optical telescopes. As a striking example, the most distant known galaxy is identified with a radio source, 3C 295, which, as a radio source, is 10^4 times stronger than the weakest source that we can detect. Presumably, many of the faint radio sources recorded correspond to optical galaxies too faint to see. Although the potential applications to the cosmological problem are obvious, no definitive answers have been found so far. The main difficulty is the inability to determine from radio measurements alone the distance and red shift of a radio source. Clearly, the discovery of a "standard candle" for radio astronomy, analogous to the "first ranked cluster member" used in optical investigations, would be of very great value, as would a feature in the radio spectrum that could be used to find the red shift. But so far it has been necessary to depend on an optical magnitude and red shift. Since detailed studies are necessarily limited to those objects that can be studied optically, the potential power of the radio telescope in investigating the very distant parts of the universe has not been fully realized.

For this reason most of the radio-astronomy effort in cosmology has depended on statistical studies of relative numbers of radio sources of various intensities and angular sizes, based on the reasonable assumption that in general the most distant objects will appear fainter and smaller. Unfortunately, the spread in intrinsic luminosity and linear dimensions of the extragalactic sources is very large, and this has greatly limited the certainty of the results. The simplest interpretation of the data indicates a large excess of faint, presumably distant, radio sources, over what would be expected in a universe uniformly filled with radio sources. It is generally assumed that this excess indicates a very much greater density of strong radio sources at large distances. Because of the travel time of light, this great density at large distances corresponds to larger densities at early epochs. Such a theory is inconsistent with the simplest form of steady-state cosmology, which requires the universe to be unchanging with time. Some astronomers, however, challenge the cosmological significance of the source counts and choose to interpret the data as merely indicating a deficiency of radio sources in the local region, rather than an excess at large distances. Moreover, the experimental result is not without criticism. Even if one accepts an excess density at large distances, a detailed understanding of the situation is not possible without first knowing the distribution of intrinsic luminosities, which requires knowledge of the distance to a large number of sources.

Some attempt has been made to divide the radio counts into categories according to type of associated optical object, surface brightness, or radio spectrum to determine which, if any, particular class of source is responsible for the observed excess. So far the results have been inconclusive, primarily because of the small number of sources involved and the difficulty of interpreting the effect of observational selection introduced when one tries to classify or identify radio sources. A major need is for more systematic data.

There is today a growing realization that, before any definitive cosmological conclusions can be drawn from radio studies, it will be necessary to understand better the nature of the radio sources themselves. Even aside from their use as a tool for cosmology, the extragalactic radio sources stand as a major problem for theoretical astrophysics. The release of gravitational energy of very massive bodies has appeared to be one of the most promising energy sources proposed and has stimulated extensive theoretical research on massive bodies and gravitational collapse. Other suggestions, including matter–antimatter annihilation, quarks, or a "creation field" have equally profound implications for fundamental physics.

E. X-RAY AND GAMMA-RAY SOURCES

The potentially observable range of the electromagnetic spectrum extends over 25 decades of frequency from megahertz radio waves to high-energy gamma rays. More than half of these decades lie above the ultraviolet region of the spectrum and constitute the province of x- and gamma-ray astronomy. Though solar x rays were first observed in 1948, it was only during the 1960's that the region of extrasolar x and gamma rays was scouted in experiments with rockets, balloons, satellites, and ground-based instruments. Beginning with the discovery of the first x-ray source in 1962, a variety of galactic and extragalactic sources has been uncovered. Extreme physical conditions exist in these sources that were never before encountered in terrestrial physics or astrophysics. They constitute, in effect, distant laboratories where one can observe previously inaccessible regimes of plasma and nuclear physics. Moreover, the observations of extragalactic x and gamma rays provide new data about the conditions in intergalactic space that are of fundamental importance to cosmology.

The first x-ray source, Sco X-1, is now known to be a variable stellar object emitting over 99.9 percent of its radiation in the form of x rays whose spectral distribution is characteristic of a plasma with a temperature of about 50 million degrees. Its average x-ray luminosity is over 1000 times the total luminosity of the sun. It flickers irregularly and occasionally flares by factors of 3 or more within minutes. It has been suggested that Sco X-1 is a close binary system undergoing violent mass exchange, but no evidence of the expected orbital periodicity has been found yet. Imbedded in the Crab nebula is a rapid pulsar (see Section III.H), discovered by radio observations and subsequently observed in the optical and x-ray regions. Over 99 percent of its pulsing radiation is in the x-ray region at energies above 1 keV.

Dwarfing these galactic sources in total x-ray luminosity is the galaxy M87. This giant star system is distinguished by its conspicuous non-thermal radio emission and by a peculiar chain of beadlike features (the "jet") that emit polarized continuum radiation and extend about 4000 light-years from the nucleus. Rocket experiments have shown that M87 emits x rays at a power level that exceeds its radio power and is a considerable fraction of the optical luminosity of all its stars. A challenge for future observations is to determine whether the x rays emanate from the "beads" or are emitted by the compact radio source in the nucleus. Their polarization is also of interest. As in the case of the Crab pulsar, we may be witnessing in these beads the effects of rotating magnetized bodies, but on an enormously larger scale involving millions of solar masses. Other extragalactic objects, including the Seyfert galaxy NGC 4151, the radio galaxy Cygnus A, and the quasar 3C 273, have been identified as x-ray sources. It will be interesting to find out whether the x rays in these sources are produced by the relativistic particles that are known to be present from the radio emission observed.

In the earliest rocket observations of galactic x-ray sources, the presence of a diffuse component of energetic cosmic photons was inferred from the existence of an apparently uniform background intensity. Many subsequent observations from rockets and balloons have established that this background is grossly isotropic and therefore probably of extragalactic origin. Its spectrum shows several distinctive features that appear to have fundamental cosmological significance. Below 1 keV, where the measurements are complicated by the effects of absorption in our galaxy, there is evidence for the existence of a particularly high intensity of soft x rays, which has been interpreted as the free–free emission of a universal intergalactic gas, with a kinetic temperature of the order of one million degrees. An abrupt change in the slope of the spectrum occurs near 40 keV. One theory attributes this finding to a corresponding change in the spectrum of electrons in intergalactic space, which are thought to produce the observed x rays by inverse Compton collisions with starlight and the cosmic microwave background (see Section III.G).

Recent observations suggest that the x-ray background is not perfectly isotropic but is associated with clusters of galaxies. If this interpretation is correct, an important new tool for exploring distant cosmic matter will have been gained.

Measurements in progress may determine if the low-energy gamma-ray spectrum near 1 MeV shows any evidence of the decay of ^{56}Ni, produced in supernova explosions during an earlier era of rapid star formation in the evolution of the universe. A resolution of the basic question of whether the observed background is truly diffuse or is the combined effect of many distant discrete x-ray emitting galaxies awaits the development of instruments with improved angular resolution and sensitivity. In any case, the properties of the extragalactic high-energy photon flux are a basic part of the observational material with which cosmology must now deal.

High-energy cosmic gamma rays were observed for the first time in a satellite experiment launched in 1967. Energetic secondary gamma rays must be produced when cosmic-ray protons collide with interstellar matter; therefore, they can serve as a probe of the distribution of high-energy interactions in the galaxy and universe. An instrument aboard the OSO-3 satellite detected gamma rays with energies of about 100 MeV arriving, as expected, from a band of directions coincident with the galactic disk in which cosmic rays and interstellar matter are concentrated. A strong peak of gamma-ray intensity from the direction of the galactic center is, however, an unexpected feature of the observations that awaits a satisfactory explanation. In addition to the galactic gamma rays, the observations give evidence for an isotropic intensity of gamma rays, which appears to be a part of the diffuse extragalactic photon flux.

Attempts have been made to detect cosmic gamma rays in the ultra-high-energy range above 10^{11} eV by observing from the ground the extensive air showers they produce in the atmosphere. So far only upper limits on the intensity of various potential discrete sources have been obtained. Nevertheless, since the spectrum of primary cosmic rays extends to at least 10^{19} eV, one can assume that the spectrum of secondary cosmic gamma rays extends to comparable energies, though the intensity is too low to be detected with present techniques. In this high-energy region, the spectrum of extragalactic gamma rays may show the effects of the opacity of intergalactic space, which is expected because of photon–photon collisions between the gamma rays and low-energy photons in starlight and in the cosmic microwave background. These opacity effects should appear near 10^{13} and 10^{16} eV for starlight and microwave background, respectively.

F. COSMIC RAYS

The study of cosmic rays within our galaxy is important to cosmology because it provides detailed information about the fast particles that are the sources of energetic photons. A comparison between the visible properties of our galaxy and other galaxies then permits inferences about the general characteristics of extragalactic radio and x-ray sources.

Cosmic rays with energies between 10^{10} and 10^{14} eV are studied by direct observations near the earth. Energies much below 10^{10} eV are strongly affected by the solar wind. Information in the low-energy range may be obtained by observing the secondary effects of such particles in interstellar space, such as heating of the interstellar gas and production of x rays by "knock-on" electrons. Above 10^{14} eV, one may study effects produced in the earth's atmosphere, such as the extensive showers of secondary particles produced when a high-energy cosmic ray collides with a nucleus high in the atmosphere.

The directional distribution of the charged cosmic rays is highly isotropic. At first this finding seems inconsistent with the assumption of a galactic origin for them. In contrast, the anisotropic distribution of electromagnetic radiation clearly reflects its predominant origin in the galactic disk. However, energetic charged particles accelerated in one way or another in the galactic disk can be trapped within the disk by the galactic magnetic field. Their trajectories are so twisted by magnetic force that any detectable trace of the anisotropic distribution of their sources is destroyed. Thus, up to at least 10^{16} eV, which appears to be about the energy limit for effective galactic trapping, it is plausible to look within our galaxy for the sources of cosmic rays. The discovery of pulsars has thrown new light on the origin of cosmic rays within our galaxy. The pulsars are apparently efficient transformers for converting gravitational energy into high-energy cosmic rays. The accelerated particles are retained within the galaxy by the galactic magnetic field for times of the order of 10^7 years before they collide with interstellar matter or leak into intergalactic space. The rate at which energy is apparently being converted by pulsars in this way could be sufficient to supply all the cosmic rays in the galaxy.

In view of the probable origin of most, if not all, of the galactic cosmic rays in pulsars, we can look on the charge composition of the primary nuclei as essentially a chemical analysis of the material in the immediate vicinity of the pulsar, if not the pulsar itself. The composition is, of course, modified by the passage of the nuclei through the interstellar matter. In recent years, the techniques for measuring this composition have advanced rapidly, so that detailed analysis of the abundance ratios of nuclei differing in Z by only one, and even isotopic composition studies among the low-Z elements, has become feasible. This is particularly exciting because certain of the observable isotopes are radioactive. By studying the isotopes whose half-lives against decay are in the right range, it is hoped that one can determine the length of time the cosmic rays are trapped in the galaxy. When used together with the known number of cosmic rays trapped in the galaxy, this lifetime yields a production rate required to account for their presence—and this is an important constraint on models of cosmic-ray sources. Ultrahigh-Z nuclei have been observed, and strong evidence for transuranic nuclei has been reported. The study of fossil tracks of cosmic-ray nuclei in crystalline lunar rocks has provided information on the intensities of galactic cosmic-ray nuclei for several billion years.

The energy spectrum of primary electrons—both positive and negative—has been studied intensively. Electrons in the energy range from 1 to 10 GeV, which are responsible for much of the galactic nonthermal radio emission, are now known to be primarily negative. Since the secondary electrons from cosmic-ray interactions with interstellar matter would contain roughly equal numbers of positrons and negatrons, one can conclude that most of the 1- to 10-GeV electrons are directly accelerated. This conclusion is evidently consistent with a pulsar origin. On the other hand, the proportion of positrons increases toward lower energies, indicating that they are of secondary origin with an increasing contribution from collisions of protons and nuclei.

Primary nuclei with energies greater than 10^{19} eV have been detected by observing the extensive showers of particles that they produce when they strike the atmosphere. Since the magnetic rigidity of such nuclei is so great that they cannot be contained by the magnetic field of our galaxy, they are almost certainly of extragalactic origin. However, photons of the cosmic microwave background interact with protons above a threshold energy near 10^{20} eV to produce pi mesons. Since the mean free path for such interactions is much less than the Hubble radius, the volume of space within which the observed extragalactic component of cosmic rays can originate must shrink at high energies. Therefore, one can anticipate a cutoff in the spectrum somewhere above 10^{20} eV. Experiments now in progress can check this prediction and thereby provide a significant new test of our ideas concerning the origins of high-energy cosmic rays and the universality of the blackbody radiation. Some evidence has been found to show that the primaries above 10^{19} eV differ from those below 10^{14} eV in being either nearly pure protons or nearly pure heavy nuclei instead of a mixture. Since the highest energy primary cosmic-ray nuclei are, in effect, intergalactic messengers, it would be extremely interesting to know, in addition, whether any of them are actually antinuclei produced perhaps in antigalaxies. Unfortunately, no feasible method exists at the present time by which one can distinguish between nuclei and antinuclei at ultrahigh energies.

G. COSMIC MICROWAVE BACKGROUND

The first theoretical treatment of a radiation-filled universe was given in 1931, and in 1948 big-bang cosmology was used in an attempt to account for the production of heavy elements by nuclear reactions in the hot material. Although this proved to be unsuccessful as a way of producing appreciable amounts of elements heavier than helium, Gamow was able to estimate a present temperature of 5 K for the blackbody radiation, a fossil remnant of the big bang.

Based on measurements with a 1.25-cm radiometer, an upper limit of 20 K and isotropy better than ±1 K were obtained for this radiation in 1945. More interesting is a determination of 2.3 K, which could have been obtained in 1941 from a measurement of the population of the first excited rotational state of cyanogen in interstellar space. The first positive indication of the radiation, a radiometer measurement giving an excess temperature of 3.5 ± 1 K at 7 cm, was made at Bell Laboratories in 1965. The interpretation of these observations as cosmic blackbody radiation was stimulated by a project then under way at Princeton to construct a radiometer at 3.2 cm to look for such radiation. Subsequently, a temperature of 3.0 K was obtained at 3.2 cm. In 1968, much better measurements gave a temperature of 2.7 K at wavelengths of 3.2, 1.6, and 0.86 cm. The shortest wavelength superheterodyne radiometer measurement is at 3.3 mm. By analyzing spectroscopically the excitation of cyanogen, a temperature of 2.8 ± 0.2 K has been obtained at a 2.6-mm wavelength. Particularly important for cosmology are the measurements of isotropy. These show that at wavelengths of the order of 1 cm the universe appears to be remarkably isotropic, and, furthermore, that the earth's velocity relative to this isotropic universe is at most 300 km/sec. Further studies of the isotropy of the background radiation may yield information on the magnitude of irregularities in the early universe.

An anomalously high temperature was reported at wavelengths below 1 mm using a bolometer in a rocket and tentatively confirmed by one balloon measurement. More recent balloon measurements give intensities closer to those expected on the basis of a 2.7 K blackbody spectrum. The significance of this anomaly, if real, is not yet clear. If the excess radiation at these short wavelengths proves to be correct, and is observed to be isotropic with a continuous spectrum, it will be necessary to assign a cosmic origin; but in the absence of these observations, the radiation could have its origin in the galaxy. If the origin should be cosmic, interesting questions about the theory of the fireball and the origin of the radiation at longer wavelengths would be raised.

A host of interesting theoretical developments has grown from the discovery of the cosmic microwave background. Some of these, such as the question of helium formation in the fireball of the early big-bang universe and the formation of galaxies through the growth of instabilities, have their roots in work by Gamow and collaborators in the late 1940's. The expected fractional yield of helium computed using general relativity, about 27 percent by mass, is remarkably insensitive to neutron half-life and present matter density, although it does depend on the net lepton number. On the other hand, the scalar-tensor theory can require very small helium formation dependent on the unknown strength of the scalar field.

The present state of our observational knowledge of the amount of helium formation that actually occurred in the fireball is not in good order. Generally, nuclear reactions in stars are expected to increase the helium content found in the interstellar medium and in young stars. Consequently, one must look at old stars to estimate the abundance of helium in primordial material. But most old population II stars are too cool to show helium lines. The evolved population II "blue horizontal branch" stars are hot enough and show very weak helium lines corresponding to a low surface abundance. The significance of this result is somewhat unclear because of the existence of some weak-lined young blue stars with weak helium lines. According to general relativity, high helium abundance seems to be required if the age of population II stars is to be less than the age of the universe, but under the scalar-tensor theory low helium is required to give a reasonable age after including the evolutionary correction caused by early rapid burning. Among other possible sources of information concerning primordial helium are the quasars, which apparently have weak helium lines, the low yield of solar neutrinos, which seems to require somewhat low solar helium; and on the other hand, the direction of evolution of the stars in the horizontal branch of the Hertzsprung-Russell diagram of globular clusters; the helium-dependent mass and luminosity of stars on the horizontal branch in comparison with the calculated luminosity and calculated mass-period relation of RR Lyrae variables; and the luminosity of population II stars with dynamically determined masses suggest nearly normal helium. None of these means of assessing the primordial helium abundance has given a completely convincing answer, and the problem continues to be a matter of great importance.

Much remains to be done. Observations at wavelengths below 2 mm are exceedingly difficult, but this part of the spectrum is very important. A great deal of energy could be contained in it, and the high-frequency tail of the spectrum is capable of providing important tests of the fireball hypothesis and the theory of the thermalization of the radiation. Many more balloon and rocket observations will be required to provide a clear picture of this spectral region. Another set of interesting questions involves the interstellar molecular lines. Are optical pumping or collision-induced transitions affecting the rotational populations, thus yielding anomalous temperatures?

H. PULSARS

Pulsating radio sources, or pulsars, were discovered by accident in a scintillation study of low-frequency radio sources. Over 60 of these extraordinary objects have been discovered. Characteristically they emit sharp pulses of radiation about once per second. The timing of these pulses is so precise that one speaks of millisecond accuracy over time intervals of a month or more. A particularly interesting pulsar is that embedded in the Crab nebula. It emits pulses simultaneously over the entire electromagnetic spectrum, from radio frequencies to x rays, at a rate of 30 times per second. It is generally thought that the Crab pulsar is the collapsed, superdense, stellar remnant of the same supernova explosion that was observed in A.D. 1054 at the present position of the Crab nebula. This remnant is probably a neutron star about 10 km in diameter, which is spinning at the pulse frequency and is gradually slowing down as its rotational energy is used in accelerating particles to cosmic-ray energies. Its electromagnetic radiation, probably synchrotron in origin, is emitted in a beam that sweeps across the earth like the beam of a distant lighthouse.

In the initial state of the collapse of the stellar core, gravitational potential energy is converted into rotational energy of the spinning neutron star. In the process, the star's magnetic field is compressed and amplified to 10^{12} G or more. Such a strong field, when rotating with the neutron star, generates enormous electromotive forces capable of accelerating electrons and ions to 10^{16} eV or more. The electrons emit the radiation we see, and the ions are a prime source of cosmic rays.

Pulsars are the latest in a series of unexpected and spectacular discoveries of modern astrophysics. The strong emission of radio bursts at highly regular intervals, of the order of a second, was unexpected because of the high intensity of this radiation, but the possibility of stars rotating with millisecond periods had been suggested on theoretical grounds. In fact, the possibility of neutron stars, with diameters of the order of 10 km and densities of the order of nuclear density, had been suggested by theoretical physicists as soon as neutrons had been discovered. Rotation periods of as little as a millisecond have been predicted, whereas all other known stellar objects (including white dwarfs) have rotation and vibration periods in excess of a second. The rotation of three white dwarfs has been found to be slow. A number of pulsars have now been observed with periods much shorter than a second, and for many the period has also been found to increase a measurable amount in a year or so. As bizarre as the idea of a whole star at nuclear densities may sound, the central energy source of a pulsar must be a highly condensed object, with strong gravitational fields. Rotational energy of neutron stars is considered the most likely—and the most conservative—hypothesis at the moment.

At least some pulsars—such as the one in the Crab nebula—are known to be remnants of supernova explosions, which is gratifying to the theorists, since they had speculated that these explosions are preceded and triggered by the dynamic collapse of an already dense highly evolved star. This collapse may lead to a highly condensed central remnant with strong gravitational fields (possibly relativistic); pulsars are presumably the outward manifestations of such remnants. The theoretical study of pulsars invokes almost every major branch of modern theoretical physics. The structure of a neutron star depends on relativistic equations of state for matter consisting mainly of neutrons and hyperons; quantum-mechanical phase transitions apply to the charged components—free protons versus aggregates of complex nuclei. General relativity is of some importance for the understanding of stable neutron stars and of overwhelming importance for fully collapsed objects. Even for neutron stars of low mass, the gravitational binding energy is much larger than the nuclear binding energy of even the most stable nuclei in the iron region. Even solid-state physics is relevant, in spite of temperatures far in excess of a million degrees, since the complex nuclei can form a "Coulomb lattice" at the extreme densities inside a neutron star. There are now such detailed and complex observations on changes in pulsar periods that theorists talk in earnest not only of solids but of starquakes, of superconducting currents, and even of volcanoes.

The rich structure of the interior of a rotating neutron star is matched by the complexities theorists invoke for their exteriors to explain the radio emission (not to mention strong gravitational radiation that is predicted for a young, distorted, and rapidly rotating pulsar). Rival theories abound for dealing with the finer details of the emission mechanism, but it is fairly clear that particle acceleration, relativistic plasmas, and coherent emission phenomena all play a role. From hyperons in their deep interior to cosmic rays in their magnetosphere, pulsars provide a testing ground for modern physics (or at least for the imagination of modern physicists). Pulsar theories also have stimulated new kinds of theoretical models for quasars. Rapidly rotating, massive, dense disks now seem attractive, possibly with a "vanished Schwarzschild singularity" in the center and, in any case, with strong gravitational fields.

I. PROSPECTS FOR FURTHER ADVANCE

Recent work embodies two somewhat contradictory tendencies. On the one hand, some of the predictions of relativistic models have been verified, such as the existence of what appears to be a blackbody background. On the other hand, some observations have cast doubt on these predictions. For example, the possible existence of noncosmological red shifts in quasars would confuse the straightforward interpretation of the Hubble expansion. Such a dichotomy is not unexpected in this field, because we are dealing with complex phenomena, which, although energetic in absolute terms, are usually very distant and therefore faint and hard to interpret. Often observations are undertaken to test one or another prediction of a model, and, although partial confirmation is frequently the result, just as often the increase in sensitivity required to make the test discloses new phenomena previously unsuspected. For example, in an attempt to extend radiogalaxy observations to large red shifts, radio astronomers discovered quasars. Strong radio galaxies tend to have nearly uniform optical luminosities and therefore have been useful as "standard candles" in extending the Hubble relation to large red shifts. Quasars are much more luminous, thus potentially useful to larger red shifts, but their luminosity varies from source to source. This variation makes them useless for extending the Hubble relation until the physics of the source is better understood.

Future work in the subfield will probably have a similar character. It will be stimulated by the desire to solve well-posed problems. Conceivably, the answers to these problems will fit a simple pattern, and a definite model for the evolution of the universe will triumph. More likely, no simple pattern will emerge. Unsuspected phenomena probably will be discovered, and revisions of the theoretical picture will be necessary. In either case, however, we will be coming closer to answering the question, "How did the universe originate?"

The United States is in a good position to make a major contribution to this subfield in the next few years. The nation has large optical telescopes, advanced radio telescopes, and space telescopes, as well as excellent theoretical groups working in both relativity and astrophysics. Other nations, notably Great Britain, the Soviet Union, Germany, The Netherlands, France, and Australia, are also in a good position to contribute, primarily as a result of efforts in radio astronomy and theoretical astrophysics, and several have indicated their intention to do so. Our country should continue its earlier efforts and share in the discoveries that will be made.

As we look to the future, we see three major resources that can be brought to bear:

1. The established groups of astronomers, with their present instruments, and the groups of theorists working in relativity and astrophysics;
2. A reservoir of physicists who, attracted by the opportunities of this field, will begin to apply to it their own specialties in other subfields of physics (as this reservoir is about ten times the number already in the field, a relatively small percentage shift can make substantial changes in the rate of progress);
3. Frontier instruments now under discussion that should be built (we refer here particularly to large ground-based and space telescopes that can extend the range at which we can detect galaxies and relativistic objects).

It is probable that each of these resources can and will be brought to bear. In Sections IV and VI we discuss their application and potential.

IV. RESEARCH METHODS IN ASTROPHYSICS AND RELATIVITY

A. OPTICAL METHODS

Large optical telescopes have played a key role in studies of the red shift–magnitude relation of the brightest members of clusters of galaxies. They are necessary to detect galaxies of large red shift, which are distant and therefore faint. The Mount Wilson 100-in. telescope yielded a red shift of 13 percent for the Boötes cluster as early as 1936. No further progress in extending the red shift–magnitude relation was made until the completion of the Palomar 200-in. telescope. Soon after the prime-focus spectrograph was finished, a red shift of 20 percent was determined for the Hydra cluster. Larger red shifts—up to 46 percent for 3C 295—have since been determined for radio galaxies with strong emisison lines in their spectra.

In studies of stellar evolution, large optical telescopes have made decisive contributions. Stellar evolution is best studied in star clusters whose members were formed at essentially the same time. Evolution will affect the brightest stars first, but to calibrate the observed effects, it is necessary to study the fainter stars that still show no evolutionary effects. These fainter stars allow a determination of the cluster's distance and an estimate of its chemical composition. In some crucial globular clusters in our galaxy, observation of stars of the required faintness with a 200-in. telescope requires a substantial part of a clear, moonless "good-seeing" night for each star. Photons arrive at a slow rate.

Spectroscopic studies yield the chemical composition of stars, which reflects the nucleosynthetic history of the interstellar gas from which they were formed. These studies require a large light-gathering power and a high spectroscopic dispersion, which can be obtained only in large stable spectrographs. The coudé spectrographs of the large telescopes have produced essentially all the information that we have concerning the chemical composition of stars.

The aperture of an optical telescope determines its light-gathering power and its angular resolving power. However, the effective resolving power of large ground-based telescopes is, at good locations, limited to about 1 sec of arc, on the average, by turbulence in the earth's atmosphere. Only high in the atmosphere or above it is a gain in resolving power obtained for larger apertures.

The main attribute of a large optical telescope is its light-gathering power. Specifically, an efficient photoelectric spectrometer at the 200-in. telescope allows detection of one photon per second; such a photon intensity corresponds to a star whose brightness is 18th magnitude (corresponding to a galaxy with a red shift of 0.1) over a spectral bandwidth of 5 to 10 Å. The reliable detection of emission or absorption lines in the spectrum depends directly on the total number of photons detected. Since most observational work in cosmology is on faint objects, it is probably realistic to say that the rate of observational progress is determined by the total light-gathering power of the larger telescopes. On this basis, the completion of the two 150-in. telescopes, now under construction at Kitt Peak and Cerro Tololo, will increase light-gathering power available to U.S. astronomers by around 60 percent. The power would be more than doubled if another 200-in. telescope were built in addition.

Cosmological studies frequently require extended observations of many faint objects–a slow procedure. The triple requirements of a clear sky, no moonlight, and "good seeing" contribute to the slowness of progress in these observational programs. Only one sixth of the total nighttime is suitable for observational work of critical cosmological importance.

Some observatories assign observing time to local and outside scientists on an annual basis, allowing them to plan and to carry out long-term programs. It has not been lack of scientific planning and foresight that has slowed progress in observational cosmology, but rather the lack of large telescopes. If the two new 150-in. telescopes are to contribute significantly to observational cosmology, a sizable fraction of their time should be committed a year ahead, in large blocks, to a limited number of astronomers on submission of outstanding long-term programs.

Beyond the obvious hazards such as clouds and moonlight, the main problems besetting the astronomer observing faint objects are seeing and night-sky emission. The night-sky emission consists of molecular bands, simulating an irregular continuum at low spectroscopic dispersion, and a few strong forbidden atomic lines. Near cities there are scattered continuum and mercury-line emission. The contamination of a star's spectrum by that of the night sky depends on the size of the "seeing" disk, the apparent angular diameter of the star due to atmospheric turbulence. The seeing disk is usually about 1 to 2 sec of arc in diameter at selected observing sites. The night-sky light within the seeing disk is as bright as a star of apparent magnitude 21, making it difficult to detect faint spectral features in stars much fainter than 19th magnitude (such as the sun, were it a faint member of a globular cluster) or in galaxies where surface brightness is added to the light of the night sky.

Since the night sky represents the ultimate limitation in earthbound observations, the performance of an instrument used in observing faint objects is characterized, first, by the time required to detect the night sky and, second, by the feasibility of subtracting the effect of the night sky from the observed spectrum of the faint object. The time to detect the night sky is essentially measured by the optical efficiency of the spectrograph and the quantum efficiency of the detector. The detector efficiency has improved considerably in recent years through the replacement of the photographic plate (efficiency of about 1 percent) by the photoelectric effect (efficiency of about 20 percent) used in image tubes, television tubes, and photomultipliers. In image tubes, the output is usually recorded on photographic emulsions, with their high two-dimensional resolution ability but nonlinear response. The spectrum of object and night sky appear side by side, but the complicated nonlinear response of the plate makes the correction for night-sky contamination unreliable. The output of a photomultiplier is usually in pulse counts corresponding to photoelectrons, hence the detection is linear; there is no imaging, and thus there is no space resolution. The effect of the night sky can be taken into account by subtracting the counts from a neighboring sky area of a size equal to that of the area centered on the star. Multichannel photoelectric spectrometers that employ a row of detectors allow moderate resolution, but in one dimension only.

The development of systems such as integrating low-noise television cameras, which combine the advantages of high-resolution one- or two-dimensional imaging with linear detection response, is feasible now and, though costly, deserves high priority. The speed of the system is high if coupled to image intensifiers. The linearity ensures proper subtraction of the night sky. The system would increase the efficiency of observing very faint objects by a considerable factor. The ideal instrument would detect each photon that strikes each resolution element. It is unlikely that further large gains are possible for ground-based telescopes, since the ultimate efficiency depends on the quantum efficiency of photoelectric surfaces and on the seeing.

The next large step requies a diffraction-limited large space telescope, for which the contamination of the stellar image by foreground light will be very much less than that for ground-based telescopes. The uses of such a telescope for cosmology have been considered in a recent report of the Space Science Board.* According to this report, a properly figured 120-in. telescope in earth orbit should yield images less than 0.1 sec of arc in diameter. The amount of night-sky light within this disk, which contaminates the stellar image, is reduced both because of the smaller disk and because of the absence, in earth orbit, of atmospheric emission. When these effects are combined, a limiting magnitude of 29 appears to be attainable; however, to achieve this in practice would require improvements in imaging devices at the focus. Such a limiting magnitude is 100 times fainter than can be observed from the ground. Therefore, galaxies or quasars like those that we have studied could be observed at ten times the distance, if the effect of space curvature is neglected. Such capability should permit determination of distances to galaxies sufficient to yield the scale and curvature of the universe.

Most of the efficient auxiliary instruments and sophisticated data-handling systems are, and should be, developed in association with the larger telescopes. These systems improve the efficiency of smaller telescopes as well, allowing observational programs such as the extended monitoring of the beat of the Crab pulsar. For some programs a small telescope is called for, since the image scale at the large telescope is excessive; for example, observations of the energy distribution of nearby giant ellipticals, important for the interpretation of red shift–magnitude diagrams, were conducted with a 4-in. telescope and a spectrum scanner at Palomar.

In conclusion, we see the need for several steps to increase the avail-

* Space Science Board, Division of Physical Sciences, National Research Council, *Scientific Uses of the Large Space Telescope* (National Academy of Sciences, Washington, D.C., 1969).

able capability for observing faint objects with large telescopes. The following four recommendations outline these needed steps:

RECOMMENDATION 1. Scheduling of Large Optical Telescopes at National Observatories

Cosmological observations often involve faint objects at great distances, so that large amounts of observing time for objects scattered over the sky are required. To plan and to carry out such observations efficiently, investigators need to have a commitment far in advance for the use of the required instruments on a regular basis, even if this means some loss of short-range flexibility for those investigators. Therefore, *we recommend that substantial amounts of observing time on large telescopes at National Observatories (including the 150-in. telescopes under construction) should be committed as much as a year in advance to investigators with outstanding long-range programs in cosmology.*

RECOMMENDATION 2. Construction of Additional Large Telescopes

Even after the completion of the 150-in. telescopes at Kitt Peak National Observatory and Cerro Tololo Inter-American Observatory, the amount of time available for extragalactic observations on large optical telescopes still will be less than that which could be used effectively. Such observations are of critical importance to cosmology. *We therefore recommend that, in proposing a balanced program for optical astronomy, the Astronomy Survey Committee consider the need for additional large optical telescopes to meet the demand for observing time on cosmological problems.*

RECOMMENDATION 3. Electronic Imaging

The speed and linearity with which one can approach the limits imposed by the night sky can be greatly increased by electronic digital imaging systems. Because of the large investment in telescopes used for extragalactic work and the few nights available per scientist, increasing their efficiency by the use of such systems deserves high priority. *We therefore recommend that each major telescope used for extragalactic observations be equipped as soon as possible with linear digital imaging devices such as high-gain television systems and solid-state arrays.*

RECOMMENDATION 4. Diffraction-Limited Space Telescopes

Ultimately, the efficiency of observing faint objects from the ground is limited almost entirely by atmospheric conditions. In principle this problem can be overcome by a diffraction-limited telescope in space. To be competitive with large ground-based telescopes, a space telescope must also be large. A large diffraction-limited space telescope, if it could be built within budgetary limitations, would be of great value to cosmology and relativistic astrophysics, as it would permit the determination of distances of galaxies with a precision sufficient to yield the scale and curvature of the universe. *We therefore endorse design studies now under way directed toward flying a large diffraction-limited telescope in space, and we recommend that, as such studies proceed, the effectiveness of the space telescope for extragalactic observations be constantly assessed, as part of the planning and budgetary process, to permit comparison with the effectiveness of contemporary ground-based telescopes.*

B. RADIO METHODS

Radio telescopes are limited by sensitivity and angular resolution. There are many problems in which the simplicity of a single antenna is useful, including the investigation of total flux density from known sources as a function of wavelength, polarization, and time. There the primary consideration is sensitivity, and the largest possible aperture is desirable. If, however, one desires information about the spatial distribution within the source, as in the case of radio galaxies in which clouds of relativistic particles are ejected, single antennas often do not provide sufficient resolution. This problem also occurs when one tries to resolve individual faint sources from one another, as in observing distant radio galaxies for cosmological purposes. Then it is necessary to use interferometers or arrays to provide large baselines and high resolution, although not necessarily a large collecting area. By making observations with different baselines, it is possible to build up an image of the source.

For many applications, such as the study of very faint extragalactic radio sources, the sensitivity is limited not by receiver noise but by confusion from unresolved sources in the antenna beam. In these cases the required resolution can be obtained only by using very large arrays. As an example, we note that at 20-cm wavelength the newly completed array in The Netherlands can observe sources one to two orders of magnitude fainter than can a 100-m paraboloid (operating at the same wavelengths), which has about the same physical collecting area and cost.

Present equipment in the United States includes a large variety of single dishes, interferometers, and arrays. Most of this equipment has already been constructed or was in the process of construction at the time of the Whitford report in 1964,* and, although recommendations were made in the report in support of a program of advanced new instruments, none of these instruments has been completed. We find that

* Panel on Astronomical Facilities (A. E. Whitford, chairman), *Ground-Based Astronomy: A Ten-Year Program* (National Academy of Sciences–National Research Council, Washington, D.C., 1964).

many of the proposals made there for instruments to be applied to cosmology are still generally valid.

Future investigation of the cosmological problem by radio observations will require the sensitivity and resolution of large arrays to measure the size and intensity of a sample of the more distant radio sources. Arrays will allow the detection of now undetected galaxies, quasars, and relatively nearby clusters. This will be invaluable in understanding the relation between galaxies and quasars, discovering why or when a galaxy becomes a radio source, and determining the statistical distribution of radio luminosities for analysis of radio source counts. A large array with spectroscopic capability can be used to study the dynamics of normal and unusual galaxies using the 21-cm and other spectral lines.

Arrays capable of a resolution of 1 sec of arc might permit counts of source number versus angular size as a test of cosmological models. This test is analogous to, but more powerful than, the relation between source number and flux density. Such resolution will permit study of the structure of radio sources, as a function of wavelength and polarization, over a wider range of flux density than now possible. Such investigations may uncover a relation between absolute radio luminosity and some observable radio property, which would permit estimates of distance from radio data alone. Extended radio sources need to be studied with high angular resolution and sensitivity, for a better understanding of the nature and evolution of strong radio sources.

The fundamental problem of energy sources may be illuminated by study of compact young sources. Temporal variations in intensity and size will determine significant properties, during and immediately following the release of energy and acceleration of relativistic particles. At early epochs these sources radiate most strongly at millimeter and centimeter wavelengths. Two types of instrument are needed: a large antenna to discover and study the details of such sources and antennas capable of monitoring time variations in the strong sources. For the latter only moderate-size antennas are needed, if provided with state-of-the-art receiving and data-processing techniques.

Investigation of the angular structure of very young and rapidly varying sources requires interferometer baselines of thousands of kilometers—very-long-baseline interferometers (VLBI). Existing radio telescopes can be instrumented, at little cost, to become part of an array with transcontinental and intercontinental baselines with sufficient resolution to study the structure and high-velocity variations of radio sources. VLBI patrols have already begun with a growing fraction of time at radio-astronomy observatories devoted to this type of work. Ultimately, special antenna facilities may be desirable to provide different baseline orientations. The worldwide NASA network of tracking stations is ideally suited for VLBI work with several long baselines between widely spaced stations with excellent low-noise receivers.

The discussion of optical methods referred to the special character of cosmological observations, requiring large blocks of time scheduled considerably in advance and possibly for many years of program. This is also true in radio astronomy, because of the faintness and variability of the sources studied.

In summary, we recognize the need for additional instruments that can map the structure of nearby extragalactic sources with high resolution, observe very distant and faint sources in spite of the confusion problem, monitor variable sources at short wavelengths, discover additional sources at millimeter wavelengths, and study small sources with very high angular resolution. Instrumentation to meet these objectives and requirements for its use are described in the following recommendations:

RECOMMENDATION 5. Large Radio Array

An instrument with a beam width of the order of seconds of arc can map the structure of nearby strong sources, detect additional nearby weak sources, and study distant faint sources in spite of the confusion problem. Such an instrument will make possible both penetrating studies of the physics of radio galaxies and quasars and statistical studies of the number, flux, and angular size of distant sources. Both are of fundamental importance to relativistic astrophysics and cosmology. Spectroscopic capability will add considerably to the power of a large array, by permitting observations of the dynamics of galaxies by means of the Doppler shift of spectral lines. *We therefore recommend that, in proposing a balanced program for radio astronomy, the Astronomy Survey Committee take into account the need for a large array that can synthesize a beam of order of seconds of arc in a reasonable period of time, for study of extragalactic radio sources.*

RECOMMENDATION 6. Large Millimeter-Wave Dish

Recent investigations show that extragalactic radio sources at their most explosive phase radiate most powerfully at the millimeter and short centimeter wavelengths. Additional facilities operating at such wavelengths would discover many new active sources and permit studies of their evolution. *We therefore recommend that, in proposing a balanced program for radio astronomy, the Astronomy Survey Committee take into account the need for a large antenna operating at millimeter wavelengths for observations of active extragalactic radio sources.*

RECOMMENDATION 7. Monitoring Variable Sources

There is a need to monitor variable extragalactic radio sources at millimeter and centimeter wavelengths on a daily basis. This task can be accomplished with a moderate-size antenna equipped with sensitive receivers and rapid data-processing facilities. *We recommend that a moderate-size antenna be equipped for monitoring variable extragalactic radio sources. If an existing antenna is not available, the construction of a new antenna may be required for this purpose.*

RECOMMENDATION 8. Very-High-Resolution Studies

Compact radio sources in quasars and radio galaxies appear to be the early states of a relativistic expansion containing enormous energy; therefore, they are very significant for understanding the energy-production mechanisms in these objects. Studies with very-long-baseline interferometers show that such sources can be resolved into fine and variable details with antenna systems having a resolving power of the order of 0.001 sec of arc, thus yielding important physical parameters of the source. *Therefore, we recommend that existing radio telescopes be equipped as terminals of a very-long-baseline interferometer for very-high-resolution studies of compact radio sources. The NASA network of tracking stations, containing as it does several long baselines between different stations and being equipped with low-noise receivers, is well suited for this work and should be made available for it on a part-time basis.*

RECOMMENDATION 9. Scheduling of Large Radio Telescopes at National Observatories

As in the case of optical telescopes, referred to in Recommendation 1, cosmological investigations require the commitment of substantial blocks of time as much as a year in advance. We therefore expand the scope of Recommendation 1 to include radio telescopes.

C. INFRARED ASTRONOMY

Infrared astronomy is still in its infancy, at least in comparison with investigations at optical and radio wavelengths. It was anticipated that many objects in the universe must be comparatively cool, so that the thermal radiation they emit might peak in the infrared. Using modest instruments and comparatively simple detectors, astronomers have already obtained important results in planetary and stellar astronomy by detecting cool objects that radiate powerfully in the infrared. Of particular importance are objects that could be stars in the process of formation and stars that are rapidly evolving. The infrared radiation apparently comes from vast clouds of dust near the object, which are being heated by the central star.

That extragalactic objects would be exceedingly powerful infrared emitters was not anticipated. For example, from observations so far, it appears that the nuclei of some galaxies emit infrared energy between 1 and 20 μm at power levels 50 to 100 times greater than all the stars in that galaxy. Infrared astronomy has already shown that the nuclei of galaxies–apparently objects in which relativistic effects occur–radiate at power levels so high that they could not continue through the life of a galaxy unless extensive mass flows into the nucleus or matter is created there. These results, of great importance to relativistic astrophysics, have been obtained by a few small groups working largely from the ground. Observations from aircraft and balloons, using a 12-in. telescope, show that the nucleus of our own galaxy emits strongly at wavelengths beyond 50 μm.

We have already discussed whether the microwave background is blackbody radiation generated in a big-bang fireball. Observations to settle this question are needed between about 1 mm and 100 μm. At present, some of the direct observations made above or near the top of the atmosphere, using rockets and a balloon, conflict with the blackbody interpretation. They may conflict with data from interstellar molecules, bathed in the background radiation field. If correct, a major argument for an evolving universe would be eliminated, but the preliminary observations made from above the atmosphere could be wrong. More data are needed to draw any firm conclusions. Critical observations include those of the spectrum at wavelengths below 1 mm and attempts to find deviations from isotropy.

The radiation of so much power in the infrared by certain discrete extragalactic objects is a great puzzle. The mechanism has not yet been identified, but the data point to an extremely large energy source. We must progress far beyond the present fragmentary observations with 60-in. telescopes and with the 120-in. and 200-in. telescopes on Mt. Hamilton and Palomar Mountain. The latter telescopes are located at sites optimized for optical work but not for infrared work.

For progress in infrared observations of extragalactic objects, astronomers need continued access to a large telescope at a very dry site selected for its suitability for infrared work. Such a telescope would be used in all the conventional infrared windows and could also make observations at wavelengths near 1 mm, where windows to be exploited also exist yet. As infrared and millimeter detectors improve, we anticipate that such an instrument will penetrate even further into space, permitting study of increasing numbers of extragalactic objects. This study, including spectra, variability, and polarization, should disclose the nature and source of the extraordinarily powerful infrared emission. It should be noted that such an instrument would be far less expensive than an optical telescope of comparable size, because of the relaxed tolerances associated with the longer wavelength.

Many parts of the spectrum between 1 mm and 1 μm cannot be observed properly unless the observation is done above, or at least high in, the atmosphere. Most of the power in some objects is in the wavelength range between 100 μm and 1 mm. Some observations at these wavelengths can be made with a large telescope from a dry site. However, the appropriate atmospheric windows are rather restricted. Thus, infrared astronomy must also be pursued from aircraft, balloons, rockets, and satellites. The anticipated gain from more flights involving a 12-in. telescope at 50,000 ft in an airplane, a 36-in. telescope at the same altitude, a 36-in. telescope at 100,000 ft on a balloon, or any satellite telescope with good pointing accuracy is great. No quantitative predictions can be made, because so little has yet been done, but it is obvious that this is an important direction in which to move.

A space program devoted to infrared and submillimeter wavelengths is also necessary to measure the spectrum of the microwave background at wavelengths below 1 mm. The techniques are quite different from those used to measure sources, for, if the background is isotropic as expected, absolute flux measurements (rather than measurements of the amount by which a source exceeds the background) are required. In either case, sensitive detectors must be flown high in or above the atmosphere.

The small but rapidly growing number of infrared astronomers in the United States need more technical support, more money for instrumental development, and worldwide surveys to find the best high-altitude sites on which to construct new infrared telescopes. They also need adequate logistic support to enable them to operate observatories at such sites. Interferometry, at infrared wavelengths, will allow measurement of extragalactic objects of exceedingly small size. We do not know yet the extent to which this technique will be developed, but clearly two infrared telescopes will be required. (See Volume 1 for further discussion of the potential of this technique.)

Infrared astronomy could hold the key to several baffling problems in astrophysics and relativity. At present it is an exploratory effort involving a relatively small group of scientists; however, it should be expanded, and more advanced instruments should be made available. The following recommendations offer specific suggestions:

RECOMMENDATION 10. Ground-Based Infrared Telescopes

Exploratory observations with moderate-size telescopes on the ground have disclosed that galactic nuclei are unexpectedly powerful infrared emitters. They are radiating more energy than is available from thermonuclear sources; therefore, they probably involve relativistic effects. Larger-aperture telescopes are needed for studies of the physics of these unusual objects. *We therefore recommend that, in proposing a balanced program for infrared astronomy, the Astronomy Survey Committee take into account the need for a large-aperture telescope at a very dry site equipped and scheduled for infrared observations of extragalactic objects.*

RECOMMENDATION 11. Infrared Observations from Aircraft, Balloons, Rockets, and Satellites

If the interpretation of the cosmic microwave background as the effect of a primordial fireball is correct, its intensity should peak near 1 mm and decrease at shorter wavelengths. Confirmation of the predicted variation at and below 1-mm wavelength is an important test of big-bang cosmology. Experiments for this purpose must be performed from high in or above the atmosphere, where its disturbing effects do not mask the background radiation. Recent observations of discrete sources at wavelengths shorter than 1 mm, including infrared wavelengths, indicate that some extragalactic objects radiate most of their power there. It is important to discover the source of such power and to see whether the additive effect of many such sources could possibly account for a substantial portion of the observed background radiation.

We therefore recommend that, as part of an overall program of space observations, a vigorous program of infrared and submillimeter observations of extragalactic background and sources be pursued from aircraft, balloons, rockets, and satellites.

D. X-RAY AND GAMMA-RAY ASTRONOMY

In the past decade, rapid developments in the instruments and methods for x-ray and gamma-ray astronomy have occurred. We now stand on the threshold of a decade in which satellite experiments in high-energy astronomy can reap a harvest of discovery and new understanding in astrophysics and cosmology.

During 1970 and 1971, all-sky x-ray survey experiments with 1° angular resolution were launched on NASA satellites. Based on balloon and rocket experiments through 1967, these experiments have detected hundreds of x-ray sources by virtue of the great increase in the total exposure that they will provide. Their complementary measurements determine source positions to several minutes of arc, low-resolution spectra from 1 to 60 keV, and variability over time scales from seconds to months.

A second generation of satellite x-ray experiments, now being prepared for 1973–1974 launches, will incorporate many advances in knowledge and technique gained from recent rocket and balloon experiments, in which new methods have been developed and tested for precise position determination, ultrasoft x-ray detection, high-resolution spectrum scanning, and background suppression. These methods are also suitable for much larger satellites such as the High-Energy Astronomical Observatory-A (HEAO-A). Detectors with sensitive areas as large as 10 m^2 or more could achieve a tenfold increase in the sensitivity of an all-sky survey with 1° resolution.

Long-range planning must be concerned now with the third generation of satellite x-ray astronomy experiments in which the emphasis will shift from general surveys to precise position determinations and the detailed examination and measurement of individual x-ray sources through high-resolution studies of their structure, spectra, and polarization. Of particular interest for cosmology is the high-resolution study of the x-ray background, with the aim of determining whether it is a superposition of many discrete extragalactic sources or is truly diffuse and therefore of intergalactic origin. Such experiments must employ grazing-incidence reflection x-ray telescopes to form high-resolution (~1 sec of arc) x-ray images, which can be electronically detected and corrected for residual spacecraft orientation error. The telescope will incorporate auxiliary instruments for high-resolution Bragg reflection spectrometry and polarization analysis. Small grazing-incidence reflection x-ray telescopes have already been used with outstanding success in solar observations that revealed the detailed structure of the x-ray emitting regions around flares. A 12-ft version will be used in the Apollo Telescope Mount-A (ATM-A) solar astronomy mission. A telescope of 20-ft focal length or greater, with an effective collecting area of 1000 cm^2, is within the current state of the art and could be placed in orbit by 1975 as a part of a large x-ray astronomy facility in NASA's projected HEAO series. Because of its high angular resolution, such a facility will have a sensitivity for the detection of faint sources that is quantum limited to about 10^{-7} cm^{-2} sec^{-1}. It could analyze the structure and polarization of extended objects and examine spectra for evidence of emission lines and absorption edges due to the elements from calcium to carbon. Perhaps most important of all, it could achieve position determinations to an accuracy of several seconds of arc, so that definite optical identifications could be achieved on even very faint objects.

New major satellite efforts are in order for the study of gamma rays. The successful low-resolution survey experiment of the Orbiting Space Observatory-3 (OSO-3), which was launched in 1967, was followed in 1972 by a high-resolution gamma-ray spark-chamber experiment on SAS-B. This experiment should delineate in considerable detail the gamma-ray distribution from the galaxy and throw new light on the origin of the extragalactic gamma rays. This instrument, however, is small for the job that needs to be done. The projected HEAO-A spacecraft would be well suited to a sufficiently large-scale gamma-ray experiment to achieve a significant improvement in sensitivity and possibly to permit the detection of extragalactic sources.

The central importance of continued balloon and rocket research to the vigorous growth of high-energy astronomy merits emphasis. Even after the first major x-ray astronomy experiments were orbited in 1970 and 1971, balloons and rockets continued to provide relatively quick and cheap means to explore new scientific and technical ideas, stimulate the development and testing of new instruments and methods, and train students in situations in which they could assume the major responsibility for carrying through entire experiments in high-energy space astronomy. This last function serves a wider need than that of providing competent scientists for future space astronomy. Successful balloon and rocket experiments generally require sophisticated and remote instruments, imaginatively designed for what are frequently speculative scientific purposes. They also require complex data processing and analysis, involving the wise use of computers. These challenges develop experimental skills that can be applied in many other fields of exploratory science.

There still remain large regions of the high-energy photon spectrum where the technical problems of achieving adequate detection capabilities have not been satisfactorily solved. One of these is the important region from 0.5 to 30 MeV, where extragalactic nuclear gamma-ray lines might be found and where the shape of the extragalactic spectrum could have important implications for cosmology. Although significant exploratory results have been achieved, the fundamental problem of obtaining a good signal-to-background performance from a directional detector has not been solved. Another energy region that may require much development before positive results can be achieved is that above 10 GeV, where air-shower techniques appear to offer the only promising line of approach. Here again, exploratory work has defined the character of the problem, but no adequate solution is clearly in sight. It is important that ways be found to support worthy new exploratory efforts in these regions of the high-energy spectrum.

Although present commitments and future plans promise a fruitful program of high-energy astronomy observations during the next decade, support for the critically important coordinated optical observations is inadequate. Rough distance indications can be obtained for x-ray sources in our Milky Way by purely x-ray techniques, but the greatest advances in the understanding of x-ray sources have occurred when they have been optically identified and studied with the precision tools of optical astronomy. Sco X-1, Cyg X-2, the Crab nebula and pulsar, the extragalactic sources M87, NGC 4151, and 3C 273 are cases in point. As in the earlier experience of radio astronomy, observations in the x-ray region of the spectrum provide the indispensable basis for new data revealing new types of phenomena and of objects. Only when the stream of information carried by optical photons is tapped with the powerful methods of optical astronomy can the full benefit of the x-ray observations be realized. Thus a balanced program in high-energy astronomy should provide adequate support for the related optical studies.

The optical counterparts of x-ray sources need to be identified by comparison of x-ray source positions with objects on photographs exposed to the faintest possible limiting magnitudes. Next, detailed studies of the spectrum must be undertaken. Often x-ray sources have proved to be optically variable, so it is important to monitor the optical variations for correlation with the x-ray data. These requirements point to the need for additional observing time on intermediate-size (2–4-m) optical telescopes that can be devoted to the monitoring and study of variable x-ray sources.

Other types of interesting objects are known to be variable, including supernovae, quasars, and galactic nuclei. The evidence on these objects from radio astronomy points to injection of fast particles and to variability at all wavelengths. Thus there is a need to monitor this wider class of sources as well.

Progress in this field can come by applying the most modern techniques of digitized recording of optical photons, including information about direction, wavelength, and polarization, at the focus of intermediate-size telescopes of modest cost. Such techniques can be applied to existing telescopes or, if the number available for this purpose is not adequate, to one or more new intermediate-size telescopes.

The instruments now under construction and planned for x- and gamma-ray astronomy from space portend a rich harvest of results important for relativistic astrophysics and cosmology. For example, detailed studies of the x-ray emission from neutron stars are possible, and large numbers of extragalactic sources will be found and identified. Detailed study of the x-ray and the gamma-ray background, which is of great importance to cosmology, will be accomplished. Consequently, this Panel emphatically supports a vigorous program of high-energy astronomy and makes specific recommendations to that end.

RECOMMENDATION 12. *High-Energy Astronomical Observatory*

X-ray astronomy offers an opportunity to detect relativistic particles at their point of origin by means of their synchrotron and inverse Compton emission. The origin of enormous quantities of these particles in galactic sources such as the Crab pulsar and extragalactic sources such as radio galaxies and quasars is believed to be connected with violent events. X-ray observations may be decisive in disclosing the nature of these events.

The diffuse background at x- and gamma-ray wavelengths is apparently cosmological in origin. Future observations of its spectrum and angular structure could disclose whether it is truly diffuse, therefore intergalactic in origin, or perhaps the result of the addition of a large number of powerful x-ray sources.

The High-Energy Astronomical Observatory (HEAO) proposed by NASA would permit decisive contributions to the study of galactic and extragalactic discrete x-ray sources and of the diffuse x-ray background by achieving large increases in collecting area, angular resolution, and spectral resolution. The usefulness of the HEAO would be enhanced by a guest investigator program similar to the successful programs operating with other scientific satellites. Continued rocket and balloon research in high-energy astronomy will be necessary even after the HEAO becomes operational in order to try out both scientific and technological innovations and to train students. *We therefore endorse the program for the development of a High-Energy Astronomical Observatory, and we recommend that an early start be made on a mission with a large grazing-incidence telescope capable of high angular and spectral resolution. We also recommend that balloon and rocket research in high-energy astronomy be continued at a reasonable level.*

RECOMMENDATION 13. *Gamma-Ray Detectors*

The gamma-ray region from 0.5 to 30 MeV, in which extragalactic nuclear gamma-ray lines may be found, is of particular interest to cosmology because of the light it will throw on explosive nucleosynthesis of the heavy elements in distant galaxies, the rate of which depends on the cosmological model. *We therefore endorse efforts to improve de-*

tectors in the range from 0.5 to 30 MeV and recommend that the best available instruments in this energy range be incorporated in High-Energy Astronomical Observatory payloads.

RECOMMENDATION 14. Monitoring Variable Optical Objects

Several types of optical object of interest to relativistic astrophysics, including x-ray sources, supernovae, quasars, and galactic nuclei, are variable in the optical wavelength range. The time dependence, spectral characteristics, and polarization of these variations can yield significant physical information. To monitor such objects takes a significant fraction of the observing time on an intermediate-size telescope. *We therefore recommend that a number of intermediate-size telescopes be made available for substantial periods for such monitoring activities and that they be instrumented with detectors and data-handling devices adequate for precise and rapid data recording.*

E. COSMIC-RAY PHYSICS

Cosmic rays provide a probe of distant conditions and events that is only beginning to be exploited in astrophysics. Cosmic rays represent a sample of matter from the sun, distant stars, and perhaps even galaxies. Studies of the fast particles from the sun show that the relative abundances of most nuclei are approximately preserved during acceleration and transit from the sun. The chemical composition of the sun can be determined from the fast nuclei ejected during solar eruptions. The solar abundance of neon has been so determined. Presumably, the galactic cosmic rays represent a sample of matter from pulsars, supernovae, and distant flare stars. Studies of the abundance of the various nuclei in cosmic rays at energies below a few GeV/nucleon (possible only with instruments carried on spacecraft) show, for instance, that the ratio of even to odd atomic numbers is large—of the order of ten—indicating that there is equilibrium of the nuclei in a condensed hot state ruling out rapid synthesis of these nuclei by collisions shattering heavier nuclei.

Cosmic-ray nuclei have been observed at energies of only a few MeV/nucleon; the persistence of heavy nuclei (Fe, Ni) at these low energies indicates that they may come from nearby sources, with an overabundance of heavy nuclei. The tracks of very heavy nuclei, such as Pb and U, in plastic detectors implies an enormous overabundance of very heavy nuclei accelerated from their sources.

Cosmic rays originate in energetic events. A few are produced in eruptions on the sun. The galactic cosmic rays seemingly are produced in supernovae and in supernova remnants (the pulsars). The very-low-energy cosmic rays must be produced nearby, perhaps in flare stars. On the other hand, the very energetic cosmic rays (10^{18} eV and above) are not confined to the galaxy and are presumably of extragalactic origin. It would be exceedingly interesting to know the nuclear composition of such very-high-energy cosmic rays, representing matter from distant galaxies.

Cosmic rays exert a significant pressure in space. They inflate the gas of the disk of our galaxy, preventing its collapse. The passage of cosmic rays through the interstellar gas ionizes and heats the gas, causing the gas to move closely with the magnetic field. The magnetic field, the cosmic-ray pressure, and the heating contribute to the instability of the gas. The general instability is the basic cause of the dumping of interstellar gas into clouds, where star formation apparently occurs.

Cosmic rays are deflected and modulated in the solar system by the magnetic fields carried in the solar wind. Observations of the variation of the cosmic-ray intensity at the surface of the earth provided some of the first clues (20 years ago) to the conditions in interplanetary space. With very-high-altitude balloons and space vehicles, it has been possible to develop instrumentation and techniques to explore the subtleties of the cosmic-ray nuclear abundances and energy distribution over the entire lower end of the cosmic-ray spectrum. With such detailed information, progress in the field is rapid.

One outstanding question is the degree to which the solar wind and its magnetic fields reduce the cosmic-ray intensity throughout the inner solar system. Until we send suitable spacecraft instrumentation to the orbit of Jupiter (5 A.U.) and beyond, we will not know the true intensity of cosmic rays in the galaxy—particularly at the low-energy end of the spectrum, where they are much reduced by the wind—nor their integrated energy.

In summary, cosmic-ray physics is a major method of studying high-energy particles accelerated in violent cosmic events and the only method that uses direct samples of matter from the cosmos. The information gained by this method on the total energy density, lifetime, chemical and isotopic composition, and energy spectrum of various types of high-energy particles yields important insights into the nature of the sources where they were accelerated, particularly when combined with data derived from the electromagnetic emission by such events. Further experiments are required to verify the nature, frequency, and spatial distribution of source events, including extragalactic events that may be the source of the most energetic cosmic rays.

This Panel has not made a study of the experimental methods used in this area and therefore has refrained from making recommendations.

However, it wishes to call attention to a critical situation developing in this field and to the response of our sister panels to it. In 1969, the Astronomy Missions Board in its report to NASA,* recommended a vigorous program in particle-and-fields astronomy which has not been strongly implemented. Indeed, in a letter to the National Academy of Sciences Physics Survey Committee dated May 21, 1970, the NASA Associate Administrator pointed out that space physics (which includes cosmic-ray physics) is being deliberately de-emphasized in NASA to encourage other programs. This letter requested the Physics Survey to treat this subject explicitly.

The Panel on Earth and Planetary Physics, in its report to the Physics Survey Committee† has done precisely that. It calls attention to the fact that the 1970 Space Science Board study, *Priorities for Space Research 1971–1980*‡ protested strongly against the rapidity of deflation of funding in space physics. This is significant because the group responsible for this report was broadly representative of the entire space-science community not just space physics. The Panel on Space and Planetary Physics concludes its discussion with this statement, "We recommend that the support proposed for space physics in the 1970 report on priorities for space research be regarded as minimal."

The report of the Panel on Space Astronomy of the Astronomy Survey Committee takes a similar view. It recommends this program:

(a) A deep space probe to a distance of the order of 30 A.U. Such a probe would measure galactic cosmic rays directly, without the disturbing modulation by the solar wind.

(b) Pioneer F and G Jupiter flyby, and the High-Energy Astronomical Observatory programs. These flights will permit a variety of cosmic-ray observations.

(c) A series of high bit-rate interplanetary monitors. These spacecraft would particularly study the effects of the solar wind.

In addition, the Astronomy Missions Board called attention to the cost savings that can be effected by including space-physics experiments on planetary probes, in these words: "Indeed, it is absolutely essential to combine the interplanetary observations in cruise modes to the

* A Long-Range Program in Space Astronomy (NASA, Washington, D.C., 1969).

† Earth and Planetary Physics Panel, Physics Survey Committee, *Physics in Perspective*, Vol. 2, Part B (National Academy of Sciences, Washington, D.C., in press).

‡ Space Science Board, *Priorities for Space Research 1971–1980* (National Academy of Sciences, Washington, D.C., 1971).

planets if costs during the next six to eight years are to be kept within manageable proportions." *After considering these matters, the Panel wishes to direct particular attention to the foregoing recommendations.*

F. SOLAR NEUTRINO ASTRONOMY

The Brookhaven Solar Neutrino Observatory, located deep in the Homestake Gold Mine in South Dakota, is continually improving its measurements of the neutrino flux from the sun; already the indications are quite exciting. The upper limit to the solar flux, which has not yet been positively detected, is considerably smaller than the original expectations generated by theoretical astrophysics. Continued searching will either increase this discrepancy or produce a positive detection. There are good reasons for this search for solar neutrinos: their detection will provide the most direct test of the hypothesis that the sun is generating thermonuclear power. The value of the flux will provide a stringent test of mathematical models of stars, on which much of our interpretation of the entire universe is based. It should be remembered that it takes over ten million years for energy in photons to reach us from the center of the sun but only 8 minutes for a neutrino. Hence the neutrino flux is the only sure indication of the present condition of the interior. This measurement provides an independent check on the physics we have been using to understand the universe. Essential for maximum results would be the development of practical schemes for detecting the lower-energy solar neutrinos from ^{7}Be, ^{13}N, ^{15}O, and the proton–proton reaction.

It should be remembered that the Brookhaven experiment is sensitive only to the total neutrino flux, from whatever sources. Because of its proximity, the sun is expected to be the dominant source. In principle, this supposition can be tested by observing an annual variation, expected to be 13 percent because of the radial motion of the earth in its elliptical orbit. Unfortunately, a much larger detector would be required for this purpose.

A related experiment is the laboratory measurement of neutrino–electron scattering. Such an experiment would measure the strength of the unknown electron–neutrino interaction, a value that plays a major role in the theory of advanced stellar evolution and in the possible coupling of neutrinos to the leptons and photons during the expansion of the fireball in big-bang cosmology.

RECOMMENDATION 15. Neutrino Astronomy

The attempt to detect solar neutrinos is critically important for all astrophysics. It is particularly so for relativistic astrophysics because of its implications for the theory of stellar evolution, the helium content of the sun, and the possible variation of the gravitational constant. These problems are closely related to the determination of the age of the galaxy, the problem of the formation of helium in cosmological models, and the choice between rival theories of relativity, all of which are critical for relativistic cosmology. *We therefore recommend that attempts to detect solar neutrinos be supported adequately until decisive results are achieved.*

G. GRAVITATIONAL RADIATION EXPERIMENTS

The detection of gravitational waves has been claimed recently on the basis of an extended series of experiments. This discovery would be of extraordinary significance and should be checked by continuation and elaboration of the original work and by other independent investigations. If it is confirmed, the result will not only be of fundamental significance for physics but also will imply the existence of totally unimagined relativistic astrophysical objects in our galaxy, the study of which probably will be crucial to understanding the structure and evolution of galaxies and could have major implications for stellar evolution and cosmology. Efforts to verify the detection of gravitational waves and the astronomical study of their sources should receive the highest priority, particularly since such work can be accomplished with modest expenditures.

The detection of gravitational waves bears directly on the question of whether there is any such thing as a "gravitational field," which can act as an independent entity. All actively pursued gravity theories deal with the concept of a gravitational field, so the mere existence of gravitational waves does not exclude any of these theories. (However, detailed properties of the waves can discriminate among competing theories, as discussed in Section VI.) Thus this fundamental field hypothesis has been generally accepted without observational support. Such credulity among scientists occurs only in relation to the deepest and most fundamental hypotheses for which they lack the facility to think differently in a comparably detailed and consistent way. In the nineteenth century a similar attitude led to general acceptance of the ether and atoms decades before the experiments that abolished the ether and confirmed the atom.

The basic style of all physics so far in the twentieth century has been set by the field concept, which arose in electromagnetic theory to replace the vanquished ether. This idea has been so overwhelmingly convincing, when tested in experimental and industrial applications, that scientists have tried to package every other known fundamental domain of physics in the same mold. Field theory is incontrovertibly successful in the case of the electromagnetic field. Application of field concepts to particle physics has been successful in many respects, but there are still many unresolved problems. Confirmation of the gravitational wave experiments would show that this concept is suitable for at least one of the other areas–that of gravitational phenomena–in which it is customarily employed.

The astrophysical implications of the gravitational wave experiments are profound and make it impossible, with any straightforward interpretation, to accept the initial observations without extensive confirmation. This situation is true even for resilient minds already stretched by the preposterous demands that radio galaxies, quasars, pulsars, and x-ray, gamma-ray, and infrared sources make on the astrophysical imagination. The gravitational wave observations could be a manifestation of some as yet unearthed subtlety. Otherwise, one relatively conservative interpretation seems to be to postulate that straightforward theory underestimates the sensitivity of the gravity antennas by several orders of magnitude, so that the emitters then could be only normally exotic by the standards of the past remarkable decade. Another interpretation, using the expected sensitivity, demands that our galaxy have but a small fraction of its original mass, with the bulk of it having been converted into gravitational radiation by a process of nearly perfect efficiency. Most of the energy in the universe might then be gravitational waves from similar galaxies. All models of sources for gravitational waves at the current receiver frequency require that masses of the order of one solar mass move at nearly the velocity of light and change their velocity by the same amount every millisecond in each brief burst of activity. Our curiosity to know whether these ideas must be faced seriously is intense and can be satisfied only by further experiments.

RECOMMENDATION 16. Gravitational Radiation Experiments

Recent experiments suggest that an enormous flux of gravitational waves could be present in space. Confirmation of the detection of such waves would constitute a crucial test of fundamental assumptions underlying the theory of gravitation. A flux of a magnitude even approaching the reported one would have extraordinary implications for astrophysical processes involving relativistic motions of astronomical objects. *We therefore recommend that experiments to detect gravitational waves and the study of their astronomical sources be fully supported.*

H. THEORETICAL STUDIES

Enormous effort has been devoted to the study of the theory of general relativity. From it, mathematical physicists have derived the extension of Newton's laws of gravitation to relativistic speeds, the interaction between point masses, the properties of gravitational waves, and models of stationary, rotating, and expanding masses that can represent stars, galaxies, or the universe. Yet the theory remains a difficult mathematical problem. Although it can be written in a single line, it embraces many nonlinear partial differential equations often having singularities of obscure origin.

The emphasis in the years ahead will be on the application to observable phenomena, such as the behavior of the universe at large red shifts and the rotational and vibrational modes of neutron stars. The flow of information to the theorist only a few years ago consisted of a few red shifts of distant galaxies. Now he must consider the isotropy and spectrum of the microwave background; the number counts of radio sources; the generation of large amounts of x-ray, optical, infrared, and radio power in galaxies and quasars, and the acceleration of fast particles therein; the structure of rotating, magnetized neutron stars; and the emission of gravitational waves by asymmetrically collapsing massive objects. This new information forces the theorist to be relevant to actual physical objects whose properties are constrained by the observations. Solutions of the equations having a high degree of symmetry (which permits mathematical rigor) will be of less interest in the future than solutions having less symmetry, which are obtained only on a computer but are nevertheless more like the real world.

Because of this trend, we can anticipate a rapid growth in the theoretical application of general relativity to the astronomical universe. The object of the work will be twofold. First, because general relativity itself is still not beyond doubt, detailed model calculations will be undertaken for the universe and relativistic objects, such as massive stars, to verify whether relativistic models will fit the data. If and when the correct theory is finally established by comparison with the data, such models will serve as an analytical tool to relate the observations to the basic phenomena. A historical prototype of this activity is the study of stellar structure based on the Newtonian theory of gravity and the quantum theory of the atom. Machine calculations based on straightforward but complex equations make it possible to infer the age, mass, composition, and internal temperature of a star from its external characteristics, such as luminosity and surface temperature. In the same way, we can ultimately hope to know the internal structure of a quasar, using the equations provided by general relativity.

One of the special problems encountered in this effort is that of singularities. Already there are available quite general theorems that prove that singularities must occur in a broad class of general relativistic solutions. This situation is almost unprecedented in classical physics, and the meaning is still obscure. As an example, we mention that in big-bang cosmology the whole universe emerges from a singularity in which density and temperature are infinite. On the one hand, the classification and prediction of these singularities will demand the efforts of mathematicians. On the other, singularities will call for the scrutiny of theoretical physicists, who no doubt will remain skeptical of any physical theory that predicts them. Such scrutiny might finally yield a modification of the theory of general relativity—for example, its quantization—which becomes important under extreme conditions and which may prevent the system from becoming truly singular.

As an example of a definite theoretical problem of observational interest that requires penetrating analysis, consider the collapse of a dead star whose mass is greater than that of a neutron star in equilibrium. Such an object must collapse at nearly the speed of light, each part interacting with every other according to the nonlinear field equations. Even if the configuration is relatively spherical at the start, rotation will ensure that it becomes less symmetric as the collapse proceeds. Moreover, it is likely that the system will be unstable, as gravitational energy can be released in various deformations of the surface or in subcollapses of internal parts. The whole object will be radiating such intense gravitational waves that precise calculation of them will be necessary to evaluate the radiation reaction, which affects the collapse itself. This calculation will require joint solution for the variables both inside and outside the star. As the object collapses, singularities develop, the dimensionality of which depends on the symmetries maintained in the collapse.

The solution to such a problem will require the services of mathematicians and physicists of the highest intellectual caliber. They should have ready access to computers of the greatest possible memory capacity and should work in close association with specialists in astrophysics, nuclear physics, plasma physics, and the like so that the relevant physical phenomena can be included as necessary.

RECOMMENDATION 17. Theoretical Studies

Application of the equations of general relativity to observable objects will be important to verify the correctness of the theory and to interpret correctly the basic phenomena that can be inferred only indirectly from observations. This activity will require the efforts of mathematicians and physicists of the highest intellectual caliber, together with the judicious use of the most powerful computers available. *We therefore recommend that individuals and groups doing outstanding theoretical* *work in astrophysics and relativity be adequately supported and that the most powerful computers be made available to them.*

I. INSTITUTIONAL ARRANGEMENTS

In general, astrophysics flourishes best when there is good contact between physicists and astronomers and between observers or experimentalists and theorists. To any problem in astrophysics, the physicist brings his knowledge of the basic laws, which is vital to the proper understanding of the astronomical phenomenon, while the astronomer brings his knowledge of the astronomical context, important if a relevant and not misleading interpretation is to be found. The theorist will bring to the problem a knowledge of relevant mathematical technique and interest in the phenomenon as a manifestation of a more general class, while the observer or experimentalist is expert in experimental technique, aware of the limitations of his data, and vitally interested in tying together the diverse observational phenomena of which he is constantly aware.

Often we find a physicist–astronomer joined in one person; similarly, some astrophysicists are equally skilled in theory and observation. More often, however, we are dealing with different people, and in that case conscious steps must be taken to promote good communication. Virtually all departments of physics and astronomy place substantial emphasis on both theory and experiment. To encourage interaction between physics and astronomy, some universities have combined the two departments into one. In other universities this action has not been taken, but efforts are made, such as joint seminars and degree programs, to encourage communication between the separate departments.

How does relativistic astrophysics fit into this framework? It seems that the most productive institutions are those in which the interrelationships just described are most lively. So far there seem to be three discernible groups of workers in the field. First, there are those working on the theory of general relativity, who usually have a background in mathematical physics and who often work in mathematics or physics departments. Then, there is a small number working in physics departments on the experimental verification of the theory. Finally, there is a large and diverse group of astrophysicists, both theoretical and observational, who are studying a great variety of astronomical phenomena that have at least some relativistic aspects. These individuals can be at home in both physics and astronomy departments. Each institution must experiment with the mix of these groups and the departmental arrangements that lead to the most effective programs. It may be helpful to institutions not yet active in the field to know that the mutual stimulation provided by the three groups is a benefit that can be realized by making at least two or three positions available.

We have alluded several times previously to the fact that the instruments of importance to cosmology tend to be the largest of those employed by astrophysicists and astronomers because of the faintness of the objects studied. For financial reasons, many of these instruments will be built at national observatories, where they are accessible to the entire community, or at least at private institutions willing to share the facilities with outside users. The concentration of large instruments at a few institutions places a special responsibility on the management of these institutions to attempt to ensure that significant work on such long-range problems as cosmology takes place in spite of the many competing demands for observing time. We have already discussed this problem in Sections IV.A and IV.B, and Recommendation 1 also is pertinent to it.

V. IMPACT ON OTHER BRANCHES OF SCIENCE

A. OTHER BRANCHES OF PHYSICS

Astrophysics and relativity has considerable impact on other branches of physics. Its role in the testing of rival theories of relativity will be discussed in Section VI. Here we consider only the relation to the theory of elementary particles and to other better established subfields of physics.

Elementary-particle theory is strongly related to the theory of the early phases in the big-bang model of the universe. If current versions of the big-bang model are correct, then the universe once was vastly denser and hotter than now. To be able to solve the gravitational field equations, we need to know the equation of state of the matter and radiation present in the early universe, but our present understanding of particle physics is inadequate at temperatures above about 10^{12} K to 10^{13} K, at which strongly interacting particles are produced copiously in thermal equilibrium. One way to deal with this problem is to treat the matter as consisting of a number of species of highly relativistic free particles. If we take a fixed number of species (say, photons, gravitons, leptons, and nucleons), then the temperature of the universe is inversely proportional to its radius. If, on the other hand, we take as many species of particles as would exist in thermal equilibrium according to a currently fashionable model of strong interactions (the Veneziano model), then the temperature varies much more slowly. Probably no free-particle model makes sense; and to understand the early universe, elementary-particle theorists will have to leave the familiar conceptual framework of S-matrix theory and venture into the unknown territory of relativistic many-body physics.

One by-product of a realistic model of the early universe might be a clue to the existence of the hypothetical fundamental particles of strong-interaction physics—the quarks. Using a crude model, it has been estimated that if quarks are real, then enough should be left over from the hot early universe to make their current abundance about equal to that of gold atoms. Needless to say, quarks are a good deal rarer on earth than gold, and it would be important to know whether this absence really means that quarks do not exist.

Finally, we hope to learn more about elementary particles from the study of specific astronomical objects such as neutron stars and quasars. For example, the present theoretical uncertainty in the electron–neutrino interaction might be removed by studies of stellar evolution, in which, as stars approach the neutron-star phase, the annihilation of electron–positron pairs into neutrinos that can escape the star probably plays an important role. Neutron stars are fairly well described by nuclear physics, but there could be marginal effects depending on unknown physics. The strange phenomena associated with quasars offer hope of discovering even more unusual conditions of density and temperature that are not consistent with terrestrial physics. It has often been suggested that conservation of baryon number (a rigorous law of terrestrial physics) might be violated, perhaps by creation of matter as required by steady-state cosmology. The discovery of baryon number nonconservation would remove a long-standing puzzle: Why is there not an electromagnetic field coupled to baryon number as there is to charge, another conserved quantity? At the same time, of course, such a discovery would have profound implications for the origin of matter and would provide a possible explanation for the enormous energy output of quasars.

The impact on other branches of physics is pervasive. One could argue that Newton had to invent calculus and modern mechanics to solve an astronomical problem and that they have subsequently found extraordinarily wide application in physics. The theory of atomic spectra was constantly challenged by the study of the sun and stars. Thermonuclear energy production was studied first in connection with the energy source for the sun and other stars and subsequently found spectacular terrestrial application. No doubt future developments in astrophysics will have equally unexpected and major impacts on physics.

Branches of physics that are supposed to be fully understood in principle have to be applied in a detailed and concrete way to situations that differ greatly from usual laboratory conditions. Physicists have thought for a long time that they understood thermodynamics and deviations from thermal equilibrium. Interstellar space is further out of thermal equilibrium than any laboratory apparatus—one finds both cosmic rays with an effective temperature of about 10^{14} deg and dust grains with a temperature of about 10 deg occupying the same region. One consequence of these deviations from equilibrium is maser action in interstellar space. Historically, masers were invented in the laboratory before they were discovered in the interstellar gas, but the problems posed by interstellar conditions stimulated research that is leading to an understanding of the relevant cooperative phenomena.

One aspect of this impact through application is cross-fertilization of different branches of physics. In discussing radiation mechanisms for pulsars, one has to employ both relativistic mechanics and plasma theory in detail; in calculating nuclear reaction rates in very dense stars, one needs solid-state techniques for dealing with zero-point vibration modes, as well as nuclear dynamics and the like. One characteristic flavor lies in the painstaking, matter-of-fact application of fundamental physics to the most bizarre and fanciful conditions in which natural objects find themselves. In particle physics the distinction between simple and composite particles is argued on a very esoteric level; for massive neutron stars this distinction can mean the presence or absence of Fermi pressure and, hence, the difference between stability and collapse. The fundamental study of strange particles is helped, at least indirectly, by their importance in engineering-type calculations for neutron stars.

On another level, cosmology raises questions about the division (always assumed to be valid) between the local laws of physics (which regulate what must be) and the actual properties of the universe (which govern what actually is). Laboratory physics is based on approximate symmetries such as charge-conjugation invariance (which states that the rates of particle reactions are nearly equal to those for the corresponding antiparticles). The universe probably does not embody these symmetries in its initial conditions, as matter appears to be much more abundant than antimatter. Is it possible that an interaction between the whole universe and local phenomena keeps the symmetry laws from being perfect? As yet there is no theory of this interaction, but it is a possibility. Another example is electrodynamics, in which the equations indicate a perfect symmetry between advanced and retarded potentials. The fact that only the retarded potential is actually observed could be related in some way to interactions with all the particles in the universe, which are known to be expanding rather than contracting. If this were true, the results of laboratory experiments might be intimately tied to the present state of the universe.

B. OTHER BRANCHES OF ASTRONOMY

The astronomer's laboratory is the universe. Since 1929, when the universe was found to be in a state of expansion, cosmology has been the central subject in astronomy. In an evolving model, the early more condensed stages of the universe contained the starting conditions for the formations of galaxies, clusters of galaxies, and quasars and, in particular, the Milky Way galaxy in which we live. Therefore, even astronomers studying stars within the Milky Way need the results of research on cosmology, just as cosmologists need the results of stellar and galactic astronomy on distances, time scales, and chemical abundances.

The finite velocity of light allows the astronomer to observe directly objects in the earlier stages of the universe. The light of such objects has traveled for many billions of years before it finally reaches us in the twentieth century. Even if the objects are as luminous as galaxies or quasars, at these enormous distances their signals on arrival at earth are weak. Large telescopes (x-ray, optical, infrared, or radio) are required to collect sufficient information in a reasonable time. As a result, observational advance in cosmology is primarily determined by the size and number of large telescopes. These telescopes are then available to study a host of other objects that may be intrinsically faint although relatively close by. In this way, construction of instruments for the study of cosmology tends to spur activity in all branches of astronomy.

C. EARTH SCIENCE

Although not always readily apparent, there are subtle relationships between the science of our solar system and relativistic astrophysics. Perhaps the most important connection is that between geochemistry and the origin of the elements. Much detailed information about abundance of the elements is determined from solid bodies in the solar system. The meteorites are preferred for this purpose, because they have apparently undergone much less chemical fractionation than has the earth–moon system. Because of their complexity, only a thorough understanding of their formation and chemical evolution will allow a confident interpretation of the conflicting abundance patterns revealed by them. For example, the abundance ratio $^{232}Th/^{238}U$ is quite variable in meteorites and lunar samples, and the correct ratio is important in determining the age of the galaxy. The choice between general relativity and the scalar-tensor theory of gravity may depend ultimately on an understanding of the geochemical fractionation among the elements U, Th, and Pb, because the two theories would assign different time scales to the galaxy.

Meteoritic and lunar sample studies offer a hope of finding stable superheavy nuclei. Some scientists have suggested that fission tracks due to such nuclei will be preserved in the meteorites and made visible by etching techniques. Definite information on the existence of superheavy nuclei would stimulate anew the study of the highly collapsed objects in which such nuclei might originate.

The question of the origin of life and the possibility of interstellar communication with life outside the solar system depend in part on the origin of planetary atmospheres, which in turn depends in subtle but significant ways on how planetary systems are born and how much young radioactivity is trapped in solid objects as they form—radioactivity capable of tipping the scales between molten and solid bodies. The rate of production of these same radioactivities plays a major role in the analysis of the age of our universe.

In addition to these specific examples, there are also philosophical relationships between cosmology and earth science. Broadly stated, earth science attempts to account for the origin of the earth and planets at a definite time in the past, their geological evolution through tectonic and atmospheric action, and the emergence and evolution of life on their surfaces. This evolutionary scheme probably is being re-enacted, with variations, countless times throughout the galaxy and the universe. Few scientists today believe that terrestrial life is unique, because astronomical research indicates that the building blocks of the universe—the stars and galaxies—are remarkably similar. Not only are there billions of stars virtually identical to the sun that presumably have planets similar to the earth, but also the relative abundances of chemical elements available for the extraordinary process we know as life seem to be virtually the same everywhere. This uniformity is simply a reflection of the uniformity of the universe as a whole. For example, carbon, a key element for life, is present everywhere, because galaxies were formed throughout the universe with similar properties. Therefore, the stars within them have similar masses and rotation rates and similar evolutionary histories. Hence the process in which three alpha particles in the interior of the star join to form carbon nuclei proceeds similarly everywhere.

Thus, evolution of the earth and of life should be viewed not as an isolated phenomenon but as one typical of a huge number of similar events scattered throughout the universe. Cosmology forms the giant canvas on which the evolution of the universe is painted, but life is given to the picture by those mysterious processes, occurring throughout the cosmos, in which matter ultimately evolves to consciousness, so that the universe becomes aware of itself.

VI. TESTING GENERAL RELATIVITY

A. PHILOSOPHY

In considering astrophysics as a bridge between physics and astronomy, two aspects of the science merit special attention. One relates to situations in which a new phenomenon of particular interest to physicists is encountered in the astronomer's domain. In the other, relativistic gravitational phenomena are involved.

The unique importance of astronomical bodies for testing general relativity results from the great weakness of gravitation. The strength of the gravitational interaction between two elementary particles is roughly 10^{-40} of the electromagnetic forces that dominate laboratory physics. In other words, the amount of space curvature under general relativity is negligible over dimensions of space–time as small as a laboratory or the two years' duration of PhD research.

Tests of general relativity require bodies of astronomical size. If an astronomical body is as massive as the sun and as compact as a neutron star, the relativistic effects should be quite large. In fact, the binding energy is ~100 MeV/nucleon, larger than any other known force in nature. Throughout the solar system, relativistic effects are miniscule, but the possibilities of precision measurements in the solar system have led to the only two presently known positive tests of general relativity: the gravitational deflection of light and the relativistic rotation of Mercury's perihelion.

It is a mistake to think of these two relatively poor observations as providing our only basis for relativistic gravitational theory. More important are the null gravitational experiments and the various laboratory experiments on which we base our confidence in special relativity, the root-structure of general relativity. Of the null experiments, the spatial isotropy experiment and the modern version of the Eötvös experiment deserve particular notice. These are important in eliminating a large number of otherwise possible theories and in supporting the equivalence principle. Any acceptable gravitational theory must automatically yield a gravitational acceleration of small bodies substantially independent of composition (to less than one part in 10^{11}). It must be explicitly noted that similar observations involving very massive falling bodies are much poorer.

The isotropy experiment effectively eliminates any theory leading to significant anisotropic gravitational-inertial effects. Thus, a Lorentz-invariant tensor theory of gravitation can be eliminated unless its form permits the unification of the Minkowski metric tensor and the field tensor. Similarly, the various null experiments and other laboratory-based tests of special relativity impose limits on gravitational theories. Any acceptable relativistic theory of gravitation might be expected to yield special relativity over the small volumes of space-time required for laboratory physics, and such an acceptable theory must yield results for laboratory physics in agreement with the observations.

With the assumption that the gravitational theory shall have Lorentz-invariant (or special relativistic) roots, a formal machinery exists ready-made for the description of gravitation, that is, the Lorentz-invariant field theories already developed for the description of electromagnetism and particle physics. Gravitation requires one or more chargeless, massless boson fields for its description. Allowable fields have spin 0, 1, or 2. A theory using a neutrino field (spin ½) has certain technical difficulties. The spin-1 field (the analogue of the electromagnetic field) leads to a repulsive force and has other difficulties. Only the spin-zero (scalar) field, spin-two (tensor) field, or a mixture (scalar–tensor theory) remain for consideration.

The scalar field theory developed by Nördstrom in 1912 is satisfactory in most ways, but its predictions concerning the gravitational deflection of light and the relativistic perihelion rotation are not in agreement with observation. Both the tensor theory and the scalar–tensor theory are satisfactory, and it is not yet clearly established which of the two theories receives the most observational support. The scalar–tensor theory has certain interesting properties; for example, it provides insight into, and a means of calculating, the coupling constant of gravitation, but this theory is more complicated than the tensor theory, as it requires two fields. For this and other reasons few scientists favor the scalar–tensor theory on either philosophical or aesthetic grounds. But observations should be the primary concern, not aesthetics.

A modest goal for the future is to devise a test of gravitational theory sufficiently accurate and unambiguous to permit the exclusion of one or the other of these two theories. A more ambitious goal is to devise enough independent tests of the remaining theory to provide strong observational support for it, if sufficient funds for the tests were made available. If it should happen that neither of these two theories is tenable, the theoretical implications would be both serious and interesting.

B. EXPERIMENTAL TESTS USING THE SOLAR SYSTEM

It is a measure of Einstein's genius that in his first comprehensive paper on general relativity, his tensor theory of gravitation, he suggested all the positive tests of general relativity known until very recently. These are the gravitational red shift, the gravitational deflection of light, and the relativistic rotation of Mercury's perihelion. The gravitational red shift does not require the full machinery of general relativity for its discussion and is more properly considered to be a test of the equivalence principle. Thus it is more closely related to the null experiments than the other two tests, which investigate the particular form of the metric about the sun and distinguish between the two theories. Under the scalar–tensor theory, the gravitational deflection of light should be $(1 - s) \times (1.75$ sec of arc) for a light ray passing close to the sun. Here, 1.75 sec of arc is Einstein's value for the deflection of a light ray grazing the sun's limb and $s = 1/(2\omega + 4)$ is the fraction of a body's weight due to the scalar field under the form of the theory for which Einstein's equations are formally satisfied, where $\omega \sim 5$ is the coupling constant of the scalar–tensor theory. If $\omega = 5$ (see below), $s = 0.07$, a measurable effect in the deflection of light.

The classical measurement of gravitational light deflection during solar eclipses has given rather poor results, but the techniques used probably could be substantially improved. In this case the importance of a single measurement is so great that a sustained effort to develop a special instrument and technique might be warranted. The great importance of this observation stems from the relative lack of ambiguity in its interpretation. The light deflection due to the solar corona should be much too small to be significant. To avoid the necessity for a total eclipse of the sun, an instrument is being developed capable of photoelectrically measuring star positions near the sun. An alternative approach is to use radio waves, which are less seriously affected by the glare from the sun. Using long-baseline interferometers, one can determine the positions of point radio sources near the sun, it is hoped with enough precision. Here the refractive effects of the solar corona can be important, requiring the use of two or more wavelengths or a sufficiently short wavelength. These approaches should be capable of distinguishing between the two gravitational theories. Experiments so far performed have yielded a value of $s = 0.06 \pm 0.06$, precision as yet insufficient to distinguish between $\omega = 0$ and $\omega = 5$.

An alternative approach makes use of the retardation of a radar wave passing the sun. This retardation is closely related to the deflection of the wave, as it is the retardation near the sun that wheels the wavefront about, changing its direction. This effect in radar returns from Mercury and Venus has been used to determine the related light deflection, obtaining the result $s = 0.1 \pm 0.2$, where the error is twice the standard deviation.

All three of the new techniques, as well as an improved technique based on a solar eclipse, seem to afford the precision necessary to discriminate between the two theories. Because of the relative lack of ambiguity in the interpretation of results, this type of observation shows the greatest promise of conclusively excluding one of the two competing theories.

The relativistic precession of Mercury's perihelion was known by the middle of the nineteenth century as an unexplained excess motion. At present, the analysis of the observations of Mercury give an excess motion of 42.3 ± 0.5 sec of arc/century, compared with Einstein's calculated relativistic motion of 43.3 sec of arc/century. Only the planetary perturbations are subtracted to obtain the above result. If the observed solar oblateness of 5×10^{-5} has been correctly interpreted as implying the existence of a solar gravitational quadrupole moment, an additional perturbation of 3.4 sec of arc/century must be subtracted, leaving 38.9 sec of arc/century for the relativistic effect. Under the scalar–tensor theory, the relativistic rotation of Mercury's perihelion is $(1 - {}^4\!/_3\, s) \times$ 43.3 sec of arc/century. This expression would yield the observed value of the precession corrected for the quadrupole effect (38.9 sec of arc), if $s = 0.07$ or $\omega = 5$. Thus, scalar–tensor theory, with $\omega = 5$, is favored if the sun has the quadrupole moment indicated by the oblateness observations. The discrepancy with conventional general relativity imposed by the solar oblateness is in excess of three standard deviations, if there are no systematic errors. However, there has been considerable controversy about the significance of the solar oblateness.

Space science provides an unusual opportunity for observing and separating the relativistic and quadrupole effects. An artificial space probe moving about the sun in an elliptical orbit and carrying a radar transponder could provide such a new measurement of a relativistic perihelion rotation. Longevity of the space probe and a careful compensation of radiation pressure appear to be essential for a successful experiment of this type. An alternative and more romantic approach is to soft-land a transponder on one of the asteroids. Radiation pressure is too weak to affect significantly the motions of large asteroids. Both of these approaches would also permit the determination of "light deflection" using the retardation method if the transponder operates in the shorter microwave region of the spectrum.

Other relativistic effects should be measurable in the solar system, but they have not yet been observed. Among the interesting effects are the Thirring-Lense, or inertial drag, effect, which should appear near a rapidly rotating massive body, and the geodesic precession of a gyroscope, caused by translating it through a closed orbit in curved space. In principle, these effects could be observed as a slow precession induced in a gyroscope. For such a gyroscope, a freely floating spinning top in a satellite orbiting the earth has been suggested. A satellite system to measure these effects is presently being designed.

Unfortunately, present accuracies do not permit a definite exclusion of one of the two competing theories, but this situation provides a challenge for the immediate future. It is hoped that the response to this challenge will be a gradually improving case for the correctness of the remaining theory. If neither theory should be correct, the situation would be even more interesting and could lead to an entirely new approach.

C. COSMOLOGICAL TESTS INVOLVING OBJECTS OUTSIDE THE SOLAR SYSTEM

Relativistic effects are miniscule in the solar system, but the close proximity of the sun and planets permits measurements sufficiently precise to be of interest. Outside the solar system compact systems for which relativistic effects are appreciable exist (neutron stars and possibly quasars). Here there is a possibility of investigating gravitational theory through gravitational radiation. If any of these compact bodies were to be a source of gravitational radiation, this radiation might conceivably be a source of information about the radiation mechanism as well as the radiating system. Gravitational radiation, whatever its source, is capable of providing a test distinguishing between the tensor and scalar–tensor gravitational theory. A spherical resonator can be excited in a radial mode by gravitational radiation under the scalar–tensor theory but not under the tensor theory.

Compact bodies are not the only possible sources of new gravitational tests. The enormous stretches of space and time available in the universe offer the possibility of observing relativistic effects even though the average space curvature is small. Cosmology is capable of providing interesting tests of gravitational theory, but this approach suffers from incomplete data and ambiguity in the interpretation of the available data. Several misconceptions exist concerning cosmology as a source of tests of general relativity. Thus, the existence of an expanding universe that appears to be reasonably isotropic and uniform in mass distribution, in the small part seen by us, is sometimes interpreted as support for general relativity. This view is based on the existence of expanding space solutions of the field equations of general relativity. Solutions of this type exist in both relativistic theories, the tensor theory and the scalar–tensor theory, but the dynamical equations that relate the deceleration parameter to the mass density do not require a relativistic calculation. Also, the initial conditions leading to the uniform and isotropic solution are as mysterious under relativity as under Newtonian cosmology. Although we currently do not understand the origin of this degree of order in the universe, we may some day be able to use the observations to test a theory of the origin of the order, if such a theory appears.

One particularly interesting but still unobserved aspect of relativistic cosmology concerns the relation between image size and distance. For relativistic cosmologies, a minimum angular size occurs at a certain distance that is similar for the tensor and scalar–tensor theories.

Under the tensor theory, there are no locally observed cosmological effects. The enormous mass of the universe expanding away from us is without effect on our galaxy or solar system. There has been confusion about this point. Some publications mistakenly claim that the galaxy becomes entrained in the expanding space and expands slowly with time.

Under the scalar–tensor theory there is a possibility of observing locally the effects of the matter distribution. The scalar, being generated by the matter content of the universe, increases slowly with time. This growth results in a decrease of the gravitational "constant" with time. The weakening of gravitation with time carries a number of implications. In principle, these implications could provide information that would lead to the rejection of one of the two relativistic gravitational theories; in practice, this result is not yet possible. The following examples show the difficulty inherent in this type of gravitational test.

The present rate of decrease of the strength of gravitation expected under the scalar–tensor theory depends on ω and the present matter density. A rate of decrease of 10^{-11} part per year would be reasonable. This carries the following implications if the scalar–tensor theory is correct: Our galaxy and solar system should be expanding at the rate of $\sim 10^{-11}$/year, and periods of planets and the moon should be increasing $\sim 2 \times 10^{-11}$/year as measured by atomic clocks.

Other differences between expectations under the tensor and scalar–tensor theory depend on the effect of weakening gravitation on stellar luminosity. For solar-type stars this effect varies as the seventh power of the gravitational constant. This variation affects the red shift magnitude diagram slightly and the computed ages of population II stars substantially, making them smaller. It could effect the temperature of the earth in the remote past.

If we assume the correctness of cosmology based on the expansion of an initially hot fireball, expectations will differ under the two gravitational theories. Under the tensor theory, nearly independent of present mass density and neutron half-life, roughly 27 percent helium is formed in the fireball (assuming a ratio of neutrinos to nucleons $<10^7$). Under the scalar–tensor theory, if the scalar field contribution to the energy density in the fireball is sufficient, the early expansion rate might be sufficiently accelerated to stop helium formation. The initial fragmentation of the expanding fireball to form gas clouds (some of which may be represented by fossil remnants in the form of globular clusters) depends on gravitational theory. Under the tensor theory, the expected mass of globular clusters can be almost an order of magnitude greater than under the scalar–tensor theory.

The techniques of space science also could be capable of yielding a gravitational test. The original motivation for placing a laser reflector on the moon was to test for the acceleration of the moon's motion expected under the two theories. Referred to a planetary ephemeris time scale, the acceleration due to tidal interactions is known over the past 200 years. Assuming that the tidal interaction has been constant for the past 200 years and will continue constant for the next ten years, we can separate the two effects and calculate the effect of weakening gravitation if this occurs. An alternative approach is provided by planetary radar. This technique is capable of detecting in a decade the gradual change of planetary periods, should changes occur. The lunar laser reflector is capable of still another type of test of gravitational theory. Under the scalar–tensor theory, the gravitational acceleration of a body depends on the fractional contribution of gravitational self-energy to the body's mass. This contribution is appreciable for planets and differs for the earth and moon. This difference in acceleration leads to a displacement by less than a meter in the moon's orbit relative to the earth under the scalar–tensor theory.

RECOMMENDATION 18. Testing General Relativity.

The choice between rival theories of gravitation cannot be made conclusively on the basis of present data. This choice is of fundamental physical significance. Moreover, work in relativistic astrophysics depends critically on this choice. *We therefore recommend that experi-*

ments using optical, radio, and radar methods to observe the deflection of electromagnetic waves by the sun, the retardation of such waves passing the sun, the precession of the perihelia and apsides of bodies orbiting the sun at various distances, and a possible lengthening of the orbital periods of such bodies be supported and emphasized within a well-balanced program of ground-based and space-based astronomy. In addition, the use of artifical satellites to detect the inertial drag and geodesic precession in earth orbit should be supported.

They're only one
day over-due!

CHAPTER NINE

Statistics

PANEL MEMBERS

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The report of the Statistical Panel of the Astronomy Survey Committee is in four sections. Section I discusses the results from a questionnaire sent to all known university departments of astronomy, astronomical observatories, and other astronomical research organizations. This questionnaire was prepared by the Statistical Panel under the supervision of Geoffrey Keller, the Panel's chairman. Returns from the questionnaire were analyzed by Terry P. Roark and Gerald H. Newsom and form Section I of the Panel report.

A second questionnaire was prepared by Richard E. Berendzen and sent under the auspices of the Astronomy Survey Committee to recent recipients of the PhD degree in U.S. astronomy departments. Berendzen also gathered currently available manpower data from sources such as the National Research Council Office of Scientific Personnel Doctorate Record File, the National Science Foundation's National Register of Scientific and Technical Personnel prepared by the American Institute of Physics, and records on graduate study maintained by the U.S. Office of Education of the Department of Health, Education, and Welfare. These data on manpower and employment were analyzed by Berendzen and Robert O. Doyle and are presented in Section II of this Statistical Panel report.

Section III consists of a report on federal funding of astronomical research that was prepared by Doyle, working with astronomy program representatives from the National Science Foundation and the National Aeronautics and Space Administration to document recent astronomy research budgets of the major federal funding agencies.

William E. Howard supervised the preparation of a list of all U.S. optical and radio telescopes, which forms Section IV of this chapter.

An attempt was made to provide as complete and comprehensive statistical information as was available from a wide range of sources. Inconsistencies may seem to exist in the values in different tables of what appears to be the same item. The *precise* definition of the quantities involved in most cases will explain any difference. However, the different numbers may nominally refer to the same item but be from different sources (who used different methods of data gathering). All such numbers are reported as they appear in the original sources; if corrections or reinterpretations are made, they are clearly indicated.

I. QUESTIONNAIRE RESULTS

A. INTRODUCTION

In October 1970, the Statistical Panel of the Astronomy Survey Committee sent out 171 questionnaires to all known U.S. university departments and institutes, government laboratories, corporations, and all independent observatories and laboratories engaged in research or teaching or both in astronomy. As of June 3, 1971, 141 responses had been obtained. Of these, 106 were judged complete and were entered in the statistical analysis without adjustment. Those who did not respond and whose responses were in question were again asked to participate or reconsider their responses. A significant number of institutions did acknowledge our request, including all major ones. For the institutions that had still not responded by June 3, 1971, estimated entries were based upon personal knowledge, telephone conversations with Harold Lane at the National Science Foundation, and information contained in the American Institute of Physics publications, *Directory of Physics and Astronomy Faculties,* 1969–1970 and 1970–1971. We have not estimated entries for a few very small institutions; their impact on the totals and correlations would be negligible.

B. SUMMATIONS

The responding institutions were separated into academic and nonacademic. The latter includes government laboratories, independent observatories and research institutions, and industrial concerns. Appendix I. A is a sample questionnaire containing the summations of the responses and estimates. The left-hand figure in each column is the sum over academic institutions; the right-hand figure is the nonacademic institution sum. Where only one entry appears, it applies to the former group. The individual sums are discussed below.

C. MANPOWER

The total number of reported full-time equivalent (FTE) PhD astronomers in the United States was 1311 in 1969–1970 and 1377 in 1970–1971, an overall increase of about 5%. Academic institutions employed 61% in 1969–1970 and 62% in 1970–1971. The distribution of astronomers among various employment categories is given in Table 9.1 and the distribution according to number of FTE astronomers per institution is given in Figure 9.1.

TABLE 9.1 Astronomy PhD Employment Profile

Employer	1969–1970[a]	1970–1971[a]
Universities	61%	62%
Independent observatories	6%	6%
Government	26%	25%
Industry	3%	3%
Others	5%	5%

[a] Columns do not add to 100% due to rounding off.

The number of PhD astronomers engaged in graduate instruction increased by almost 5% in 1970–1971. FTE undergraduate instruction increased by the same amount. The number of FTE PhD astronomers engaged in research other than that connected with graduate theses increased by 9% in academic institutions and by 2% in nonacademic institutions.

Research comprises about 98% of the activities of PhD astronomers at nonacademic institutions. At academic institutions, 35% of FTE effort is spent in graduate student instruction and 22% in undergraduate instruction, while the remaining 43% is spent in basic research. The low-percentage effort spent in undergraduate instruction is probably a reflection of the large number of mathematics and physics courses taught by nonastronomers that must be taken as prerequisites to advanced undergraduate astronomy courses.

The reported astronomy PhD manpower can be compared with the predictions made in the Whitford report (*Ground Based Astronomy: A Ten-Year Program*, National Academy of Sciences–National Research Council, Washington, D.C., 1964) in their Table 2 and Figure 19. For 1969–1970, the prediction ranged between 953 and 914 depending on the assumed growth rate. The survey showed at least 1311 FTE astronomers for that same year, or an increase over the prediction of 38% to 44%. For 1970–1971, the predicted values were from 1055 to 989, but the survey tabulated 1377–increases of 30% and 39%, respectively. These increases cannot be solely explained by an influx of new PhD's. (see Table 9.2). It may be that the numbers used as a foundation for the Whitford extrapolation were underestimated, or, more probably, a considerable number of persons with PhD's in fields other than astronomy have been attracted into astronomy-related research and included in the survey.

The production of PhD astronomers has also increased at a faster rate than predicted in 1963. The data are shown in Table 9.2. The total increase over the predictions can be assumed correct, but incomplete or

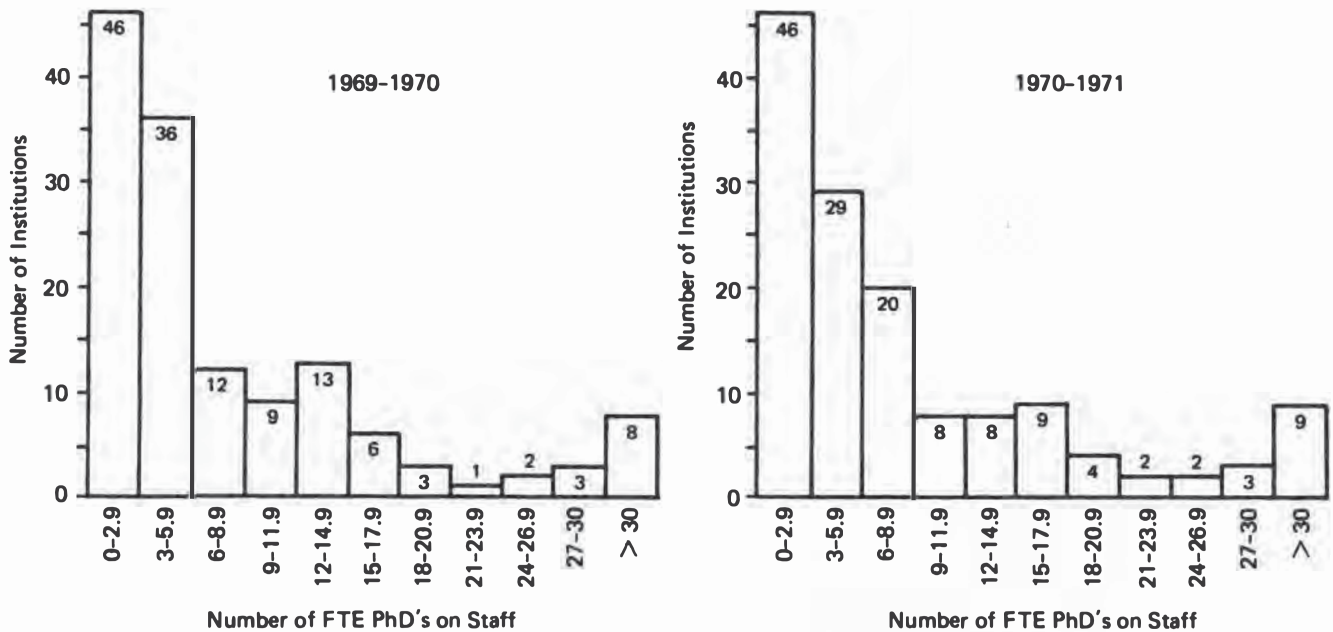

FIGURE 9.1 Distribution of FTE PhD astronomers at academic and nonacademic institutions.

TABLE 9.2 Astronomy PhD Production, 1966-1969

Year	Number Predicted	Number Reported	Reported Rate of Increase	Reported / Predicted
1966–1967	59 to 56	81	–	1.37 to 1.45
1967–1968	71 to 64	112	38%	1.58 to 1.75
1968–1969	83 to 72	116	4%	1.40 to 1.61
1969–1970	98 to 81	125	8%	1.28 to 1.55

faulty responses may have contributed to the jump between 1966 and 1967.

D. FUNDING

Total funding for astronomy over the three fiscal years 1968–1969, 1969–1970, and 1970–1971 was obtained by summing the entries in items I, J, and K of the Questionnaire (Appendix I. A). These totals are given below in Table 9.3.

The distribution of funds between academic and nonacademic institutions is shown in Table 9.4. Since the figures in Table 9.4 represent the total funds available to astronomy, including large one-time grants (e.g., construction of telescopes and development grants), it may be useful to exclude these and make comparisons based on funds earmarked for ongoing research and education only. The comparison from this survey is shown in Table 9.5. Details of federal funding for astronomy can be found in Section III.

The totals for single, one-time, large items (costing in excess of $100,000) for fiscal years 1969, 1970, and 1971 are presented in Table 9.6, although the breakdown of funds from a long project into individual fiscal years depends on the accounting procedure used, and a variation between successive years may be more apparent than real. The breakdown of Tables 9.5 and 9.6 by funding agency is included in Appendixes I. B and I. C.

The federal versus nonfederal funding for both continuing projects and single large items can be totaled as shown in Table 9.7.

The declining share of the federal government's fraction of the support for astronomy results partly from a small decrease in federal support and partly from increased university contributions to astronomy departments to meet inflation and increased undergraduate enrollments.

The problem of including overhead costs in the above figures is difficult and no accurate measure of such costs is available, although a crude

TABLE 9.3 Total Funds Allocated for Astronomy (in $Thousands)

Year	Amount	Percentage Change
1968–1969	$116,700	–
1969–1970	$118,100	+1%
1970–1971	$125,900	+7%

TABLE 9.4 Academic versus Nonacademic Total Funding (in $Thousands)

Year	Academic	Percentage Change	Nonacademic	Percentage Change	Academic / Nonacademic
1968–1969	$56,000	–	$60,700	–	0.92
1969–1970	$57,600	+3%	$60,500	0%	0.95
1970–1971	$61,400	+7%	$64,600	+7%	0.95

TABLE 9.5 Funds Expended for Ongoing Research and Education (in $Thousands)

Year	Academic	Percentage Change	Nonacademic	Percentage Change	Academic / Nonacademic
1968–1969	$48,000	–	$51,300	–	0.94
1969–1970	$52,000	+8%	$53,400	+4%	0.97
1970–1971	$54,000	+4%	$53,300	0%	1.01

TABLE 9.6 Funds Expended for Single Large Items (in $Thousands)

Year	Academic	Percentage Change	Nonacademic	Percentage Change	Academic / Nonacademic
1968–1969	7,237	–	9,368	–	0.77
1969–1970	4,848	–33%	6,989	–25%	0.69
1970–1971	6,598	+36%	11,212	+60%	0.59

TABLE 9.7 Federal versus Nonfederal Funding (in $Thousands and Percentages)

	1968–1969	1969–1970	1970–1971
Academic			
Federal	$ 39,937 (71%)	$ 40,777 (71%)	$ 41,817 (68%)
Nonfederal	15,909 (29%)	16,599 (29%)	18,757 (31%)
Unknown	120	220	810 (1%)
TOTAL	$ 55,966	$ 57,596	$ 61,384
Nonacademic			
Federal	$ 59,372 (98%)	$ 58,888 (98%)	$ 63,185 (98%)
Nonfederal	933 (1%)	1,390 (2%)	1,218 (2%)
Unknown	441 (1%)	190	150
TOTAL	$ 60,746	$ 60,468	$ 64,553
Academic and Nonacademic			
Federal	$ 99,309 (85%)	$ 99,665 (84%)	$105,002 (83%)
Nonfederal	16,842 (14%)	17,989 (15%)	19,975 (16%)
Unknown	561	410	960 (1%)
TOTAL	$116,712	$118,064	$125,937

estimate can be made. Since federal contracts normally include overhead charges, the correction to amounts reported above from the federal sources is believed to be fairly small. However, the nonfederal funding of academic astronomy is mostly from astronomy department budgets and probably includes almost no addition for overhead. A realistic overhead charge for these budgets appears to be about 75% of salaries and wages; most, but not all, of nonfederal funding for academic astronomy is estimated to be used for salaries and wages. An overhead estimate of 50% of nonfederal academic astronomy is shown in Table 9.8. These amounts are not included in any other tables in this report.

Figure 9.2 shows the number of institutions with total astronomy budgets in the ranges of less than $99,000, $100,000–499,000,

TABLE 9.8 Estimate of Additional Support to Academic Astronomy Represented by Overhead (in $Thousands)

1968–1969	1969–1970	1970–1971
$7955	$8300	$9379

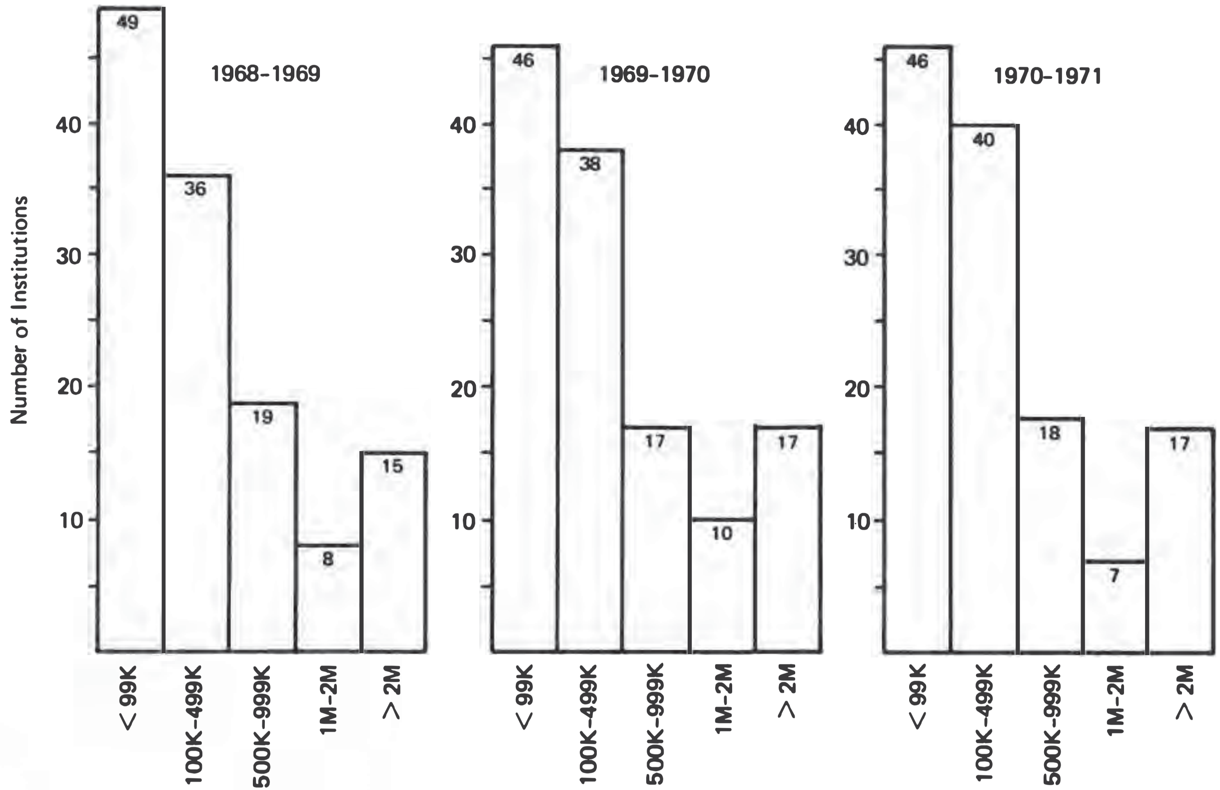

FIGURE 9.2 Distribution of funds for ongoing research and teaching.

\$500,000–1,000,000, \$1,000,000–2,000,000, and greater than \$2,000,000.

The cost to support the work of each scientist (defined here as the total budget of an institution divided by the FTE effort of PhD scientists employed at that institution) plotted against the total budget of the institution for ongoing research and teaching is shown in Figure 9.3. Individual points are not shown, but the crosshatching delineates the area containing the points. As expected, low-budget institutions (usually devoted mostly to undergraduate instruction) have a low cost per scientist, while institutions with large budgets (and frequently with a large nonscientist supporting staff and expensive equipment requirements) show a much higher cost per scientist. The sum of budgets divided by the sum of the FTE PhD scientists yields the value of \$85,000 per scientist for 1969–1970, decreasing to \$77,000 per scientist in 1970–1971. These figures and those that follow are based only on positive, unambiguous responses to the questionnaire—no estimated values have been included. If we exclude the NASA portion of an institution's ongoing research and teaching budget, we find \$44,000 per scientist in 1969–1970 and \$45,000 per scientist in 1970–1971.

None of the above figures include technical and clerical employees on the non-PhD level. As can be seen from Appendix I. A, these persons represent a very large portion of the total work force in astronomy. We have derived the average cost per employee for two groups as shown in Table 9.9.

The comparison in Table 9.9 is made by counting graduate students as employees in academic institutions. If we exclude all except those FTE's engaged in undergraduate instruction we find for academic institutions a cost per employee that was \$25,000 in 1969–1970 and dropped to \$21,000 in 1970–1971 (a 16% decrease with academic to nonacademic ratios of 1.0 and 0.84 for these two years).

E. EDUCATION

Undergraduate instruction in astronomy increased 13% in the number of students taught during the period covered by the questionnaire—from 45,800 students in 1969–1970 to 51,600 in 1970–1971. FTE PhD's engaged in undergraduate instruction increased by 5% from 178 in 1969–1970 to 187 in 1970–1971, while reported FTE graduate teaching assistants (T.A.'s) went from 112 to 130 during the same period (an increase of 16%). Taking only reported, nonestimated data from the questionnaires, we may form the undergraduate student/FTE teacher ratios shown in Table 9.10. Graduate institutions had at least one gradu-

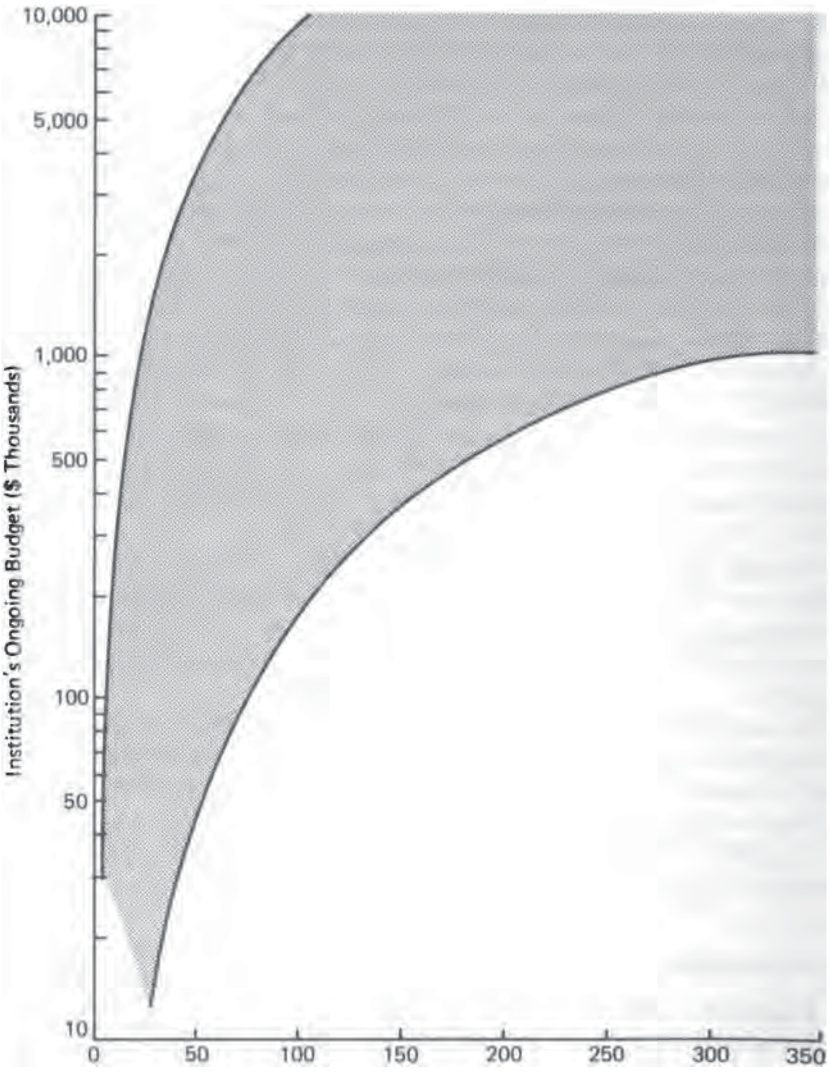

FIGURE 9.3 Cost per PhD versus total budget for ongoing research and teaching, 1970–1971.

TABLE 9.9 Cost per Employee for All Employees

Year	Academic Institutions	Change	Nonacademic Institutions	Change	Academic / Nonacademic
1969–1970	$17,000	–	$25,000	–	0.68
1970–1971	$14,000	−18%	$25,000	0	0.56

ate student in residence during the report period, while undergraduate institutions had no graduate students.

Ninety-four institutions indicated that they had taught undergraduate students during the two-year report period. Figure 9.4 shows how these students were distributed among the institutions. Thirty-six institutions reported awarding at least one baccalaureate degree in astronomy during 1969–1970.

Graduate students were reported by 75 institutions in 1969–1970 and by 78 the next year. The total number of graduate students in astronomy increased from 1125 to 1186 during the report period and were distributed among their home institutions as shown in Figure 9.5. This 6% increase in the number of graduate students may be compared with a nearly 5% rise in FTE PhD effort devoted to graduate education (from 297 in 1969–1970 to 311 in 1970–1971). Using non-estimated data only, we find a graduate student-to-FTE faculty ratio of 4.02 in 1969–1970 and 4.12 in 1970–1971. Foreign nationals accounted for 119 and 132 of the total number of graduate students, respectively.

A follow-up questionnaire was sent to graduate institutions in late May 1971, asking for their expected graduate enrollment in 1971–1972. Forty-one responses were received; of these 12 indicated an enrollment increase, 22 a decrease, and 7 were unchanged. In terms of total numbers, the 41 institutions reported 890 graduate students for 1970–1971 and 844 for 1971–1972—a decrease of 5%.

An analysis of graduate degrees awarded is given in Table 9.11. Additional discussion of the numbers in this table is given in the following section on Manpower.

A crude estimate of the production cost of each PhD granted can be made. We define the portion of the institution's budget that goes into graduate education as the product of the total ongoing research and teaching total budget times the fraction of FTE PhD staff devoting efforts to graduate education. The fractional budgets are then summed and divided by the total number of PhD's produced that year. The results are given in Table 9.12. The additional breakdown into budget source was accomplished in an analogous fashion using the fraction of the total budget originating from the source of interest.

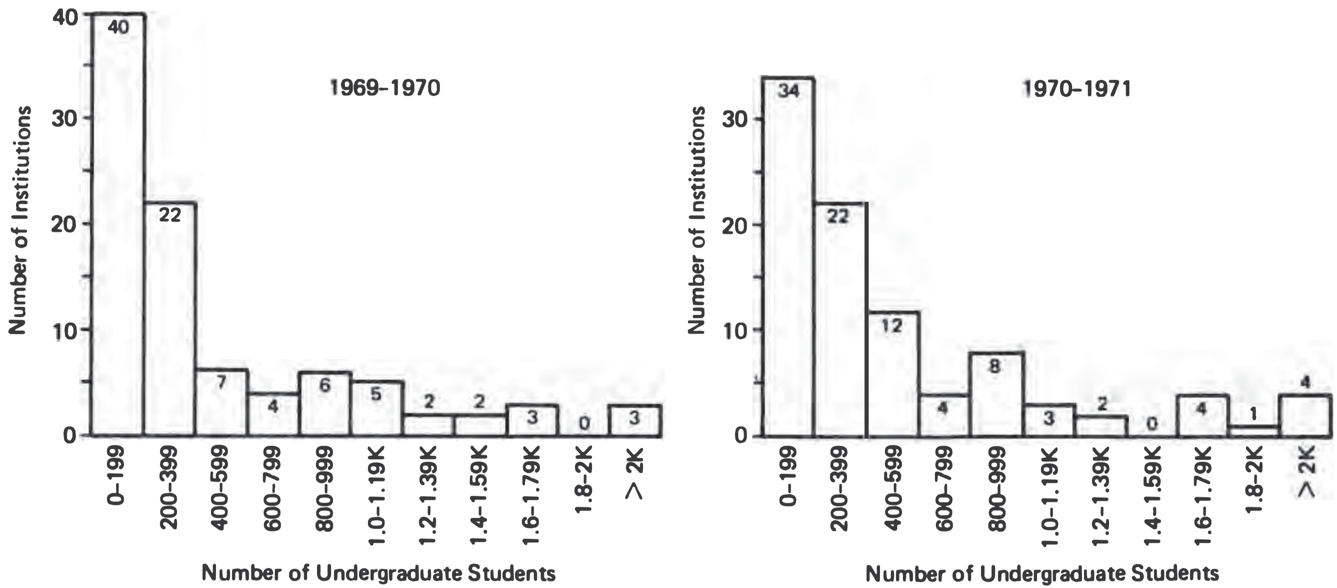

FIGURE 9.4 Distribution of undergraduate enrollment per year in all astronomy courses.

TABLE 9.10 Student[a]/FTE Teacher Ratios for Undergraduates (Academic Institutions Only)

Type of Institution	Ratio 1969–1970	Ratio 1970–1971	Change
All			
Excluding T.A.'s	288	300	+4%
Including T.A.'s	171	172	0%
Graduate			
Excluding T.A.'s	296	322	+9%
Including T.A.'s	159	164	+3%
Undergraduate			
No T.A.'s	233	236	+1%

[a] The numerator is a head count of all enrollments in all astronomy courses given during the academic year.

TABLE 9.11 Graduate Astronomy Degrees Awarded (Estimates Not Included)

Year	Number of Institutions	PhD	Change	Number of Institutions	M.S.	Change
1966–1967	31	70	–	25	72	–
1967–1968	34	102	+46%	30	74	+ 3%
1968–1969	37	106	+ 4%	36	119	+61%
1969–1970	37	112	+ 6%	38	108	– 9%

TABLE 9.12 PhD Production Costs, 1969-1970 (Estimates Not Included)

Budget Source	Number of Institutions	Number of PhD's Granted	PhD Cost
Total budget	33	103	$109,000[a]
NASA portion	26	91	$ 48,000
NSF portion	31	101	$ 20,000
Other federal portion	21	67	$ 25,000
Nonfederal portion	29	96	$ 33,000

[a] The average costs attributed to portions of budgets derived from different sources do not add to the overall average, since different groups of institutions were used in computing the separate averages.

TABLE 9.14 FTE Effort Expressed in Percentages, To Show What Percent Effort in Each Type of Institution Is Spent in Various Types of Research

	Undergraduate Program Only	Undergraduate and Graduate Program	Graduate Program Only	Primarily Research, Univ.-Affiliated	Research, Industry-Affiliated	Research, Fed. Govt.-Affiliated	Research, Independent
Ground-based optical observations of objects outside the solar system	39	24	2	5	0.	7	41
Ground-based radio observations of objects outside the solar system	0.	10	10	8	10	8	14
Ground-based optical and radio observations of the solar system, but excluding the sun	4	4	4	19	4	6	6
Ground-based optical observations of the sun	7	3	0	2	21	7	8
Ground-based radio observations of the sun	0.	2	<1	0.	5	2	0.
Space-based observations of all kinds, of all objects other than the sun	0.	9	4	8	26	13	0.
Space-based observations of the sun	0.	3	0	2	23	7	0.
Laboratory astrophysics	0.	6	2	5	2	12	10
General-purpose instrument development	6	6	54	14	4	8	10
Astrometry and celestial mechanics	0.	5	1	19	0	7	0.
Theoretical astrophysics	44	26	15	15	5	17	11
Not classified elsewhere	0.	2	8	5	<1	7	0.
TOTAL	100	100	100	[a]	100	[a]	100

[a] Percentages in the column do not add to 100% because of rounding off.

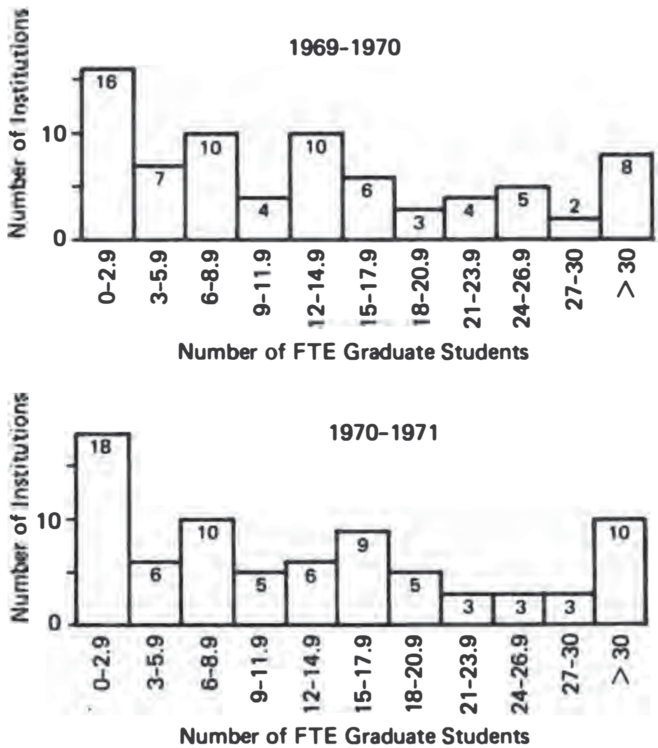

FIGURE 9.5 **Distribution of FTE graduate students.**

F. RESEARCH

The survey questionnaire asked each respondent for estimates of the percentage of FTE time devoted to graduate education and research in 1969–1970, in each of 12 areas of astronomy. The percentages were converted to FTE effort in each area by each institution, and a total was derived of the time spent in each area of research effort. For both the academic and nonacademic institutions, the sums could be represented as a percentage of the total research done at that type of institution.

The FTE totals in each research area for each type of institution (for purposes of this classification, Kitt Peak National Observatory and Cerro Tololo Inter-American Observatory were considered as government-affiliated) are shown in Table 9.13. This information is also shown (Tables 9.14 and 9.15) with the effort expressed as a percentage of the total research carried on by that classification of institution and as a percentage of the total research effort devoted to that branch of astronomy. The results show, for instance, that universities with both undergraduate and graduate programs dominated the research done on ground-based optical observations of objects beyond the solar system and ground-based radio observations of the sun, while government-affiliated organizations did most of the research on ground-based optical observations of the sun and space-based solar observations.

APPENDIX I.A

QUESTIONNAIRE SUMMATIONS

Please return completed questionnaire to:
Mr. Bruce Gregory
Division of Physical Sciences
National Academy of Sciences
2101 Constitution Avenue
Washington, D.C. 20418

Privileged data for use only by the
Astronomy Survey Committee of the National Academy of Sciences

Name of Institution ____________________

Department and/or Other Organizational Subdivisions ____________________

Name and Title of Individual Responding ____________________

Full-Time Equivalent (FTE) Personnel in Astronomy (For example, one individual dividing his effort equally between two categories should be counted as 0.5 in each category.)	FTE (on 12-month basis), July '69–June '70		Estimated FTE for July '70–June '71 on basis of best current information for your institution	
A. Staff and/or Faculty (PhD or Equivalent) Include postdoctoral fellows. Include visitors which you pay and staff which you pay even if visiting elsewhere.	Academic	Non-academic	Academic	Non-academic
(a) FTE engaged in graduate instruction and supervision of thesis research	288	9	302	9
(b) FTE engaged in research other than that covered in (a)	334	502	365	514
(c) FTE engaged in undergraduate instruction	177	1	186	1
(d) FTE total (sum of a, b, and c)	799	512	853	524

	FTE (on 12-month basis), July '69–June '70		Estimated FTE for July '70–June '71 on basis of best current information for your institution	
B. Graduate Students (U.S. and Foreign Nationals)	Academic	Non-academic	Academic	Non-academic
(a) FTE engaged in graduate study, including thesis research	931	5	968	5
(b) FTE engaged in undergraduate instruction	112	0	130	0
(c) FTE engaged in other activities (specify ________)	77	0	83	0
(d) FTE total (sum of a, b and c)	1,120	5	1,181	5
C. Graduate Students Who Are Foreign Nationals (FTE total) included in B(d)	116	3	130	2
D. All Other Employees (paid by you from funds earmarked for astronomy, not included in A, B, or C above, FTE)				
(a) FTE technical (e.g. engineers, programmers, photographers, electronics techs., night assistants)	822	1,055	834	1,017
(b) FTE administrative, maintenance, clerical	386	591	396	594
(c) FTE total (sum of a and b)	1,208	1,646	1,230	1,611
E. Total Number of Student Enrollments in All *Undergraduate Courses* Given in All Quarters/Semesters (cross out one) during the Period July 1969–June 1970				
(a) Survey, descriptive or introductory courses	42,293	0	47,708	0
(b) Other courses	3,528	0	3,917	0
(c) Total (sum of a and b)	45,821	0	51,625	0

F. Number of Degrees Granted with Major in Astronomy or Astrophysics	Academic PhD	Academic Masters	Academic Baccalaureate
July '66–June '67	81	80	99
July '67–June '68	112	89	117
July '68–June '69	116	134	139
July '69–June '70	125	123	150

G. Fields of Interest Represented by Your Faculty and Staff. Please give very approximately the percentage of FTE time (for graduate education and research) indicated in A(a) and A(b), for '69–'70, which were devoted to the following science areas:

	Academic	Non-academic
(a) Ground-based optical observations of objects outside the solar system	20.9	6.6

APPENDIX I.A (*Continued*)

	Academic	Non-academic
(b) Ground-based radio observations of objects outside the solar system	9.9	8.2
(c) Ground-based optical and radio observations of the solar system, but excluding the sun	5.5	8.0
(d) Ground-based optical observations of the sun	2.7	7.0
(e) Ground-based radio observations of the sun	2.0	1.5
(f) Space-based observations of all kinds, of all objects other than the sun	8.1	12.6
(g) Space-based observations of the sun	2.6	6.8
(h) Laboratory astrophysics	5.9	10.0
(i) General-purpose instrument development	10.2	9.3
(j) Astrometry and celestial mechanics	4.4	9.2
(k) Theoretical astrophysics	24.9	15.0
(l) Not classified elsewhere (specify ______)	2.9	5.8
Total	100%	

H. Please provide comments on any *personnel* problems that seem to be particularly pressing at your institution. Possible topics might be (1) the success (or lack thereof) that your recent PhD and Master's recipients are having in obtaining jobs that make appropriate use of their training and interests; and (2) limitations that you are (or are not) placing on the number of new graduate students that you accept.

Funds Earmarked for Astronomy

Approximate Expenditures *Rounded to the Nearest $1000*

I. For Ongoing Research and Education, excluding major one-time projects (see J below). Include all expenses such as salaries, equipment, services, and overhead.	July '68–June '69		July '69–June '70		July '70–June '71 Estimated on the Basis of Best Current Information for Your Institution	
	In Thousands of Dollars					
Source of Funds	Academic	Non-academic	Academic	Non-academic	Academic	Non-academic
(a) NASA	$23,770	25,820	$24,564	26,801	$26,328	22,092
(b) NSF	$ 6,517	14,489	$ 7,766	15,869	$ 8,220	20,766
(c) Other Federal Agency (Specify ______)	$ 5,725	10,136	$ 6,273	9,419	$ 5,338	9,265
(d) Nonfederal, e.g., derived from state or city appropriations, allocated from private donations, derived from university endowments or tuition fees collected by it	$12,005	896	$13,433	1,353	$14,114	1,196
(e) Total	$48,017	51,341	$52,036	53,442	$54,000	53,319

J. For Single, One-Time, Large Items Such as Major Capital Facilities or Development Grants. Include here only items *totaling more than $100,000*. (Smaller items should be included in I. Do not include here any item included in I.) Round to nearest $10,000. Do not include any item if completed prior to July 1968, or if not started before June 1971.

	Item 1	Item 2	Item 3
(a) Item Description:			
(b) Source of Funds, e.g., NASA, Nonfederal, etc. If multiple sources of support, give proportions, such as 50% NASA, 50% nonfederal, etc. (Given in $1000)			

	In Thousands of Dollars			
	Academic	Non-academic		
(c) Expenditure prior to July 1968	$14,287	$11,266	$	$
(d) Expenditure July '68–June '69	$ 7,237	$ 9,368	$	$
(e) Expenditure July '69–June '70	$ 4,848	$ 6,989	$	$
(f) Est. Expenditure July '70–June '71	$ 6,598	$11,212	$	$
(g) Est. Expenditure July '71 to completion	$14,450	$ 9,400	$	$
(h) Est. Total (Sum of c through g. Do not include here if less than $100,000.)	$47,420	$48,235	$	$

K. For Major *Contributed* Services during the Period Indicated, i.e., for which you *do not pay* either directly or through overhead. Include only contributions from *nonastronomical* organizations such as a computer center. Do *not* include services from astronomical organizations such as NRAO, KPNO, NCAR, or NASA facilities. We will solicit this information from those organizations.

APPENDIX I.A (*Continued*)

Name of Contributing Organization	Estimated Value of the Contribution to the Nearest $1000 (if you had to pay for it)			
	July '69–June '70		July '70–June '71	
	Academic	Non-academic	Academic	Non-academic
	In Thousands of Dollars			
Item (a) (Specify)	$712	37.	$786	22.
Item (b) (Specify)	$		$	

L. Please provide comments on any funding problems that are particularly pressing at your institution, e.g., observing equipment, buildings, staff, student support, general operations, computer costs.

APPENDIX I.B Funds Expended for Ongoing Research and Education, Broken Down by Source (in $Thousands)

	Academic			Nonacademic		
Source	1968/69	1969/70	1970/71	1968/69	1969/70	1970/71
NASA	23,770	24,564	26,328	25,820	26,801	22,092
NSF	6,517	7,766	8,220	14,489	15,869	20,766
Navy, ONR	324	344	190	4,311	4,119	4,053
Air Force, AFCRL, AFOSR	1,007	943	898	1,839	1,683	1,739
Army	38	0	0	0	0	0
ARPA	91	50	26	0	0	0
DOD	2,707	3,061	2,919	1,090	800	582
NDEA	17	13	16	0	0	0
AEC	24	15	20	100	100	100
JPL	0	15	0	0	0	0
NBS	380	360	380	850	850	850
S.I.	0	0	0	1,851	1,817	1,891
AURA	0	83	30	0	0	0
Misc. federal	87	93	130	95	50	50
Nonfederal	12,005	13,433	14,114	896	1,353	1,196
Unknown	1,050	1,296	759	0	0	0
TOTAL	48,017	52,036	54,000	51,341	53,442	53,319

APPENDIX I.C Funds Expended for Single Large Items, Broken Down by Source (in $Thousands)

	Academic			Nonacademic		
Source	1968/69	1969/70	1970/71	1968/69	1969/70	1970/71
NASA	1,922	764	308	2,957	3,532	3,610
NSF	1,928	1,335	1,548	5,070	1,750	7,160
Navy	0	0	0	0	1,477	282
Air Force	0	0	0	900	40	10
Nonfederal	3,317	2,579	3,982	0	0	0
Unknown	70	170	760	441	190	150
TOTAL	7,237	4,848	6,598	9,368	6,989	11,212

II. MANPOWER AND EMPLOYMENT IN AMERICAN ASTRONOMY

In 1964, the Whitford Panel concluded its astronomy manpower study with the following words:

> The surge of students into the graduate departments of astronomy has followed a steady upward course for at least six years, and as yet shows no signs of rounding off . . . a sharp increase in the number of astronomers in the country is inevitable . . . not less than a doubling . . . in the next decade. There will surely be more than enough astronomers waiting to use the [recommended] new instruments.

In 1972, a statistical survey of astronomy departments and observatories throughout the country, and a survey of those who have earned PhD's in those departments in the past four years, revealed that, with one slight change, the above statement is as true today as it was in 1964. The change is that there are finally signs of "rounding off" in the astronomy graduate student population. Despite this, and the predicted slowdown in astronomy PhD production that it anticipates, there is no sign of slackening in the 15% annual average increase in the number of PhD's added to the astronomy work force. The explanation is the source of the PhD's: Today the major fraction of new researchers comes not from astronomy departments but from departments of physics. The year 1972 is something of a watershed in astronomy, as the total number of PhD physics researchers working in the field exceeds for the first time the number with PhD's earned in astronomy, highlighting the continued transformation of the subject from astronomy to astrophysics. Numerical discrepancies between Sections I and II of this chapter arise, in part, from this effect.

The Statistical Panel attempted to respond to a number of questions raised by the Astronomy Survey Committee. Some of the manpower questions raised are included here as introduction to the statistics gathered in an attempt to answer them.

How has the number of astronomers been changing with time, and how does it compare with changes in other fields of the physical sciences?

How many degree-granting astronomy departments are there in the United States, and how has this number been changing with time?

How many PhD's are being produced in astronomy? What subfields of astronomy are of interest to the new PhD's? Are they finding research jobs in their fields of interest?

What is the rate of unemployment in astronomy? How does it compare with the reportedly severe unemployment problem in physics?

How do new PhD's find jobs? Can improvements be made in this area? What is the nature of the first professional position—teaching or research, on a university campus or in industry?

How does the current production of astronomers compare with the expected future demand? How does growth in financial support of astronomical research compare with growth in manpower?

How are astronomers distributed among the subfields of astronomy—theoreticians, radio observers, optical observers, space astronomers, etc.?

The most perplexing problem in analyzing manpower in astronomy is the lack of any precise, unequivocal, and universally acceptable definition of just what constitutes an astronomer. Even if a good definition existed, who is to apply it—the scientist himself or his employer or the educational institution that qualifies him professionally? Is the definition fixed once and for all time, or can a researcher change his interests and thus his label?

The survey of astronomical manpower polled the scientists, their employers, and their educational institutions. It also included those who regularly study scientific manpower, namely, the National Science Foundation National Register of Scientific and Technical Personnel, the U.S. Office of Education of the Department of Health, Education, and Welfare, the American Astronomical Society, and the International Astronomical Union.

This collection of data reveals a picture of astronomy manpower that is consistent in its gross overall structure, both the approximate absolute level and trends with time. When examined in detail (with a hoped-for 10–20 percent accuracy), the picture is less precise. The best estimates for the period 1960–1970 of the numbers of PhD's employed in U.S.

astronomy and the *total* number of professional astronomers (B.A.'s, M.A.'s, and PhD's) are shown in Figure 9.6. These numbers are full-time equivalents (FTE), counting two half-time astronomers as one astronomer. Both the number of PhD's and the total number of astronomers have approximately tripled in the past decade and have been averaging a roughly 15% annual growth rate for the past five or six years.

This growth is part of a pattern that began in the 1950's. It was first pointed out by Otto Struve who estimated the worldwide number of active astronomers had doubled during the 1951–1960 decade, following several decades of approximate constancy, apart from the wartime periods (Figure 9.7).

As will be seen below, the increased rate of growth in the number of astronomers was not only the result of increased astronomy PhD production but also a consequence of PhD's from other fields working in astronomy and calling themselves astronomers. No doubt the reasons

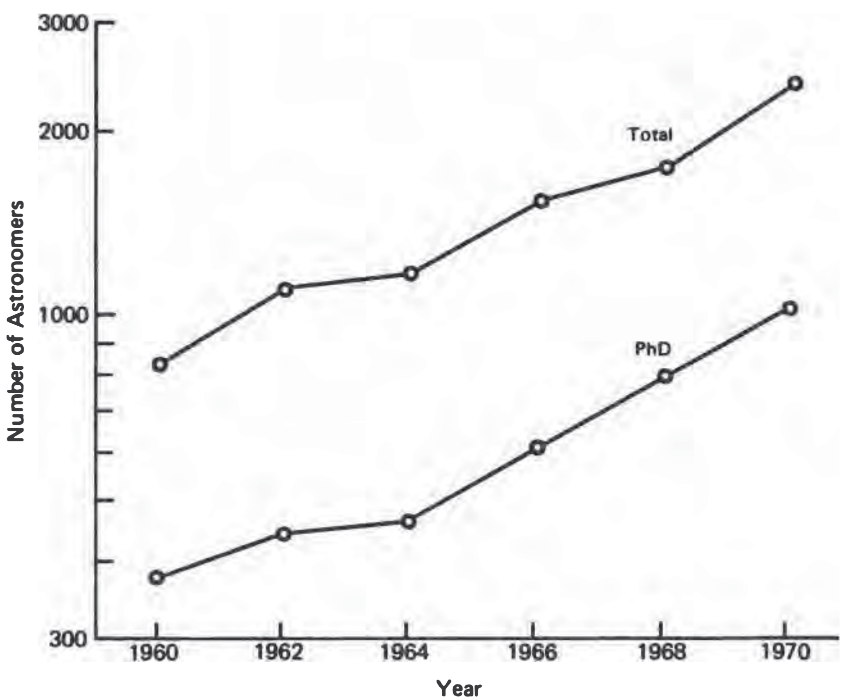

FIGURE 9.6 Number of persons (total and PhD's) employed in astronomy in the United States, 1960–1970. [Source: National Register. The data have been corrected for completeness and consistency, as described in footnotes to Table 9.18.]

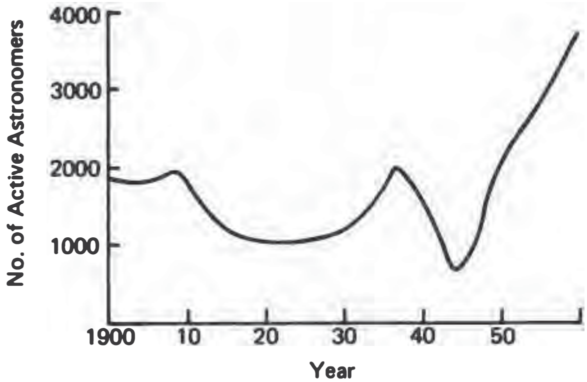

FIGURE 9.7 Number of active astronomers worldwide, 1900–1960. [Source: O. Struve and V. Zebergs, *Astronomy of the 20th Century* (Macmillan, New York, 1962).]

they came were many, but the opening of the new field of radio astronomy and anticipation of a vast new program in astrophysical research supported by the U.S. space program must have been major factors. Since some of these researchers established graduate study programs and began to produce PhD's, the strong growth rate in "astronomers" in the late 1950's is mirrored about seven years later in an increased production of astronomy PhD's.

In Figure 9.8 we see that the rate of astronomy PhD production in the United States from 1920 to 1960 was reasonably constant at a 4% annual rate and significantly below the 7% rate of physics and other related sciences. Beginning in about 1960, with the advent of the space era and the start of a period of unparalleled discoveries like quasars, pulsars, and the cosmic fireball, astronomy's annual growth rate jumped to 15 to 20%. Thus for the past ten years astronomy has been expanding exceptionally fast, tripling its population in a decade. But over the broader time scale of the last 50 years, the recent surge only brings the field into approximate equilibrium with the average 7% growth in other sciences, almost restoring the 1920's ratio of one astronomy PhD being awarded for every ten physics PhD's. This change in manpower since the early 1960's is seen most dramatically by contrasting our Figure 9.8 with Figure 17 of the Whitford report.

Figure 9.9 shows that while the number of departments granting PhD's in astronomy remained nearly constant through the 1950's, it more than doubled during the 1960's.

Table 9.16 lists departments of astronomy and the number of PhD's they awarded each year.

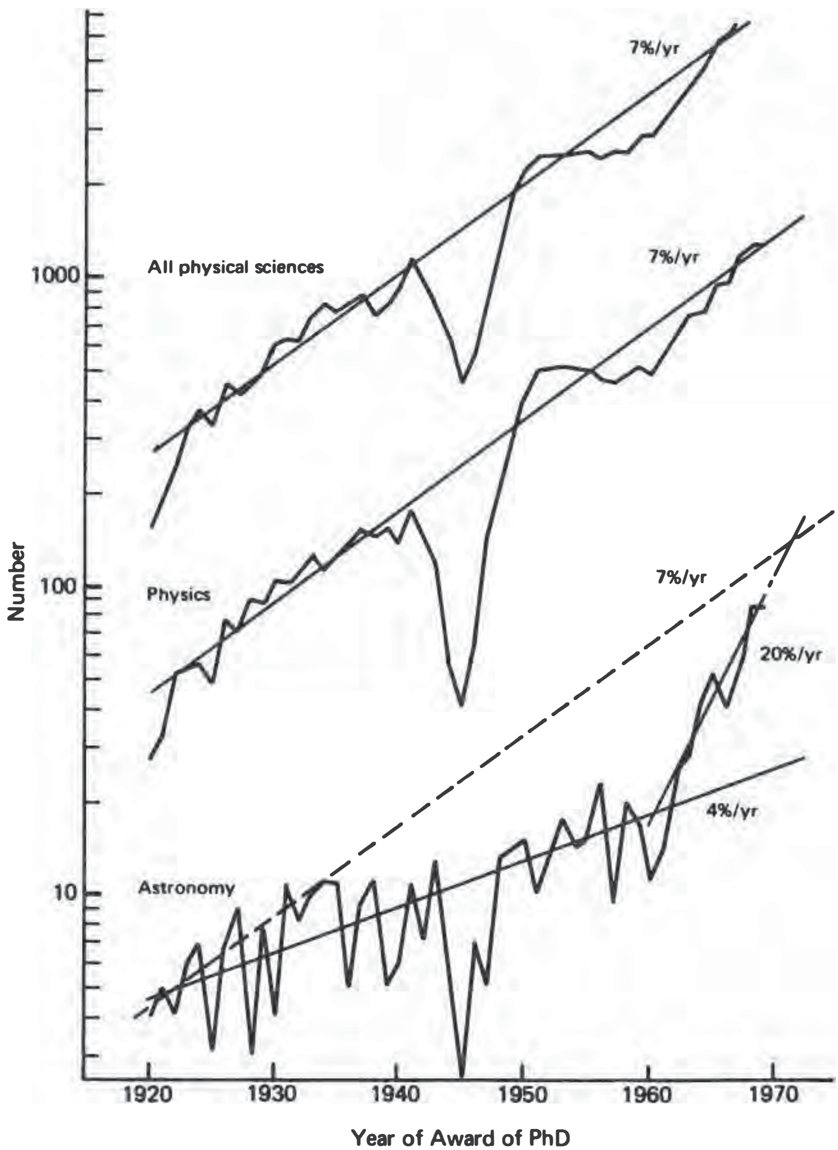

FIGURE 9.8 Number of PhD's awarded in the United States in astronomy, in physics, and in all physical sciences (including engineering), by a academic year, 1920–1969. [Sources: 1920–1958, Office of Scientific Personnel, National Research Council, *Doctorate Production in United States Universities, 1920–1962, with Baccalaureate Origins of Doctorates in Sciences, Arts, and Professions*, NAS-NRC Publ. 1142 (National Academy of Sciences–National Research Council, Washington, D.C., 1963); 1959–1969, *Earned Degrees Conferred*, U.S. Office of Education.]

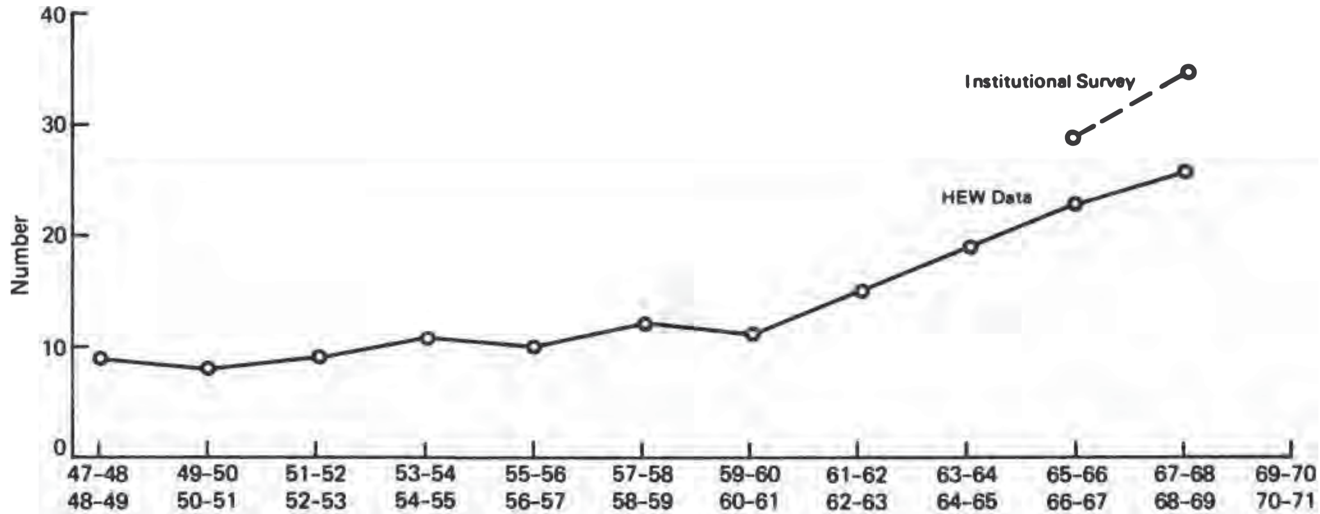

FIGURE 9.9 Number of institutions in the United States awarding at least one PhD in astronomy/astrophysics, by intervals of two academic years, 1948–1969. [Sources: Department of Health, Education, and Welfare and institutional survey.]

In order to estimate future PhD production, it is necessary to know the number of graduate students presently enrolled in U.S. astronomy departments.

The Whitford Panel significantly underestimated the number of PhD's who would be employed in astronomy in 1970 (the reason being the influx of physics PhD's) and slightly underestimated the 1970 production rate of PhD's awarded in astronomy, but it significantly overestimated the 1970 number of graduate students in astronomy departments.

One reason for overestimating the number of graduate students has been the unexpectedly rapid decline in federal support of students. For example, the National Aeronautics and Space Administration (NASA) fellowship and traineeship program was supporting 886 graduate students in all fields in 1964, and the Whitford Panel was advised that the number was expected to reach 4000 in 1970. On the contrary, in 1972, this program has been completely eliminated.

Figure 9.10 shows the total number of graduate students, and the number of first-year graduate students, in U.S. astronomy departments. A clear leveling trend is evident in first-year enrollments since about 1966–1967. Given the five- or six-year interval of graduate study, we anticipate soon a leveling in the annual number of PhD's awarded in astronomy. Increased numbers of astronomy departments with a leveling trend in the total number of graduate students suggests that the old and larger departments have cut back admissions. Students who otherwise would have wished to attend established research centers enroll in the smaller, newer departments. A planned reduction in the number of PhD's produced, if applied only at well-established institutions, may result only in the same number of PhD's with training by a different mix of institutions.

The anticipated slowdown in the number of PhD's awarded in astronomy does not guarantee reduction in the rate of PhD's entering astronomical research, since the number entering the profession with doctorates in physics is accelerating at present.

The Register data indicate that approximately 700 new PhD holders have entered astronomy during the last decade. According to HEW reports, approximately half of that number of PhD degrees was awarded by U.S. astronomy departments during the same period. Register data show that a massive immigration of foreign astronomers did not cause this discrepancy. A significant influx into astronomy must have arisen from a transfer from other degree fields.

The Register data support this conclusion. Whereas in the early 1960's less than a quarter of the PhD holders employed in astronomy held their doctorates in physics, by 1970 the portion had risen to nearly half. (See

TABLE 9.16 Number of PhD's Awarded in Astronomy or Astrophysics by Institution per Academic Year[a]

	1960 –61	1961 –62	1962 –63	1963 –64	1964 –65	1965 –66	1966 –67	1967 –68	1968 –69	1969 –70
U. of Arizona					2	3	2(2)	4(4)	4(5)	(5)
Boston U.								(0)	(0)	(2)
Brandeis U.							(1)	(1)	(1)	(1)
Brigham Young U.							(1)	(1)	(2)	(0)
Caltech	(?)	1	1	5	3	3	2(5)	4(4)	2(2)	(8)
U. of Calif., Berkeley	(?)	2	1	6	4	4	10(10)	5(6)	5(4)	(5)
U. of Calif., Los Angeles					2	2	2(2)	1(1)	5(1)	(2)
U. of Calif., Santa Cruz								(0)	1(1)	(0)
U. of So. Calif.								(3)	(0)	(3)
Case Western Reserve	1		2	1	3	2	2(2)	7(7)	3(3)	(1)
U. of Chicago	1	1	2	2	1	2	3(3)	5(5)	1(2)	(2)
U. of Cincinnati			1	1				(0)	1(0)	(0)
U. of Colorado	2				1		2(3)	5(6)	(3)	(5)
Columbia U.	(?)	1	1			1	1(1)	(1)	1(0)	(4)
Cornell U.						1	(1)	(2)	1(2)	(0)
U. of Florida								(1)	(1)	(5)
Georgetown U.	3	2	1	3	3	1	1(1)	1(0)	3(0)	(0)
Harvard U.	2	5	1	7	5	3	2(2)	13(13)	7(7)	(7)
U. of Illinois	(?)		2		3		1(1)	3(2)	3(3)	(2)
U. of Indiana	2		2	3	4	2	4(5)	5(5)	6(5)	(6)
U. of Iowa							(1)	(1)	(1)	(2)
U. of Maryland						1	(1)	4(4)	(1)	(5)
U. of Michigan	(?)	4	3	2	6	3	4(4)	4(4)	6(6)	(6)
U. of Minnesota								(0)	(1)	(1)
Northwestern U.						2	1(1)	1(1)	1(1)	(1)
Ohio State U.							(0)	3(3)	4(3)	(3)
U. of Oklahoma								(0)	(0)	(1)
U. of Penn.	1	1	3	3	3	2	(0)	5(0)	1(0)	(5)
Penn State U.							(0)	(1)	(2)	(0)
Princeton U.		1	2	2	3	3	5(4)	6(2)	9(3)	(3)
Princeton U. Dept. of Physics							(2)	(0)	(3)	(0)
Rensselaer Polytech. Inst.					1	1	(2)	1(2)	2(1)	(2)
Rice U.							(2)	(1)	(3)	(2)
Rochester U.							(0)	(0)	(2)	(2)
Stanford								(1)	(4)	(2)
U. of Texas							3(1)	2(2)	3(4)	(4)
U. of Virginia							1(1)	(2)	(2)	(4)
U. of Washington							(0)	1(2)	1(1)	(1)
U. of Wisconsin	(?)	3	3	4	3	1	4(4)	5(5)	3(3)	(3)
Yale U.	1	4	3	1	3	3	3(4)	3(6)	9(8)	(3)
TOTAL	13	25	28	40	50	40	53	86		

[a] The principal source for the data presented here is "Earned Degrees Conferred," compiled by HEW. The numbers in parentheses were obtained from the institutional questionnaire (Section I

Table 9.17) During the 1960's the percentage of doctorates working in astronomy who had PhD's in fields other than astronomy or physics remained about 7%. Over half of the PhD's employed in American astronomy hold doctorates in fields other than astronomy. A recent study by the Economic Concerns Committee of the American Physical Society

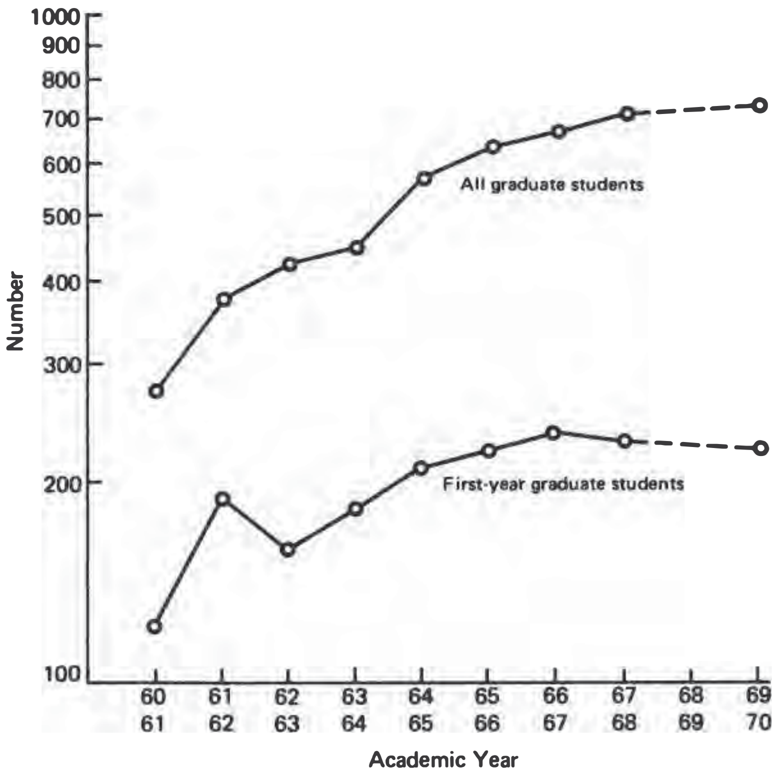

FIGURE 9.10 Number of graduate students in astronomy and first-year graduate students, by academic year, 1961–1970. [Source: HEW.]

of this chapter). Since HEW's information on degrees awarded depends on voluntary responses from graduate institutions, it is not always comprehensive, although it is nearly so. HEW uses only the categories of "Physics" and "Astronomy." Degrees in astronomy-related disciplines (e.g., "Physics-and-Astronomy") are listed in one or the other of them, in accord with the way that the individual graduate institutions report the fields. Besides this, it appears there may be serious omissions. For instance, in 1960–1961, degrees probably were awarded at Caltech, Berkeley, Illinois, Michigan, and Wisconsin, even though none were reported to HEW by those institutions. But even with these shortcomings, HEW's data on degrees awarded are the best available that span the decade.

TABLE 9.17 Percentage Distribution of U.S. Astronomers by Field of Their PhD[a]

Field of PhD	1966	1968	1970	1972 (est.)
Astronomy	67%	57%	48%	46%
Physics	26%	37%	45%	47%
Other	7%	6%	7%	7%

[a] Source: Unpublished data from the National Register of Scientific and Technical Personnel (NSF).

found that 40% of the current PhD physics candidates expressed an interest in research in astrophysics.

Of the physicists entering astrophysics, only about one third have just received their PhD; the *net* increase in PhD physicists working in astrophysics averages a 20% growth each year, as compared with a 5% growth for the *net* increase in PhD's with degrees in astronomy. Net increase is defined here as the number of PhD's awarded minus those who drop out of the subject, retire, or die; most who drop out appear to be a few years out of graduate school without a tenured position. If the growth in physics PhD's continues, but tapers to 7% per year, there will be roughly 1300 physics and 850 astronomy PhD's working in astrophysics in 1979–1980.

A detailed comparison is given in Table 9.18 of various estimates of astronomy manpower. As noted above, there are discrepancies at the 20% level, most of which can be explained by the methods used to collect the data.

A. EMPLOYMENT

Because of concern over a job shortage for well-prepared astronomers, a study was made, in the spring of 1971, for the Astronomy Survey Committee. Since persons most directly affected by a job shortage are usually at the beginning of their careers, recent recipients of PhD's in astronomy or astrophysics from U.S. institutions were surveyed.

Ideally, the survey should have included PhD recipients in physics; this was not attempted because of the size of the physics population. Findings for that group would virtually defy analysis. If physicists were not finding employment in astronomy, what precisely would that mean? If astronomy PhD's were failing to find jobs in their own field, the conclusions would be less vague, and the actions required, simpler.

Figure 9.11 shows findings from the survey. Virtually all of the persons in the sample had found employment, but with progressively more difficulty during the past four years. Those receiving more than one job offer decreased from two thirds of the 1967 graduates to roughly one third of the 1970 graduates. Since some might have received more job offers if they had not already accepted an early one, the data probably reflect a lower limit of the potential job market. Each year a higher percentage sent out larger numbers of applications, but an increasing fraction of those sending multiple letters received only one job offer. The percentage of graduates sending more than three letters rose from 24% in 1967 to 52% in 1970.

The way that the first job was found also changed, as is shown in Figure 9.12. This further demonstrates the tightening of the market.

Interestingly, comparison of numerous variables in this study against the relative ranking of "Effectiveness of Doctoral Programs" by the American Council of Education (ACE) shows that the only significant difference among the institutions in terms of employment of their graduates arose in the way by which they secured their first job. Figure 9.13 substantiates what might be expected intuitively–graduates from the highest-ranked graduate programs were greatly aided in their job securement by their faculty, while those from the lowest-ranked programs had to rely more on their own efforts.

According to data from the 1970 National Register, unemployment was not a major problem among astronomers as a whole. Of the nonstudents in the field, 1.5% were unemployed and seeking employment, a figure that is identical to the average for all scientists listed in the Register. For PhD's in astronomy, the percentage was 0.8, compared with the Register average of 0.9.

It should be emphasized, however, that the job market is constantly tightening. For the class of 1970 in astronomy, the number of jobs and number of applicants were about equal. The recent trend indicates that the situation in the next few years probably will not be improved.

A follow-up survey by the NSF in spring 1971 showed that unemployment among all scientists had risen since 1970 from 1.5% to 2.6%, and for PhD's from 0.9% to 1.4%; however, for physicists it had risen to 3.9%. Specifically, that survey found that the age group under 30 had the highest unemployment, and that more than half of the nation's unemployed scientists were in either chemistry or physics.

The findings in this study for recent PhD's in astronomy indicate that even the ones under age 30 had a comparatively small problem with unemployment. Less than 1% of them were unemployed, and an additional group of under 1% were underemployed in the sense that they could only obtain part-time work; however, half of the 2% with employment prob-

TABLE 9.18 Number of Persons in Astronomy by Various Definitions[a]

Year	National Register Numbers: Total Corrected	National Register Numbers: PhD Corrected	Number by Field of PhD: PhD in Astronomy	Number by Field of PhD: PhD in Physics	Number by Field of PhD: PhD in Other	HEW Numbers: Number of Graduate Students in Astronomy Departments	HEW Numbers: Annual No. of U.S. PhD's	HEW Numbers: Cumulative PhD's since 1920	Institutional Survey Numbers: PhD's or Equivalent Employed	Institutional Survey Numbers: Graduate Students	Institutional Survey Numbers: PhD's Awarded	AAS Members	U.S. IAU Members
1970–71									1377	1186		2701	
1969–70	2440	1056	517	475	64	733			1311	1125	125	2587	728
1968–69			490	378	55			(815)			116	2434	
1967–68	1967	814	464	301	49	714	87	729			112	2223	
1966–67			440	221	46	672	53	642			81	1889	580
1965–66	1553	623	417	162	44	630	40	589				1648	
1964–65			360	144	38	570	52	549				1651	
1963–64	1185	470	310	127	33	456	40	497				1352	400
1962–63						423	28	457				1301	
1961–62	1101	444				379	25	429					320
1960–61						272	14	404				897	
1959–60	840	376					11	390					

[a] Until its termination following the 1970 census, the National Register of Scientific and Technical Personnel published biennially by NSF provided the only large-scale, continuing census of scientists in the United States. It contains vast resources of information, dealing with such issues as age, degrees, primary work activity, sex, and employer; however, these data must be used with extreme care, lest they be misleading. Below are some cautionary suggestions.

(1) Information for the Register is obtained by polling the members of professional societies, like the AAS. Roughly 80% (varies between about 70 to 90%) of the polled scientists return their questionnaires. Thus numbers listed in the Register are strictly counts of respondents, which is not, of course, the total population of scientists. Therefore, although the percentage of replying scientists varies slightly from sampling to sampling, an approximation to the absolute number of astronomers in the nation at a given year can be obtained by multiplying the figure given in the Register for that year by about 1.25.

(2) While absolute numbers in the Register are always too low, the relative distributions of the Register's figures over various subgroups are presumably reliable. Thus for a given year, percentages can be taken from the Register with reasonable confidence but total figures cannot.

(3) The questionnaire Specialties List (i.e., list of subfields in which a scientist might specialize) has changed in every biennial sampling for the Register. For instance, the subfields that define astronomy have been changed at least slightly in each of the polls. Consequently, comparisons among data from the Register for different years are not entirely reliable; an apparent change in a percentage distribution of astronomers with time might not be "real" but rather the result of incompatibility of the questionnaires and definitions of astronomy.

One such example was found in the 1970 version of the Register; an apparently minor change of category took some scientists called "astronomers" in the 1968 Register and relabeled them "physicists." The effect on the physics population was negligible but whereas 87% of persons whose PhD was earned in astronomy were called "astronomers" in the 1968 Register, in 1970 this change left only 59% of PhD's earned in astronomy departments in the category of "astronomers." The authors of this report re-examined the 1970 data, applying the *same* definitions used in 1968 (and 1966) in order to extract numbers consistent enough over time to detect meaningful trends.

(4) The Specialties List is divided into large topics (e.g., Atmospheric, Earth, Marine, and Space Sciences; Physics and Astronomy), which in turn are divided into specialties (e.g., Solar/Planetary; Mechanics; Astronomy), which are segregated into subfields (e.g., Planetary Atmospheres; Astrophysics; Galaxies). A respondent is classified under Astronomy in the Register if he selects subfields under that heading. For example, a number of scientists, about 1.5 times the number of astronomers in the Register, choose subfields like Planetary Atmospheres, Lunar and Planetary Geophysics, Tektites and Meteorites, Ionisphere, etc. Since these subfields are given in the Specialties List under titles far removed from physics and astronomy, NSF presumes that persons selecting them do not consider themselves primarily to be astronomers but rather "earth scientists" or "space scientists."

(5) Categorizing any scientist is difficult because, as NSF's data have shown, how a scientist should be classified depends upon the questions you wish to answer. Different replies occur when a respondent is asked the field in which he has the greatest competency or the field of his highest degree or the field in which he is employed or what he calls himself professionally. This unavoidable problem should be realized by all who use manpower data.

(6) Besides defining *who* is an astronomer, it is further difficult to specify *when* one is an astronomer. Most important in its effect on the statistics is the question of whether a graduate student should be counted as a professional scientist. The significance of this problem arises because there are so many young persons in the field. Since NSF obtains its information by polling members of professional societies, and since some graduate students do not join such societies until close to receiving their PhD, a mild selection effect is operative.

Some notes of caution are also useful on manpower data taken from the Institutional Survey.

(7) The institutional survey was made just once, for two years of manpower data (1969/1970 and 1970/1971). It was made *during* the second year (1970/1971) for which data were sought, thus introducing a temporal bias, since both years were not viewed from the same vantage point.

(8) While the survey requested information concerning only personnel in astronomy, it polled a number of departments of physics and of physics and astronomy not conventionally included in astronomy lists (e.g., those of the Office of Education or NSF). This would produce numbers higher than in conventional surveys.

(9) The survey asked not for PhD manpower but for the "PhD or equivalent." This, too, worked to produce a significant upward bias in the numbers compared with conventional PhD surveys. Apparently there were about 250 "PhD Equivalents in Astronomy" in 1970.

(10) The "total" numbers of scientific and technical personnel reported in Figure 1 (p. 56) of Volume 1 of this survey for the years 1969–1970 and 1970–1971 were obtained from the institutional questionnaire described in Section I of this chapter. These numbers include PhD and Equivalent and Technical but not Graduate Students. Figures for prior years were obtained by scaling up National Register data for Total Scientific and Technical Personnel by the ratio of the institutional questionnaire figure to the Register figure for the year 1969–1970. Similarly the "PhD or Equivalent" numbers in Figure 1 of Volume 1 for 1969–1970 and 1970–1971 were derived from the Institutional Questionnaire, and figures for prior years were scaled up from Register data for "PhD" personnel by the ratio of the respective figures for 1969–1970.

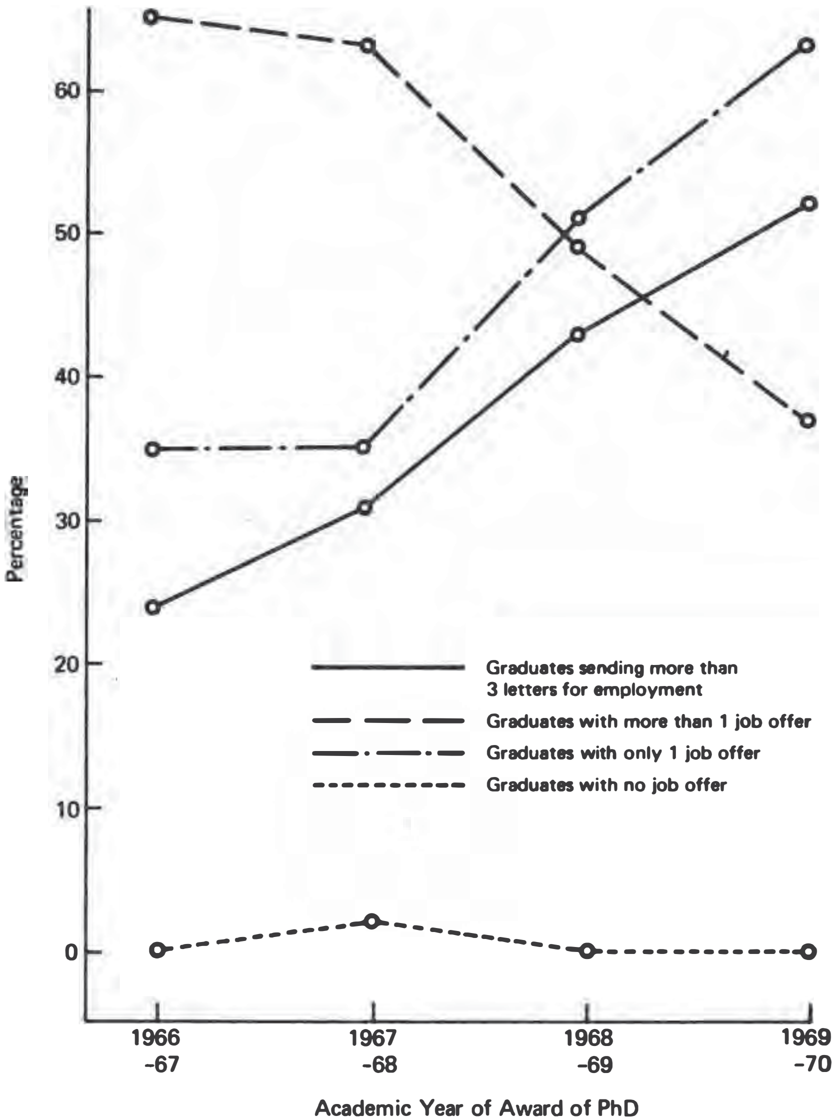

FIGURE 9.11 First postdoctoral employment (other than fellowship) of PhD's in astronomy: success at obtaining job versus year of award of PhD, 1967–1970.

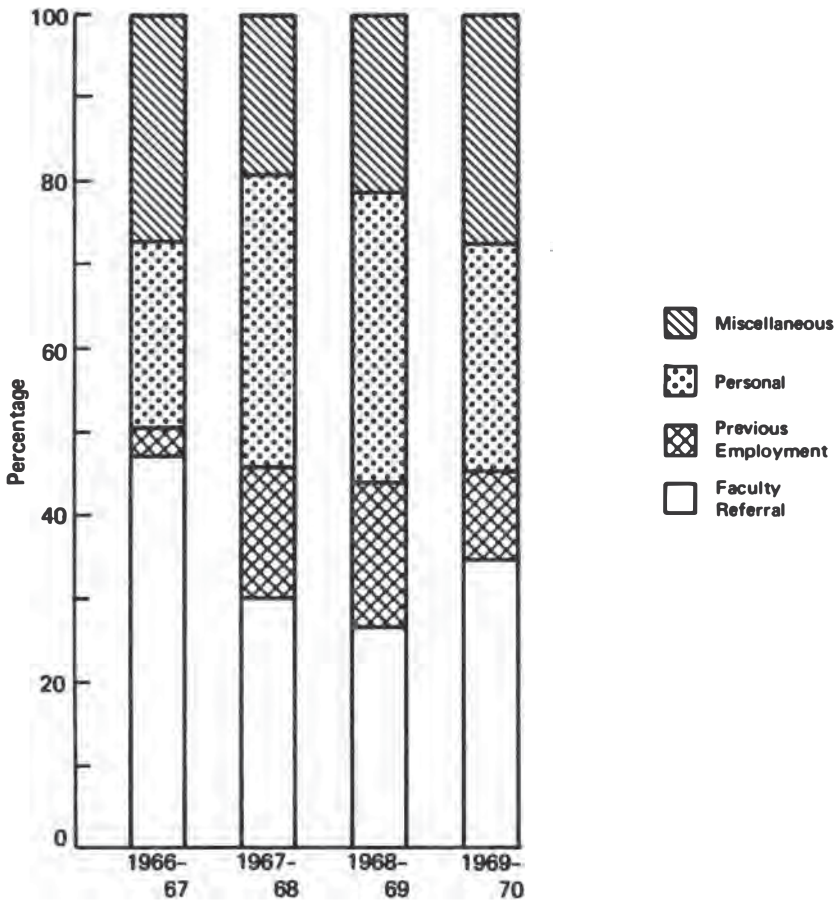

FIGURE 9.12 First postdoctoral employment (other than fellowship) of PhD's in astronomy: how job was obtained versus year of award of PhD, 1967–1970.

lems had restricted their employment search to specific geographic locations.

In contrast, a report of the Physics Economic Concerns Committee found for recent recipients of PhD's in physics that the unemployment and underemployment levels are about 4% and 2.5%, respectively.

The Executive Office of the American Astronomical Society reports that as of mid-1972, approximately 150 AAS members are seeking positions, but only four of these are now unemployed. Over 40% of this group are in temporary posts, such as postdoctoral fellowships. But as of mid-1972, the AAS has been notified by 29 employers of open posts.

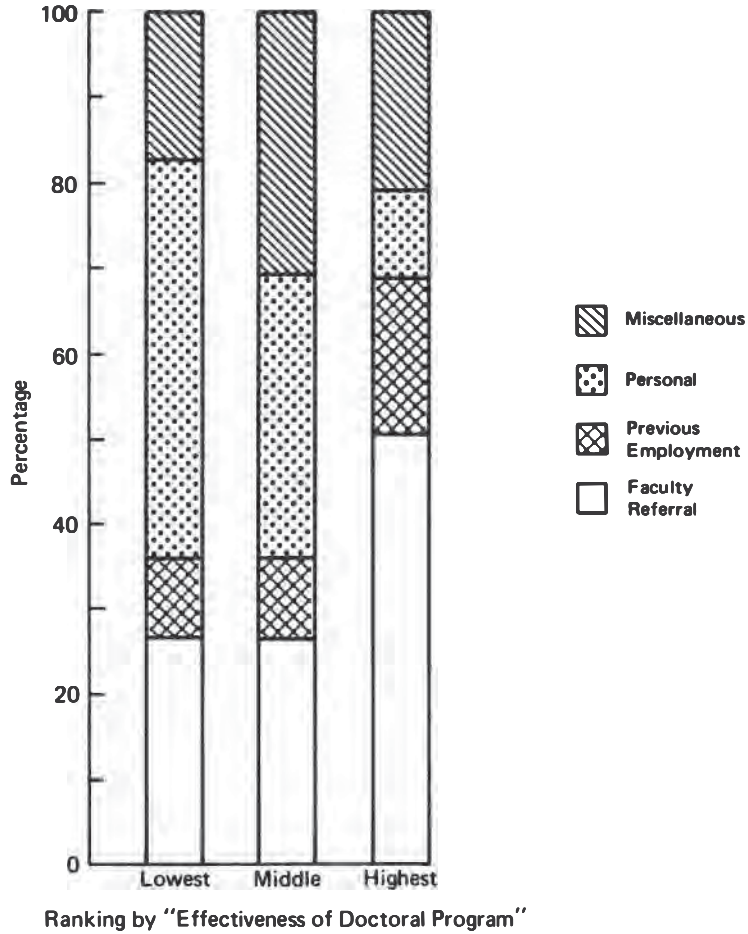

FIGURE 9.13 First postdoctoral employment (other than fellowship) of PhD's in astronomy: how job was obtained versus "effectiveness" of the doctoral program at the recipients graduate institution.

Even though 99% of the recent PhD's in astronomy are employed, not all of them found positions in astronomy. Twenty-four percent had looked for a position in other fields, usually physics, computer technology, or "teaching"; but only 9% of those employed today actually had taken a nonastronomical job, a normal migration rate in science. Those who had searched outside of astronomy had done so because of either job scarcity in astronomy or their greater interest in another field. Half of the 9% who are employed in other fields had made the change by choice, and a third of the 9% said that they had sought but had been unable to find a position in astronomy. These statistics suggest that the current demand for astronomers is essentially equal to the supply.

The situation for young physics PhD's is strikingly worse: about 30% of those who sought employment in traditional sectors of physics in this country failed to secure such jobs.

The percentage of astronomy PhD's seeking employment outside the field is an indicator of growing fear of job scarcity in astronomy. While 13% of the 1967 graduates looked outside of astronomy, 40% of the class of 1970 did so. Moreover, many of the graduates did not seek regular employment immediately after receiving their doctorates; instead, 31% obtained postdoctoral appointments. Only a tenth of these appointments were for less than one year, and no increase in a "holding pattern," or short-term tiding-over period prior to employment, was evident. Each year, from 1967 to 1970, approximately the same fraction of the graduates who applied for postdoctoral fellowships received them—about two thirds. But during that period the fraction who applied for such appointments rose.

In 1970, according to the National Register, about 50% of all astronomers were employed by educational institutions, 20% by the government, 10% by nongovernmental research centers, and 10% by industry. For just the PhD's working in the field, the pattern was different, with 65% being employed by educational institutions.

The employers of the recent astronomy doctorates (in terms of FTE percentages) were, in decreasing order: research on-campus at educational institutions, teaching on-campus at educational institutions, research at government facilities, and research off-campus at educational institutions. Those four endeavors account for over three fourths of the total working time of the recent astronomy doctorates.

The nature of the employment of the new PhD's has changed drastically during the past few years, as is shown in Figures 9.14 and 9.15. Employment at on-campus educational institutions plummeted from 80% for the 1967 graduates to 37% for the 1970 graduates. There was a simultaneous rise in the relative levels of employment in nonacademic positions. The nature of the work activities also shifted (Figure 9.15). The principal work activities of 1967 graduates in their first job were divided approximately into research and development (48%) and teaching (41%). The principal activities of the 1970 graduates were research and development (81%) and teaching (13%).

This shift may have arisen because traditional teaching positions are saturated, a situation long forecast by A. M. Cartter. The slowing of

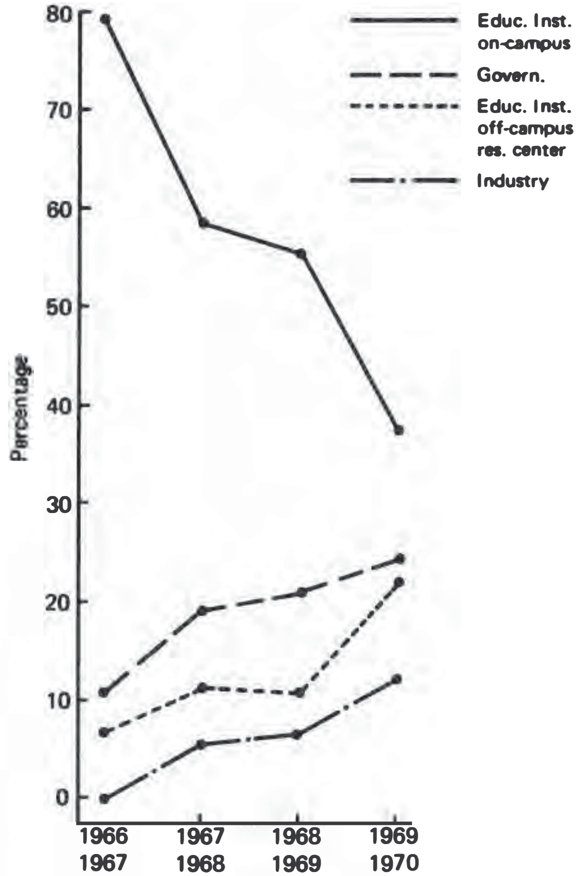

FIGURE 9.14 First postdoctoral employment (other than fellowship) of PhD's in astronomy: percentage FTE employers versus year of award of PhD, 1967–1970.

expansion of positions in astronomy is more likely than a preference by the graduates for new forms of employment.

This shift in employment patterns may be one of the reasons besides actual job scarcity for recent graduates needing to send increasing numbers of job applications. In the past, the traditional employer of astronomy doctorates was educational institutions, but this has changed.

Astronomers' ideal and present distributions of time and research were remarkably homogeneous, as Table 9.19 demonstrates. (Compare the distribution of time for all astronomers given in Section I, the Institutional Survey.) Among the respondents who spent more than half of

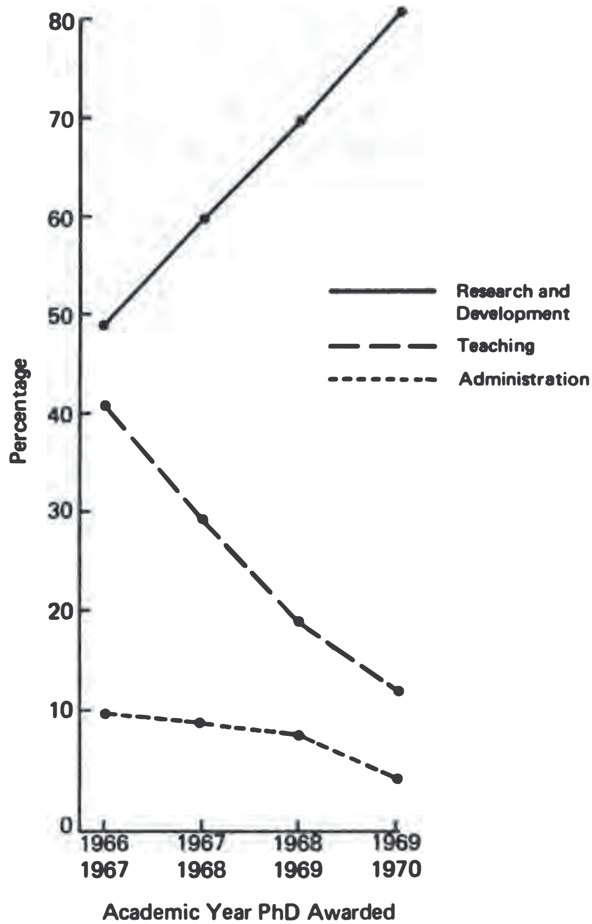

FIGURE 9.15 First postdoctoral employment (other than fellowship) of PhD's in astronomy: percentage FTE employment activity versus year of award of PhD, 1967–1970.

their time in research, almost 95% were able to devote at least three fourths of their research time exactly as they would like; in fact, only 7% of the total research time of the entire set of recent astronomy doctorates was spent in subfields that the individuals ideally would not pursue.

This study also attempted to assess employment disappointments by asking the respondents specifically for their complaints. The most frequently mentioned problem was not too much teaching (only 10% of the sample) but "lack of intellectual stimulation," mentioned by 25%.

TABLE 9.19 Distribution of Time in Research for Recipients of PhD's in Astronomy in the United States, 1967-1970

	Distribution of Time		
Subfield of Astronomy	Thesis	Present	Ideal
Theoretical astrophysics	38%	28%	33%
Observ., ground-based, optical	31	28	31
Observ., ground-based, radio	6	9	7
Observ., space-based, all	6	8	9
Instrument development	5	8	6
Celestial mechanics and astrometry	8	8	8
Other	6	11	6
TOTAL	100%	100%	100%

Another 25% of the sample reported no disappointments. Other frequently mentioned problems were "isolation" and the lack of miscellaneous support, such as assistants, travel funds, and secretaries.

Rumor holds that many young astronomers are forced to choose between teaching at remote, undesirable sites or taking mundane jobs below their technical competency. This study found this rarely to be true. There were not enough jobs in the Southwest to meet the demand, for instance; some astronomers had to accept employment in locales not of their preference.

On the whole, the situation could not be described as bleak; half of the persons in the study with a preference had obtained a job in the state of their first choice, and three fourths of them secured a position in one of their first three states of preference. Even though a job in one of the top choice states does not ensure that the particular institution within the state was desirable, only 18% mentioned any disappointment with their location, and often not for professional reasons.

B. SURVEY OF RECENTLY AWARDED PhD's IN ASTRONOMY

We present the detailed results of our survey of recent PhD's in the form of a copy of the questionnaire filled in with the statistical answers to questions where appropriate (Appendix II.A).

This questionnaire was sent to recipients of PhD degrees in astronomy or astrophysics at U.S. institutions between 1967 and 1970. It was mailed in the spring of 1971 under the auspices of the Statistical Panel of the Astronomy Survey Committee and included a cover letter from the Chairman of the Survey Committee, Jesse L. Greenstein.

Eighty-two percent of the persons polled returned their forms, which is a substantial response rate considering the length and complexity of the questionnaire. Efforts were made to ensure that all appropriate persons received questionnaires by having the forms forwarded to them by their graduate institutions. No sampling bias was detected for the respondents either in their graduate institutions or in their year of award of PhD. It is possible that persons who received their degrees in astronomy but have since left the field would not have received the form or would not have responded to it, leading to an unknown systematic effect.

C. CONCLUSIONS AND COMMENTS

Starting in the 1950's, manpower in American astronomy began to increase at a phenomenally large rate, although from the perspective of the last 50 years, the surge of the past decade only brought astronomy's secular growth roughly into line with that of other sciences. The magnitude of its recent expansion, especially when compared with funding (see Figure 9.16), has almost exactly brought the supply of astronomers into balance with the demand. This situation is dramatically different from that of the early 1960's, which was an employees' market. If the recent trend should continue, the supply would substantially exceed the demand.

How critical is the job problem in astronomy? The answer can only be given in relative terms. This study shows that the availability of jobs in the field today is severely limited compared with five years ago, but it is not so limited in astronomy as it is in many other sciences. Undoubtedly, one of the major factors that has exacerbated the job problem has been the migration into astronomy of scientists from other disciplines, especially physics, where the job situation is even more difficult. Even though the nation as a whole has been in a recession, astronomy has survived comparatively well.

Overall, employment levels in American astronomy are high, and despite anecdotal stories to the contrary, most of the recent astronomy doctorates seem fairly satisfied with their jobs. But many have had to seek employment outside the traditional academic environment and some away from their ideal geographic location; moreover, getting a job has often been difficult. The situation seems to be rapidly worsening.

How could the job market in astronomy be improved? As in any problem of supply and demand, there are two related remedies. Increasing the demand for young astronomers implies increased funding for the field, particularly on a per capita basis. However, as will be seen in Section III of this chapter, the current trend has been toward a significant

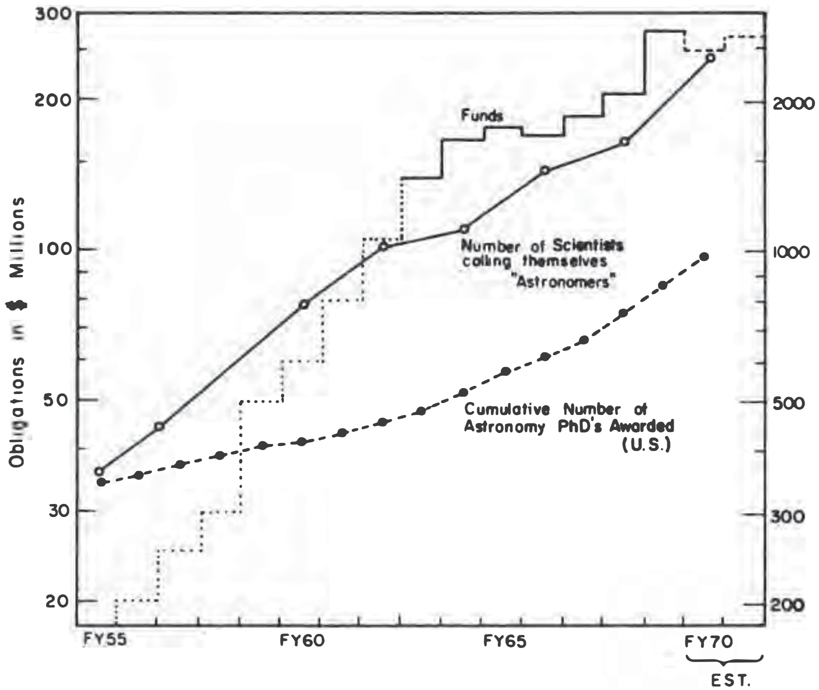

FIGURE 9.16 Federal obligations for basic research in astronomy compared with manpower in astronomy fiscal years 1955–1971. [Sources: *Federal Funds for Research*, etc., Vols. XIII–XIX (estimated before fiscal year 1963 and after fiscal year 1969) "Astronomers" from NSF's National Register of Scientific and Technical Personnel corrected for incompleteness. Cumulative numbers of U.S. PhD astronomy degrees from Whitford report, p. 30, and HEW report "Earned Degrees by Field, U.S. 1959–1969."]

reduction of per capita funding, and many research groups have had to curtail postdoctoral employment opportunities.

But what about decreasing the supply, reducing the number of astronomers? Two thirds of the young PhD's in this study said that relative to funding, there are too many astronomers in the United States today; over 80% of them recommended having graduate astronomy departments train fewer people. As we have noted, there are many signs that relatively fewer people are being trained: (1) the total number of graduate students in astronomy departments appears to be leveling; (2) the number of first-year astronomy graduate students is declining; (3) the fraction of new astronomy PhD's hired by academic institutions has declined from 80% to 40% in the last four years; and (4) the percentage of new astronomy PhD's employed in teaching has correspondingly declined from 40% to 15%.

But while reducing admissions of graduate students to astronomy departments would help to limit the supply, by itself it would deal with only part of the problem. Over half of the future astronomers in the United States will receive doctorates in physics rather than astronomy, and a reduction in physics department programs producing astrophysicists would be equally necessary.

The replacement of the 4% average annual growth rate that prevailed for four decades before the Whitford report by today's 15% to 20% rate, leads to the following prediction. According to Volume 1 of this report, an average annual funding growth rate of 5.5% is sufficient to accomplish the entire recommended program of the Astronomy Survey Committee. Since this rate is less than the average annual manpower growth rate (even the physical sciences norm of 7%), we predict that over the next several years there will be a significant decline in the average funding per astronomer. Clearly this will affect the nature of employment in astronomy, but how it will affect the number of jobs is problematical.

APPENDIX II.A Summary of Replies to Questionnaire Sent to Recipients of PhD Degrees in Astronomy/Astrophysics from U.S. Institutions, 1967–1970 (289 Forms Sent, 71% Usable Replies)

GENERAL

Age range 25–45 (Yr on 1 Jan 1971) (mean=30.4)	Sex	___Male 92.6% ___Female 7.4%	Married	___Yes 84.2% ___No 15.8%

Country of Birth 87.2% U.S.A. 2.5% Canada 10.3% other foreign
Country of Citizenship 87.7% U.S.A. 2.5% Canada 9.9% other foreign

EDUCATION

	Institution	Graduation Date	Major
Bachelor's	________	________	________
Master's	________	________	________
Doctorate	________	________	________

Number of years (full-time equivalent) from Bachelor's to Doctorate 3< range <11 5.3 Mean

APPENDIX II.A (*Continued*)

PART A FINAL TWO PREDOCTORAL YEARS

1. During this period, how was your time allocated by percentage?
   11.9% courses
   65.7 thesis
   18.6 nonthesis astronomical job (teaching or research)
   1.9 nonastronomy-oriented job
   1.9 other; specify ______
   100%

2. By percentage, indicate the sources that provided for your expenses during these two years.

	total educational (i.e., tuition, books)	living (food, rent, etc.)
teaching (TA)	42.8%	40.7%
astronomical research (RA)	7.0	5.8
nonastronomical job	34.6	26.3
fellowship	2.1	2.4
loans	0.5	1.7
personal or family	9.6	20.8
other; specify ______	3.4	2.0
	100 %	100 %

3. If you were supported by a fellowship of scholarship (other than a TA or RA), please specify source. Total=109 (17 by 2 sources)
   34 NSF
   35 NASA
   4 DOD
   21 University
   9 foundation (e.g., Woodrow Wilson)
   1 private individual or corporation
   22 other; specify ______
   __ unknown

4A. If you held a nonastronomical job, please denote the main reason why you chose that type of employment. Total, 19 responses
   4 most interesting
   4 only one available
   7 most rewarding financially
   4 other; specify ______

B. What was the nature of the work performed?
   16 technical
   3 nontechnical
   specify briefly ______

5. Did you apply for a postdoctoral fellowship? 93 Yes 106 No
   Did you receive one? 63 ‡ Yes 33 No ‡ includes 3 who did not apply
   If yes, what was the length of your appointment? ______ months

PART B EMPLOYMENT SEARCH FOR FIRST POST-PhD POSITION (OTHER THAN A POSTDOCTORAL FELLOWSHIP)

6. When searching for your first post-PhD position, how did the following items influence your planning? Please rank from 1 (none) to 5 (very strongly).

Means:

4.2 1 2 3 4 5 field; specify ______
2.9 1 2 3 4 5 geographical location

2.7 1 2 3 4 5 financial remuneration
3.7 1 2 3 4 5 opportunity for future advancement
2.9 1 2 3 4 5 educational opportunity
2.2 1 2 3 4 5 usefulness to society
3.2 1 2 3 4 5 nearness to "expert(s)"
3.4 1 2 3 4 5 availability of equipment
1 2 3 4 5 other; specify ______

7. Your professional preference by state (or country, if not the U.S.) for your first job.
Three most frequently chosen:
California (29.5%)
Colorado (10.7%)
Arizona (8.7%)
48.6% got 1st choice
9.1% got 2nd choice
The actual state (or country) of your first job. 9.7% got 3rd choice

8. Number of formal employment applications sent. See Figure 9.10
Number of positive application responses received. ______

9. Number of job interviews at your graduate institution. ______
Number of trips to potential employers for "on-site" interviews. ______

10. Number of definite job offers made to you. See Figure 9.10
Number that were acceptable and attractive to you. ______

11. Did you actively seek employment outside astronomy? 47 Yes (24%) 149 No (76%)
If yes, in which field(s)?
30 physics
5 engineering
10 computers
2 business
13 teaching; field ______
10 other; specify ______

Why?
11 greater interest in other field
__ sought by other employer
__ financial gain
__ desire for more education
2 more socially pertinent job/field
14 scarcity of astronomy jobs
18 other; specify ______

PART C POST-PhD EMPLOYMENT (NOT POSTDOCTORAL FELLOWSHIP)

12A. Have you been employed full-time since you finished your academic work?
_____ Yes _____ No

B. Are you employed now? 184 Yes 2 No (1.1%)
If yes, in astronomy? 154 Yes 19 No (11%)
If no, for how many months? _____
Why? ______

13A. How did you learn of your first position?
2.1% visit by employer for university
24.6 personal solicitation by you
13.1 previous employment
32.4 faculty referral
6.8 family connection or friend
1.6 job listed in university files
19.4 misc.

APPENDIX II.A (*Continued*)

B. If you have had more than one astronomical job, indicate how you learned of the subsequent positions (2nd, 3rd, etc.)

About 95% had only 1 job since obtaining their PhD.

14. By employer or activity, distribute your actual working time by percentage. The sum over the ENTIRE grid should = 100%.

	Gov.	Industry	Educ. Inst. On-Campus	Educ. Inst. Off-Campus Research Center	Other	Row Sums
Research	15.0	4.2	26.8	9.9	1.7	57.6
Design & Development	0.8	1.8	2.8	1.7	0.8	7.9
Teaching	0.1	0.0	24.1	0.4	0.1	24.7
Administration	2.4	0.4	2.7	0.9	1.1	7.5
Other	1.1	0.1	0.8	0.1	0.1	2.2
Column Sums	19.4	6.5	57.2	13.0	3.8	100.0

15. What aspects of your first job disappoint(ed) you?
- 3 too much research required
- 20 too much teaching required
- 23 financial remuneration
- 37 geographic location (of total 203, 51 had multiple disappointments)
- 51 intellectual stimulation
- 77 other; specify incl. isolation, lack of money for supportive personnel; too much administration
- 51 no disappointments

16. Are you now searching for a new position? 25 Yes 75 No
If yes, why? ______

17. If you are an observer, 122 answered this question

A. As a graduate student, did you use a national observatory? 22 Yes ___ No
local observatory? 48 Yes ___ No
both? 33 Yes ___ No
neither– 19

B. Do you now use a national observatory? 18 Yes ___ No
local observatory? 54 Yes ___ No
both? 39 Yes ___ No
neither– 11

18. On the grid below, please indicate by percentages how you allocate your *research* time. Please show the distribution in three ways: what it was for your PhD thesis; what it actually is now; and what it ideally would be now.

	Thesis	Present	Ideal
(a) ground-based optical observations of objects outside the solar system	25.8	21.0	24.4

(b)	ground-based radio observations of objects outside the solar system	5.5	7.8	7.1
(c)	ground-based optical & radio observations of the solar system, not the sun	2.2	2.9	3.0
(d)	ground-based optical observations of the sun	3.2	4.0	3.5
(e)	ground-based radio observations of the sun	0.4	0.5	0.3
(f)	space-based observations of all kinds, of all objects other than the sun	3.9	6.5	7.2
(g)	space-based observations of the sun	1.8	1.3	1.3
(h)	laboratory astrophysics	1.8	1.1	2.1
(i)	general-purpose instrument development	4.6	8.4	5.6
(j)	astrometry and celestial mechanics	8.1	7.7	7.8
(k)	theoretical astrophysics	36.3	27.0	30.7
(l)	other; specify ______	5.9	5.8	6.0
		100%	100%	100%

19A. Do you find your research restricted by reasons other than personal time?

122 Yes (62%) 45 No (38%)

B. If yes,* check contributing shortages(s)

No. of replies:

- 27 available observing time on local optical telescopes
- 4 available observing time on local radio telescopes
- 15 available observing time on national optical telescopes
- 6 available observing time on national radio telescopes
- 36 funds for computer time
- 62 funds for laboratory assistants
- 65 other; specify ______

*63 had multiple complaints

20. Relative to current and anticipated funding, how would you rank the supply of astronomers in the U.S. today?

66% Too many 32% About right 2% Too few

Considering the realities of funding, should graduate astronomy departments train fewer people? 82% Yes 18% No

21. If you could change your graduate program in light of your work experience, how would you alter these factors from 1 (much less) to 5 (much more)?

Means			Means		
3.7	instrument training	1 2 3 4 5	2.9	thesis topic specialization	1 2 3 4 5
3.3	computer applications	1 2 3 4 5	3.1	humanities	1 2 3 4 5
3.1	history of astronomy	1 2 3 4 5		other: ______	1 2 3 4 5
3.3	science courses not related to astronomy	1 2 3 4 5			

22. If you directed federal funding for astronomy, what would be your three highest priorities?

1st ______

2nd ______

3rd ______

23. Any other comments that you think might be helpful and pertinent would be appreciated.

III. FEDERAL SUPPORT OF ASTRONOMICAL RESEARCH IN THE UNITED STATES

A. INTRODUCTION

Early meetings of the Astronomy Survey Committee and its various panels produced a number of questions that partially required statistical data for their answers. Among the questions related to funding sources were the following:

1. How is astronomical funding divided between new capital costs, operating costs, scientists' salaries, and supporting personnel costs, and how has this been changing with time?
2. How do recent changes in astronomy funding and manpower growth compare with the situation in related sciences like physics and in the total U.S. research and development picture?
3. Admitting the difficulties of prognosis, how closely does the present allocation of funding and manpower resources among the subdiscipline areas of astronomy reflect the expected scientific return from each area?
4. A specific example is the distribution of resources between space- and ground-based observational research. Are space observations "cost-effective" if an ultraviolet or other space photon costs ten times as much to collect as a visible photon measurable from the ground? Within the space program, what is the present allocation among ultraviolet, x-ray, and gamma-ray research or among solar, galactic, and extragalactic astronomy?
5. Another question is the balance between the National Science Foundation (NSF) national research centers and the universities they serve. What fraction of new capital equipment is being placed in the hands of leading university-based research groups? How are resources allocated between the NSF centers and the NSF basic research program?
6. Within the National Aeronautics and Space Administration (NASA) astronomy research effort, what fraction is done "in-house" at NASA centers? How has NASA's astronomy research effort been affected by the Apollo program?
7. How much space astronomical research is supported by agencies other than NASA? How much is being done by the NSF, by the Atomic Energy Commission (AEC), and by the Department of Defense (DOD)?
8. How much ground-based astronomical research does NASA support? What large optical telescopes has NASA helped to construct in support of the planetary exploration program? How much

time on NASA's giant Deep Space Network of tracking antennas is devoted to radio-astronomical observations?

9. What percentage of all astronomical research is supported by the DOD, and how has this been affected by the recent relative de-emphasis of military basic research?

B. FEDERAL FUNDS FOR ASTRONOMY

An estimate of the total federal support for astronomical research should include contributions from the NASA, NSF, the DOD, the Smithsonian Institution, the Department of Commerce, and the AEC. The series *Federal Funds for Research, Development, and Other Scientific Activities,* published annually by NSF, as a report to Congress, has included obligations* for astronomy *basic* research since fiscal year 1963, and astronomy *applied* research since fiscal year 1967. A summary of this record of astronomy support is given in Tables 9.20 and 9.21.

C. NASA RESEARCH IN ASTRONOMY

NASA provides over 80 percent of the federal funds for research in astronomy reported in Tables 9.20 and 9.21. Since many of these funds are spent on "indirect" costs it is worth noting that our survey of astronomical institutions (Section I) showed that 65% of the federal support received by academic institutions and 56% of all federal support came from the space agency in fiscal year 1970.

Astronomical research at NASA accounts for almost one third of all basic research in the agency, and this has been true for several years as can be seen from Figure 9.17. The way NASA distributes the other two thirds of its research budget is shown in Table 9.22, as reported to the Congress through NSF. The way the astronomy fraction is divided among various programs or projects is shown in Table 9.23, again as reported to the Congress through the NSF.

NASA's budget identifies its various astronomy-related projects in a significantly different way. In particular, all the "indirect" costs (e.g., tracking costs and a share of NASA management expenses) and some of the "direct" costs of Table 9.23 (e.g., launch vehicles) are reported on separate budget lines. Table 9.24–9.29 below contain essentially all

*An "obligation" means that a government agency has signed a contract or made a grant to an astronomical institution. When funds are spent, they are reported as expenditures, often in later fiscal years. Unless specifically noted all figures in this section are obligations. Figures in Section I (the institutional survey) represent expenditures. With one exception noted, all dollars in this section are current dollars, without corrections for inflation.

TABLE 9.20 Federal Obligations for Basic Research in Astronomy (in Millions of Current Dollars)[a]

	Fiscal Year[b]									
	1963	1964	1965	1966	1967	1968	1969	1970	1971	1972
NASA	125.7	148.3	156.7	143.9	154.5	176.0	245.3	(170.4)	(180.0)	(148.4)
NSF	9.1	10.2	11.0	15.6	14.6	16.5	23.0	(19.9)	(23.2)	(28.7)
NSF[c] plus facilities		[14.4]	[19.4]	[22.5]	[20.6]	[27.9]	[28.4]	[21.2]	[27.6]	[n.a.]
DOD	4.4	9.2	8.3	8.9	8.1	9.5	9.6	(9.6)	(6.1)	(6.2)
Smithsonian Institution	0.9	0.7	0.8	1.3	2.7	2.9	3.3	(3.2)	(3.6)	(4.6)
TOTAL[d]	140.1	168.5	176.9	169.9	179.9	204.9	282.2	(203.9)	(213.8)	(188.7)

[a] Source: *Federal Funds for Research, Development, and Other Scientific Activities,* Vols. XIII–XX, Tables C-36–C-38.
[b] Figures not available for fiscal year 1962 and earlier.
[c] Bracketed figures in this section are subtotals or other numbers *not* to be included when totaling columns. The NSF plus facilities line is shown here because it was used in Volume 1, Figure 3. Parentheses around numbers indicate estimated figures.
[d] Figures do not include facilities and may not add to total due to miscellaneous smaller amounts not shown but obligated by the Department of Commerce and the Office of Science and Technology. Data for fiscal years 1971 and 1972 are estimates.

TABLE 9.21 Federal Obligations for Applied Research in Astronomy (in Millions of Current Dollars)[a]

	Fiscal Year				
	1967	1968	1969	1970	1971
NASA	8.82	16.63	10.30	(13.63)	(11.43)
NSF	0	0	0	0	0
DOD	9.53	17.37	7.8	(8.4)	(8.6)
Army	[.93]	[.01]	[.04]	[.01]	[0.01]
Navy	[.30]	[.30]	[.31]	[.24]	[0.64]
Air Force	[6.00]	[16.00]	[6.45]	[7.54]	[7.69]
Defense agencies	[2.30]	[1.06]	[1.01]	[.60]	[0.30]
Smithsonian Institution	0	0	0	0	0
TOTAL[b]	18.35	34.00	18.11	22.06	20.46

[a] Sources: *Federal Funds for Research, Development, and Other Scientific Activities*, Vols. XIX, XVII, and XVIII, Tables C-55, C-56, C-57.
[b] Totals include smaller items from other agencies.

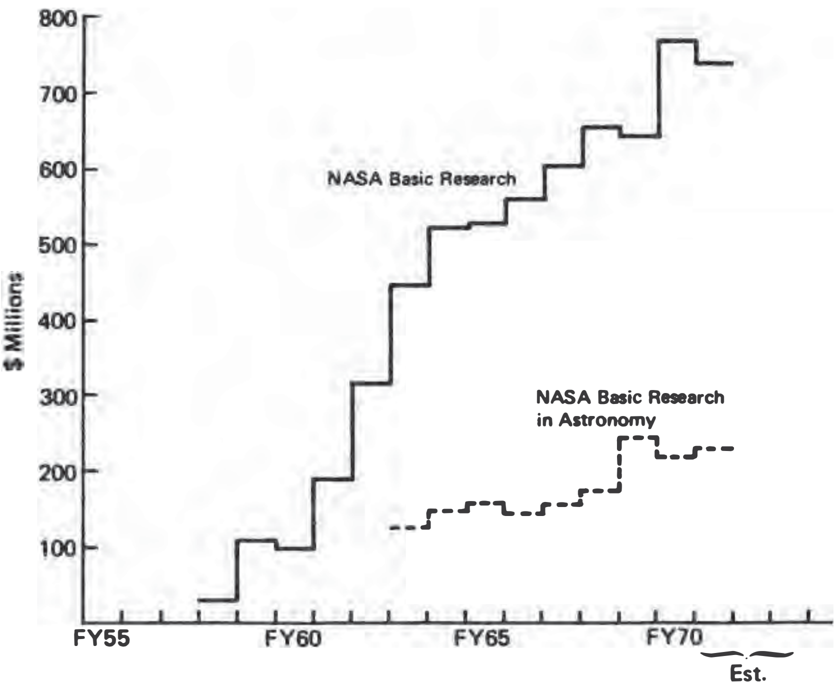

FIGURE 9.17 NASA obligations for basic research and NASA basic research in astronomy. [Source: *Federal Funds for Research, Development, and Other Scientific Activities*, Vol. XIX; astronomy numbers from Vols. XIII–XIX.]

TABLE 9.22 Distribution of the Other 70 Percent of NASA Basic Research, Fiscal Year 1970 Budget[a]

	Astronomy	Environmental Sciences	Physics	Engineering	Life Sciences	Biology	Chemistry
$Million	220	250	109	78	69	3.4	2.6
Percent	31	34	15	10	9	0.5	0.4

[a] Source: *Federal Funds for Research, Development, and Other Scientific Activities*, Vol. XIX.

the items in the NASA budget that support astronomical research, although obviously only a fraction of many of these items represent astronomy. The following percentages of the major NASA programs can be roughly identified as astronomical research:

In the Office of Space Science (OSS):
- 65% of the Physics and Astronomy Program (Table 9.26)
- 15% of the Lunar and Planetary Exploration Program (Tables 9.27 and 9.28)

In the Office of Manned Space Flight (OMSF):
- 100% of the Apollo Telescope Mount and smaller astronomy experiments on Skylab *or* about 10% of the Skylab mission (Table 9.30)

In the Office of Tracking and Data Acquisition (OTDA):
- 100% of the "project unique" costs of astronomy missions and 2% of the "operational base" (see Table 9.29)

In the Office of Advanced Research and Technology (OART)
- 10–25% of Large Diffraction-Limited Telescope Research (astronomy-related research in OART is estimated to involve about 4$ million per year).

The trend of various astronomy budget line items and the number that NSF reports to Congress as "Basic Research in Astronomy" at NASA are shown in Figure 9.18.

The numbers from *Federal Funds for Research, Development, and Other Scientific Activities* (Tables 9.22 and 9.23) and the NASA budget numbers (Tables 9.24 to 9.29) both contain certain "nonreprogrammable" funds that would have to be spent by the agency even if astronomy programs were not continued. The NASA astronomy program staff prepared an independent estimate of what they describe as "direct" and "reprogrammable" astronomy funding. This estimate is shown in Table 9.30. The reader who may attempt a detailed comparison of Tables 9.23, 9.24, and 9.30 should keep in mind the different definitions of lines with similar titles. For example, in Table 9.23 the costs of launch vehicles are included in line items such as the Orbiting Solar Observatory. In Table 9.24 the entries include astronomy *and physics* projects. The estimated astronomy percentage of each line is extremely coarse.

In Figure 9.19 the annual budgets for the Observatory series of spacecraft are plotted alongside the schedule of completed and planned launches in each series. Note that the first OSO was launched successfully after only about 10 to 15% of the total cost (through fiscal year 1972) had been spent. Note also that the first OAO was only launched (unsuccessfully) after about 50% of the total cost through fiscal year 1972 had been spent.

D. NSF RESEARCH IN ASTRONOMY

Based on its annual reports to the Congress, the NSF provides about 10% of the funds spent by the government for basic research in astronomy. The survey of American observatories, departments of astronomy, and other institutions engaged in astronomical research, found that 26% of the funds they received from the federal government were provided by the NSF. The difference is largely in the fraction of NASA astronomy expenditures that go to aerospace contractors and to in-house NASA expenses, which are not under the *direct* management of astronomy researchers.

Astronomical research at NSF has averaged about 10% of the Foundation's research budget in recent years, compared with 15% for chemistry and 20% for physics research. As with the NASA budget, the amount of money spent by the NSF on astronomy research depends on details of how astronomy is defined. In Table 9.31 are four different methods of estimating NSF support of astronomy research, included to illustrate the difficulties involved in determining simple, unambiguous budget figures. These problems must be kept in mind in interpreting year-to-year fluctuations.

The first column is drawn from the report to the Congress, prepared each year by NSF for inclusion in *Federal Funds for Research, Development, and Other Scientific Activities.* Since major capital equipment is not included in "Basic Research," the large 1968–1969 peak in funding associated with construction of the two 150-in. telescopes does not appear. The second is taken from reports prepared by the NSF astronomy program staff for the White House Office of Science and Technology. Here the peak fiscal year 1968 costs of the two large optical telescopes appear. Large fluctuations are to be expected when significant

TABLE 9.23 NASA Estimated Obligations for "Basic" Research in "Astronomy" ($Thousands)[a, b]

	Fiscal Year						
Program/Project	1965	1966	1967	1968	1969	1970 (est.)	1971 (est.)
Office of Manned Spaceflight							
Apollo experiment	–	1,090	79	163	47	3,005	2,145
Space flight operations (incl. Skylab ATM experiment)	–	6,940	6,400	12,748	50,728	21,225	18,434
Office of Space Science							
Physics and Astronomy Program							
P&A Supporting Research and Tech./Advanced Studies	7,525	7,620	8,146	10,500	10,945	11,564	11,200
Mariner	7,916	–	–	–	–	–	–
Orbiting Solar Observatories (incl. AOSO)	18,367	20,052	12,106	12,707	16,212	16,000	16,100
Orbiting Astronomical Observatories	40,880	30,800	33,703	54,349	45,192	37,200	28,600
Orbiting Geophysical Observatories	4,697	3,632	2,877	2,177	1,343	680	600
Explorers	2,081	8,627	14,416	9,636	7,724	9,832	12,780
Sounding rockets	5,580	5,790	6,000	5,665	6,030	6,432	6,432
Data analysis	–	580	503	810	204	481	400

Lunar and Planetary Program							
L&P SRT/Advanced Studies	4,036	4,140	4,247	3,829	4,320	3,545	3,434
Pioneer	18,657	8,350	4,059	4,242	3,520	12,700	24,400
Viking	–	–	–	–	509	1,200	1,400
Planetary astronomy	–	–	–	–	3,700	3,800	4,800
Office of University Affairs							
Training and Research Grants	866	1,025	1,200	519	420	330	–
TOTAL, DIRECT PROGRAM	110,605	98,646	93,736	117,345	150,894	127,994	130,725
Other (Indirect)							
Obligations allocated to the above projects for tracking and data-acquisition services, administrative operations, and other support	46,125	45,261	60,733	58,706	94,436	92,561	100,742
Indirect as a percentage of total	29%	31%	39%	33%	38%	42%	47%
NASA TOTAL (AS REPORTED TO NSF FOR THE FEDERAL FUNDS SERIES)	156,730	143,907	154,469	176,051	245,330	220,555	231,467

[a] Costs of launch vehicle for specific projects are included in the line items.
[b] Source: W. Lilly, NASA Administration Office.

TABLE 9.24 NASA Physics and Astronomy Programs ($Millions)[a]

	Fiscal Years													
	1960	1961	1962	1963	1964	1965	1966	1967	1968	1969	1970	1971	1972	Astronomy % of Line
OAO	35	6.81	34.8	31.8	35.6	31.4	22.3	27.7	40.4	36.9	(31.6)	(27.1)	(13.4)	100
OGO			18.5	27.5	42.9	30.2	28.2	24.0	20.2	13.2	(6.0)	(5.2)	(0)	
OSO	1.86	3.7	3.24	8.65	13.8	9.90	9.7	9.80	12.1	12.0	(14.7)	(16.1)	(18.6)	100
AOSO					6.2	7.9	9.3							
HEAO													(13.4)	50
Explorers			–	9.3	15.5	22.0	18.6	19.2	17.3	19.6	(18.9)	(25.6)	(22.6)	50
Sounding Rockets			7.76	11.5	17.0	16.0	19.3	20.0	20.0	20.1	(18.5)	(18.5)	(18.0)	50
Airplane										1.0	(1.6)	(3.0)	(2.7)	100
Data Analysis							2.	2.	2.9	3.4	(3.0)	(3.0)	(5.0)	10
Supporting Research and Technology[b]			5.2	13.7	16.0	16.5	20.6	19.9	23.0	19.9	(17.5)	(17.5)	(16.4)	50
PHYSICS AND ASTRONOMY SUBTOTAL			66.5	102.4	147.0	133.9	130.0	122.6	135.9	126.1	(111.8)	(116.0)	(110.1)	
Launch Vehicles[c]			11.0	29.2	50.9	(40?)	28.7	37.8	42.1	16.4	(17.7)	(11.6)	(11.0)	50
Tracking and Data Acquisition (Unique Costs)			(~2)	(~3)	(~3)	(~4)	(~4)	(~5)	5.2	5.7	(5.8)	(3.9)	n.a.	50

[a] Sources: NASA Budget Estimates, fiscal years 1962–1971, and NASA staff for fiscal year 1972 (NASA internal documents).
[b] See Table 9.25 for a further breakdown of this SR&T budget.
[c] See Table 9.26 for a breakdown of Launch Vehicle costs.

TABLE 9.25 NASA Physics and Astronomy Supporting Research and Technology ($Millions)[a]

	Fiscal Year									
	1963	1964	1965	1966	1967	1968	1969	1970	1971	1972
Astronomy	2.1	3.0	3.14	3.69	3.7	2.992	3.859	(2.7)	(3.0)	(2.9)
Astrophysics	–	–	–	–	–	0.300	(0.266)	(0.44)		
Solar physics	1.6	2.42	2.7	3.52	3.50	3.169	(3.793)	(2.6)	(2.5)	(2.4)
Energetic particles	3.4	4.2	5.3	6.3	6.4	3.304	(3.845)	(3.0)	(3.0)	(3.2)
Magneto-dynamics	1.0					2.459	(2.428)	(1.6)		
Ionosphere and radio	1.8	0.88	1.10	1.0	1.1	1.302	(1.427)	(1.0)		
Meteorology	0.6	0.86	1.11	1.4	1.3	1.347	(1.359)	(0.9)	(0.7)	(0.8)
Interdisciplinary	0.8	1.50	2.145	3.21	2.91	7.183	(4.345)	(3.0)	(2.6)	(2.0)

[a]Sources: NASA budget estimates and NASA staff (fiscal years 1971 and 1972).

capital expenditures are involved. The third column is drawn from two of NSF's internal publications–the NSF Databook and the NSF Annual Reports. Finally, the last column contains figures prepared by the Astronomy Section of NSF expressly for the use of the Astronomy Survey Committee.

TABLE 9.26 NASA Physics and Astronomy Launch Vehicles Breakdown of Launch Vehicles by Project ($Millions)[a]

	Fiscal Year									
	1962	1963	1964	1965	1966	1967	1968	1969	1970	1971
Explorer L/V										
Scout		11.2	8.8	n.a.	6.3	4.300	4.600	2.300	4.400	3.900
Delta					4.8	13.683	12.900	2.700	6.400	6.200
OAO L/V										
Agena	3.4	4.3	15.1	~10	8.5	5.400				
Centaur						6.617	10.000	8.800	5.600	1.500
OSO L/V	2.5	2.7	4.8	~2	1.0	2.000	4.200	2.400	1.300	n.a.
OGO L/V Agena	5.1	11.0	22.2	n.a.	8.1	5.800	6.400	0.233	0	0
LAUNCH VEHICLE TOTAL	11.0	29.2	50.9	–	28.7	37.8	42.1	16.4	17.7	11.6

[a]Source: NASA budget estimates.

TABLE 9.27 NASA Lunar and Planetary Exploration Programs ($Millions)[a]

	Fiscal Year								
	1964	1965	1966	1967	1968	1969	1970	1971	1972
Supporting Research and Technology[b]	19.1	22.	23.	20.9	19.8	18.6	18.0	(17.4)	(18.2)
Advanced planning of missions and technology					12.0				(3.2)
Data analysis					6.	2.3	2.6	(3.9)	
Voyager		80.	17.1	10.45	1.0				
Pioneer	13.6	14.7	12.7	7.2	7.0	4.7	20.8	(32.9)	(15.3)
Ranger	30.6	16.5	1.0						
Lunar Orbiter	20.0	41.8	58.1	28.8	9.5				
Surveyor	70.7	76.	104.6	84.5	33.0				
Mariner Mars '64	42.1	19.							
Venus '67			17.6	35.2	3.4				
Mars '69					62.8	26.1	4.5	(0.20)	(0.1)
Mars '71						20.0	60.3	(29.6)	(16.9)
Mercury '73						–	1.0	(21.1)	(44.6)
Viking						12.4	40.0	(35.0)	(176.2)
Planetary astronomy					–	3.7	3.7	(4.7)	(4.8)
TOTAL	196.1	198.0	234.1	187.0	154.5	87.8	150.9	(144.8)	(279.3)
Launch Vehicles									
Pioneer L/V									
Delta			4.0	1.2	2.0	1.2			
Centaur						1.3	4.6	(15.9)	n.a.
Mariner L/V									
Atlas–Centaur						11.0	9.8	(4.3)	n.a.

[a]Source: NASA budget estimates and NASA staff (fiscal year 1972).
[b]The astronomy fraction of this SR&T line is broken down further in Table 9.28.

NSF's budget is divided into basic research grants, specialized research facilities (major capital equipment, primarily at universities), the National Astronomy Observatories (in which radio astronomy costs have been separated to show trends in that field), institutional support (grants to universities), and science education.

Table 9.32 shows that the National Observatories were a little more than half of the NSF budget at the time of the Whitford report, increased in the years when the two 150-in. telescopes dominated the budget, and have recently averaged about two thirds of NSF astronomy obligations, fluctuating with new facilities construction.

Individual budgets for the National Centers, including construction

TABLE 9.28 Planetary Astronomy–Formerly Part of L&P SR&T Science ($Thousands)[a]

	Fiscal Year								
	1964	1965	1966	1967	1968	1969	1970	1971	1972
Major facilities	500	975	3000	1447	400	600	0	(1000)	(1000)
Instrumentation	370	0	560	575	518	405	432	150	245
Observations	1959	1945	1914	2055	2417	2250	2844	3320	3290
Theory/lab	180	57	385	431	357	345	342	230	265
Space Science Board, NAS-NRC					197	100	122	0	0
TOTAL	3009	2977	5859	4508	3889	3700	3800	4700	4800
JPL and NASA Centers [Sub-total of above]	[355]	[535]	[1180]	[1195]	[1118]	[1095]	[1213]	[1108]	[1195]

[a] Source: NASA program staff.

TABLE 9.29 NASA Astronomy Project–Unique Tracking Costs and "Base of Operations" Cost ($Thousands)[a]

	Fiscal Year			
Project	1968	1969	1970	1971
OSO	20	312	470	(378)
OAO	4924	3423	2878	(2757)
OGO	162	1325	1363	(193)
Pioneer				
IMP	125	179	307	(173)
Explorers				
Uhuru (SAS)	–	240	142	(262)
RAE-A	–	230	680	(162)
TOTAL PROJECT–UNIQUE COSTS	5231	5709	5840	(3925)

OTDA Base of Operations[b] ($Millions)

Operations	229.	233.	(229.6)
Satellite Net	43.5	42.4	(44.)
Deep Space Net	32.2	36.	(39.8)
Manned Spaceflight Net	83.5	86.4	(79.2)

[a] Source: OTDA staff.
[b] Approximately 2 percent of the operational base is astronomy related.

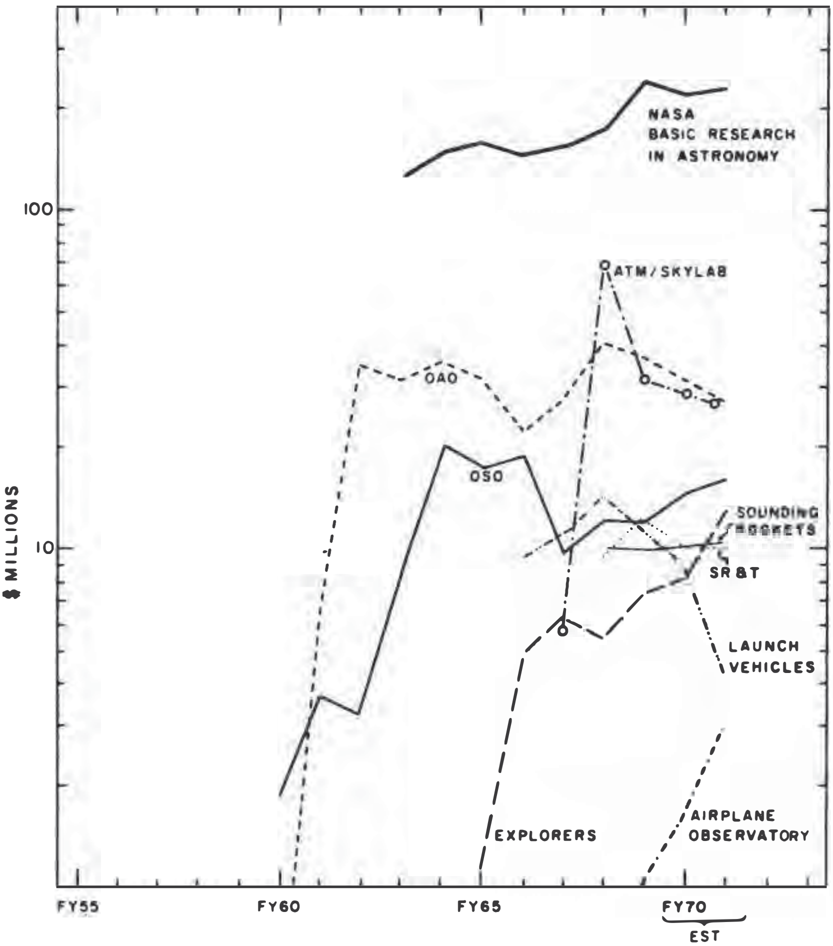

FIGURE 9.18 Selected NASA budget line items compared with NASA "Basic Research" in astronomy. The total of the budget line items does not equal the amount "Basic Research" since launch vehicles, tracking costs, and other indirect costs would have to be included; see Tables 9.23 and 9.30 for details. [Sources: Budget line items from NASA budget estimates, fiscal years 1962–1971. NASA "Basic Research in Astronomy" is from *Federal Funds for Research, Development, and Other Scientific Activities*, Vols. XIII–XIX.]

facilities, are given in Table 9.33. Figure 9.20 compares the fiscal growth of the center to the growth in the number of U.S. scientists they serve.

The facilities' budgets at the astronomy centers are compared with new capital investment by NSF in research instrumentation at universities in Table 9.34. The overall facilities budget has declined since the

TABLE 9.30 NASA "Astronomy" Direct Program–"Reprogrammable" Costs (\$Millions)[a]

	Fiscal Year										
	1962	1963	1964	1965	1966	1967	1968	1969	1970	1971 (est.)	1972 (est.)
OAO	34.8	31.8	35.6	31.4	22.3	27.7	40.4	36.9	33.3	(26.4)	(12.9)
LST study											(0.5)
OSO	3.2	8.6	13.8	9.9	9.7	9.8	12.1	12.0	14.5	(16.1)	(18.6)
AOSO			6.2	7.4	9.3						
			20.0	17.3	19.0						
HEAO											(13.4)
Explorer Satellites				1.1	4.7	6.1	5.5	7.4	8.2	(12.8)	(12.3)
Sounding Rockets	3.9	6.7	8.5	8.	9.6	10.	10.2	10.0	10.3	(10.5)	(9.0)
Airplane								1.0	1.6	(3.0)	(2.7)
Data Analysis					0.1	0.1	0.1	0.3	0.4	(0.4)	(0.4)
Supporting Research and Technology	2.5	6.8	8.	8.2	10.	10.	9.5	12.2	10.2	(10.2)	(8.2)
SUBTOTAL (OSSA)	41.4	53.9	72.1	66.0	65.7	63.7	77.8	79.8	78.5	(79.4)	(75.9)
Launch Vehicles	5.9	7.0	20.	12.	9.5	14.	14.2	11.2	8.9	(4.3)	n.a.
SUBTOTAL	47.3	60.9	92.1	78.0	75.2	77.7	92.0	91.0	87.4	(83.7)	n.a.
ATM/Skylab						5.7	69.5	34.3	28.3	(27.2)	(18.7)
TOTAL	47.3	60.9	92.1	78.0	75.2	83.4	161.5	125.3	115.7	(110.9)	

[a] Source: This table was prepared by the NASA astronomy program staff for the express use of the Astronomy Survey Committee in early 1970 and updated in March 1972.

Whitford report, especially if we disregard the fiscal year 1968 peak. Moreover, the university percentage of the facilities budget has been reduced by more than a factor of 2 in recent years. The dollar support for research equipment at the universities has fallen by about a factor of 5. As noted above, caution should be used in interpreting this change, which may be a small fluctuation from the perspective of the decade. Furthermore, the large optical telescopes at Kitt Peak and Cerro Tololo and the radio telescopes at Green Bank are major tools for university scientists everywhere.

Facilities obligations, by their nature, fluctuate. At National Centers, the obligations for radio-astronomy instruments declined in recent years, reversing with the surface upgrading of Arecibo. A reduction in

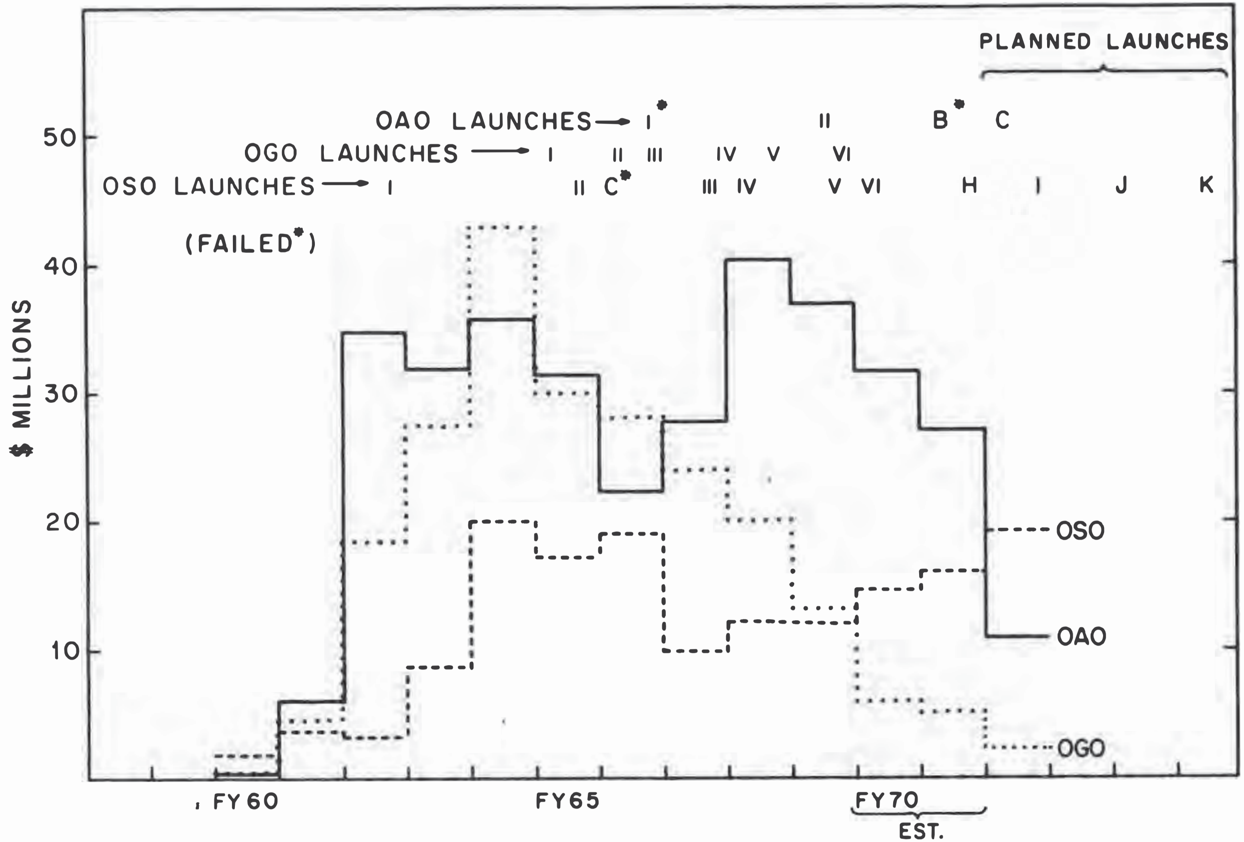

FIGURE 9.19 Annual budgets for the three Observatory series of spacecraft, shown with their launch schedules. OGO has been discontinued, and no OAO is presently planned beyond OAO-C. [Sources: NASA budget estimates and NASA *Pocket Statistics*.]

TABLE 9.31 Astronomy Research Support at NSF According to Various Definitions ($Millions)

	NSF "Basic Research in Astronomy" (Facilities Not Included)	NSF "Ground-Based" Astronomy and Astronomy-Related Research (Includes Funds from Phys. Sec., Math. Sec., etc.)	NSF "Astronomy Obligations" (Includes Institutional Support and Science Education Funding)	NSF Astronomy "Support" (Research Only)
FY55				0.36
56				0.53
57				4.34
58				5.2
59				10.8
60				4.2
61				9.9
62				12.2
63	9.10			13.4
64	10.2			14.3
65	11.0			18.0
66	15.6	22.9	24.5	20.7
67	14.6	22.7	21.3	20.0
68	16.5	30.4	29.7	26.5
69	23.0	28.6	27.4	24.7
70	(19.9)	(23.3)		21.9
71	(23.2)	(30.3)		28.4
72	(28.7)	(29.9)		(30.1)

Sources: *Column 2, Federal Funds for Research, Development, and Other Scientific Activities,* Vols. XIII–XX. *Column 3,* Reports to the Office of Science and Technology by NSF program staff, 1970. Updated by NSF staff. *Column 4,* NSF Annual Reports, NSF Databook–1969, obligations by discipline. *Column 5,* NSF astronomy program staff, May 1971.

the percentage allocated to radio facilities occurred in capital expenditures at universities, shown in Figure 9.21. Radio astronomy had been receiving the major fraction of a smaller capital investment until recently.

NSF Basic Research Grants in astronomy (mostly awarded to university groups) held fairly level at about $6 million per year for eight years, with a recent rise. But the money available for facilities at the universities, the modern astronomical instrumentation needed to maintain an advanced research program, has fallen an order of magnitude from 30% to 3% of the Basic Research Grants amount (last line Table 9.34).

A lack of growth in Basic Research Grant funds from fiscal year 1966 to fiscal year 1971, coupled with a significant increase in the numbers of excellent proposals submitted to NSF (caused by rapid growth of interest in the subject and new PhD's in astronomy) may lead to a critical

TABLE 9.32 NSF Obligations for Astronomy ($Millions)[a]

		Fiscal Year									
		1964	1965	1966	1967	1968	1969	1970	1971	1972	1973
Basic research grants	Total	4.3	4.5	6.8	5.8	6.2	6.8	5.8	6.4	(8.0)	(8.8)
	Radio				[1.3]	[2.1]	[3.0]	[2.1]			
Specialized research facilities	Total	0.65	1.8	1.4	1.9	0.07	0.3	0.2	0.2	(0.3)	[b]
	Radio				[0.43]	[0.08]	[0.04]				
National Astronomy Observatories[c]	Total	10.0	12.3	13.1	13.3	23.9	19.7	17.2	24.0	(22.9)	(26.2)
	Radio	[4.6]	[3.4]	[4.7]	[5.0]	[7.9]	[8.2]	[6.4]	[13.0]	[11.1]	[13.6]
SUBTOTAL		[14.9]	[18.6]	[21.3]	[21.0]	[30.2]	[26.8]	[23.2]	[30.6]	[31.2]	[35.0]
Astronomy research support											
Institutional support		(2.6)	(2.6)	2.6	3.1	2.4	2.4	2.2	1.7	(1.7)	(1.7)
Science education		(0.75)	(0.75)	0.75	0.75	0.81	0.6	0.6	0.4	(0.2)	(0.0)
TOTAL		(18.3)	(21.9)	24.7	24.9	34.0	29.5	26.0	32.7	(33.1)	(36.7)

[a] Source: NSF budget documents: "Justification of Estimates of Appropriations."
[b] "University facilities" is scheduled to disappear as a budget line item in fiscal year 1973, to be incorporated in basic research grants.
[c] The four astronomy centers (AIO, CTIO, KPNO, and NRAO) and the High Altitude Observatory, a solar observatory run by NCAR—see Table 9.33 for details.

TABLE 9.33 NSF National Research Center Budgets–Including Construction of Facilities ($Millions)[a]

	Fiscal Year										
	1963	1964	1965	1966	1967	1968	1969	1970	1971	1972	1973
Arecibo Ionospheric Observatory[b] (AO) (now NAIC)							0.9	1.55	6.1	(4.0)	(3.3)
Facilities									[3.8]	[1.8]	[0.30]
Cerro Tololo Inter-American Observatory (CTIO)	1.00	1.00	1.38	1.43	1.71	2.32	4.55	1.90	2.28	(2.50)	(2.70)
Facilities		[0.35]	[1.01]	[0.69]	[0.69]	[1.50]	[3.45]	[0.36]	[0.31]	[0.40]	[0.32]
Kitt Peak National Observatory (KPNO)	3.75	4.40	6.92	5.79	5.50	12.50	5.70	6.46	7.22	(7.72)	(8.22)
Facilities	[1.37]	[1.94]	[4.10]	[2.98]	[2.06]	[8.33]	[1.14]	[0.05]	[0.13]	[0.54]	[0.36]
National Radio Astronomy Observatory (NRAO)	4.55	4.60	3.38	4.72	4.98	7.86	7.28	5.86	6.90	(7.06)	(10.27)
Facilities		[1.30]	[1.57]	[1.80]	[1.39]	[0.87]	[0.48]	[0.68]	[0.0]	[0.08]	[3.02]
National Center for Atmospheric Research (NCAR)			7.80	11.50	11.40	11.80	11.04	11.38	14.76	(19.20)	(18.10)
Facilities			[1.15]	n.a.	[0.19]	[0.12]	[0.44]	[0.01]	[0.48]	[0.70]	[0]
High Altitude Observatory (part of NCAR)			[0.61]	[1.23]	[1.05]	[1.24]	[1.34]	[1.43]	[1.55]	[1.56]	[1.67]
SUBTOTAL	9.3	10.0	11.68	11.94	12.19	22.68	18.43	15.77	21.68	(21.3)	(24.5)
Four Astronomy Centers (AIO, CTIO, KPNO, NRAO)											
SUBTOTAL–CONSTRUCTION OF FACILITIES (ASTRONOMY CENTERS)		3.59	6.68	5.47	4.14	10.70	5.07	1.09	4.24	(2.82)	(4.00)
Percentage of Facilities		36%	57%	46%	34%	37%	28%	7%	20%	13%.	16%

[a] Source: NSF budget documents: "Justification of Estimates of Appropriations."
[b] Now National Astronomy and Ionosphere Center.

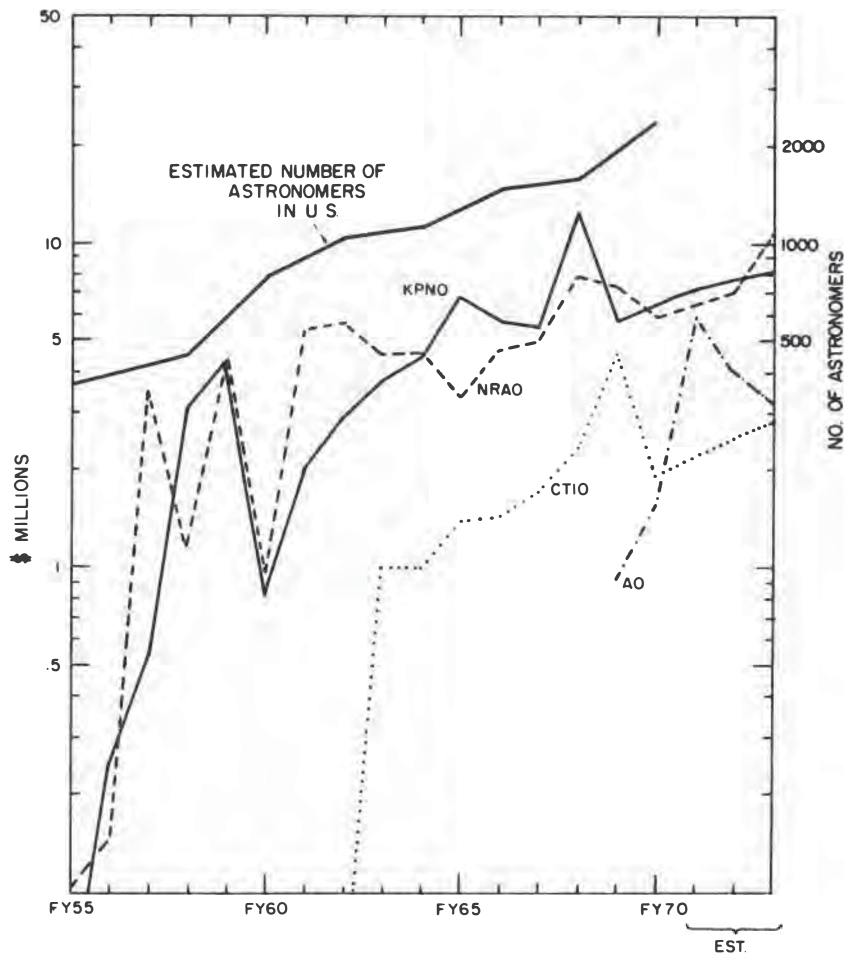

FIGURE 9.20 National Observatory budgets. [Source: NSF budget documents.]

funding situation. The NSF staff have reduced the average grant period from nearly two years to one year. In this way they have stretched the same amount of annual obligations to support a larger number of investigations. The result has been a steady reduction in the size of the average grant. A return to a two-year average grant period is desirable as soon as feasible. Hoped-for fiscal years 1972 and 1973 budget increases (see Table 9.32) may allow some breathing space to accomplish this.

An estimated two thirds of a million dollars annually in Basic Re-

TABLE 9.34 Obligations for NSF Astronomy Facilities–R&D Plant, Major Research Equipment ($Millions)[a]

	Fiscal Year								
	1963	1964	1965	1966	1967	1968	1969	1970	1971
Universities	1.6	0.6	1.8	1.4	1.86	0.66	0.32	0.19	0.2
% Radio	69%	74%	73%	66%	18%	20%	7%	n.a.	n.a.
National Centers		3.59	6.682	5.47	4.14	10.7	5.07	1.09	4.24
% Radio		36%	23%	33%	34%	8%	9%	62%	90%
TOTALS		4.2	8.4	6.9	6.0	11.4	5.4	1.3	4.4
% Radio		42%	38%	44%	28%	9%	9%	60%	85%
% Universities		15%	21%	20%	31%	6%	6%	15%	5%
Ratio of Facilities Budget to Basic Research Grants			0.40	0.21	0.18	0.11	0.05	0.03	0.03

[a] Sources: Universities, NSF Annual Reports and Astronomy Section staff estimates; National Centers, NSF staff and NSF budget documents.

search Grants from other NSF science sections (Physics, Atmospheric Sciences, Mathematics, etc.) support astronomy-related research.

E. DEPARTMENT OF DEFENSE RESEARCH IN ASTRONOMY

Branches of the Department of Defense (DOD) provide about 3% of the federal funds for basic research in astronomy reported by NSF to the Congress and about 6% of the combined basic and applied research funds. The basic research figure has been essentially constant since the Whitford report (fiscal year 1964) at between $8 million to $10 million per year. Astronomy accounts for 3 to 4% of the DOD total obligation for basic research. This assessment, from the *Federal Funds for Research, Development, and Other Scientific Activities* series, is in substantial agreement with the DOD budget line item known as "Astronomy and Astrophysics" through fiscal year 1970, as can be seen from Tables 9.35 and 9.36. However, the drop in astronomy and astrophysics in fiscal year 1971 was by far the largest single decrease in the ten or so items included in DOD basic research.

Basic research obligations are estimated to account for less than half of the DOD "support" for astronomy, according to the reports to the Office of Science and Technology by the NSF (see Table 9.37). An important example of astronomical research and DOD applied research (and exploratory development) was the building and operation

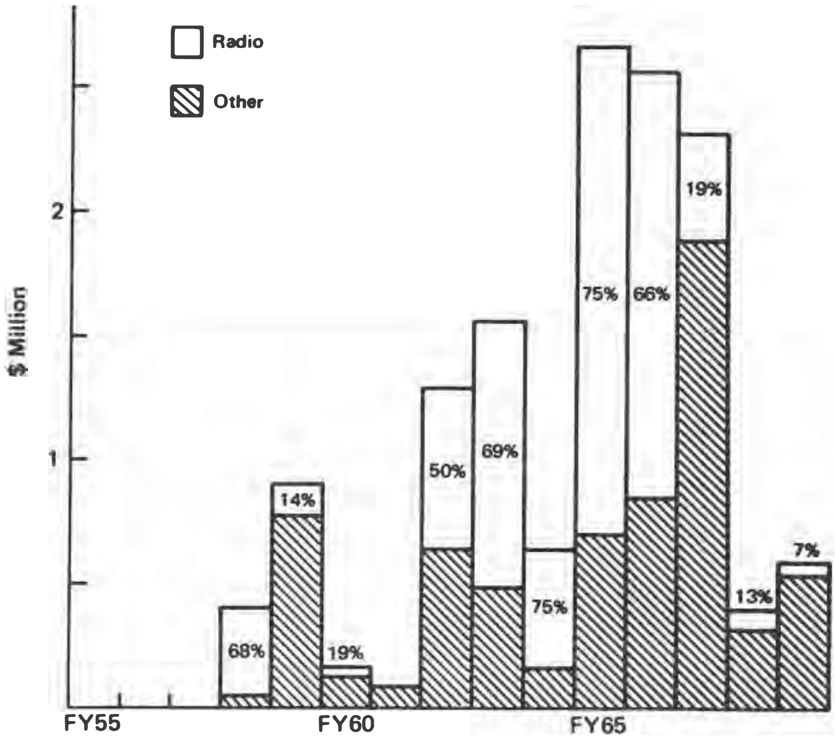

FIGURE 9.21 NSF university astronomy facilities expenditures (obligated amounts have been divided equally among the grant years to get an estimate of the amounts spent). [Source: NSF Astronomy Section staff.]

TABLE 9.35 DOD Basic and Applied Research in Astronomy ($Millions)[a]

	Fiscal Year								
	1963	1964	1965	1966	1967	1968	1969	1970 (est.)	1971 (est.)
Basic	4.41	9.25	8.28	8.92	8.08	9.48	9.62	9.38	8.69
Applied						17.4	7.81	8.44	8.64
TOTAL						26.9	17.4	17.8	17.3

[a] Source: *Federal Funds for Research, Development, and Other Scientific Activities.* Vols. XIII–XIX.

TABLE 9.36 DOD Budget Line #6.1 "Basic Research–Astronomy and Astrophysics"

Air Force	6.421	6.304	6.457	6.669	6.422	5.997	3.792
Navy	2.409	2.509	2.606	3.892	3.864	3.434	2.895
Army	0.025						
ARPA	8.855	8.813	9.063	10.561	10.286	9.431	6.687

[a] Source: DOD budgets.

($2 million per year) of the Arecibo Observatory by the Advanced Research Projects Agency (ARPA). Table 9.36 shows that ARPA reported no spending for astronomy and astrophysics basic research in the DOD budget line #6.1. Now that the NSF will be operating Arecibo as a National Observatory, devoted to similar research, it will be reported to the Congress as $2 million per year for basic research. Unfortunately, this will not represent an increase in funding for basic research.

Figure 9.22 indicates approximate agreement between the NSF estimates for the Office of Science and Technology (OST) and DOD budget lines. However, a major omission in the OST reports is space astronomical research by the DOD. Our panel has also been unable to make a reasonable estimate of DOD space research. Because of military classification, astronomical space research is a small, but unknown, fraction of a large research effort.

A more comprehensive attempt to identify astronomical research in the DOD would have to look carefully at the following programs:

USAF

Systems Command (Lincoln Laboratory Aerospace Corp.)

Office of Aerospace Research (AFCRL, Sacramento Peak, universities, Discoverer satellites)

TABLE 9.37 DOD Support of "Ground-Based" Astronomy[a]

Air Force	9.6	12.1	8.5	8.0	7.3
Navy	8.7	8.6	8.2	7.4	6.7
Army					
ARPA	2.6	2.8	2.7	1.7	1.3
Ground-Based Astronomy	20.9	23.5	19.4	17.1	15.3

[a] Source: OST reports for 1966, 1967, and 1968.

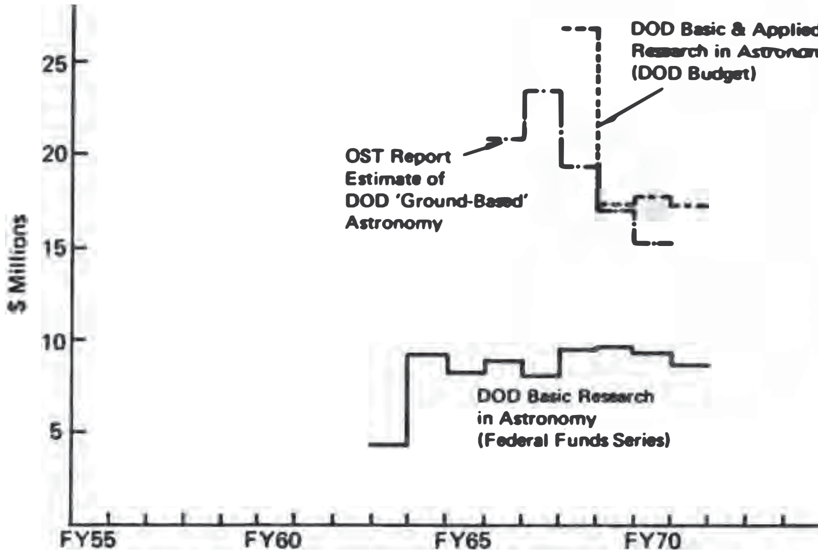

FIGURE 9.22 DOD support of astronomy. [Sources: *Federal Funds for Research, Development, and Other Scientific Activities,* Vols. XIII–XIX; Office of Science and Technology reports; DOD budget.]

USN
- Office of Naval Research
- Naval Research Laboratory, Hulburt Center for Space Research
- Naval Observatory
- Bureau of Naval Weapons (SOLRAD Satellites)

ARPA
- Project Defender (Haleakala, Arecibo) (now Strategic Technology Office)
- Project VELA of Nuclear Test Detection Office (X-Ray Satellites, universities)

Such a study might further reveal state-of-the-art military instrumentation that could be declassified for astronomical research. Economies in and speed of application of advanced DOD instrumentation might result.

F. OTHER FEDERAL AGENCIES SUPPORTING RESEARCH IN ASTRONOMY

Among the other federal agencies supporting research in astronomy the largest contribution (1%) comes from the Smithsonian Institution, whose budget includes support for the Smithsonian Astrophysical Observatory (SAO) and for some astronomy-related research at their Radiation Biology Laboratory. The Smithsonian contribution to basic research in astronomy, as reported by the NSF to the Congress is compared with the Smithsonian's direct support of the SAO in Table 9.38.

The National Bureau of Standards and the Environmental Science Services Administration have recently averaged $1 million per year for support of astronomy basic research according to the *Federal Funds for Research, Development, and Other Scientific Activities* series. Some of these activities will appear in the future in the budget for the new National Oceanic and Atmospheric Administration.

Finally, we should mention the astronomy research funded by the Atomic Energy Commission (AEC). Although the AEC does not report any basic or applied research in astronomy to the Congress through the NSF *Federal Funds* series, a significant amount of astronomy research is carried out at the AEC's Los Alamos Scientific Laboratory, at the Sandia Corporation, and in its rocket program at Lawrence Radiation Laboratory.

G. SOME GENERAL COMMENTS ON NSF, NASA, DOD, AND SI SUPPORT OF GROUND-BASED OBSERVATIONAL ASTRONOMY

Of the ten largest optical telescopes in the United States only four have been completed in the last decade. Of these, two were supported primarily by NASA and state funds (the 107-in. at the University of Texas and the 88-in. at the University of Hawaii), one was supported by the NSF (the 84-in. at KPNO), and one was supported by the NSF and state funds (the 90-in. at the University of Arizona). Three more large

TABLE 9.38 Smithsonian Institution Astronomy Research Budgets ($Millions)[a]

	Fiscal Year						
	1966	1967	1968	1969	1970	1971	1972
Basic research in astronomy	1.313	2.735	2.870	3.268	3.195	(3.560)	(4.560)
SAO[b]	1.169	1.686	1.784	1.898	2.086	(2.107)	(2.655)

[a] Sources: *Federal Funds for Research, Development, and Other Scientific Activities*, Vols. XIII–XIX, Tables C-36, C-37, C-38; SAO administrative staff.

[b] This line is not the entire SAO budget, since the Smithsonian Institution is not the only agency that supports astronomical research at the SAO.

telescopes are being built, two by the NSF (158-in. at KPNO and CTIO) and one by a private donor (100-in. at the Carnegie Institution's Southern Observatory). An unusual multiple mirror array of optical telescopes supported by the Smithsonian Institution is being built at SAO.

In the past, the NSF was the primary federal supporter of university ground-based astronomical research: most of the recent Foundation effort in large optical telescopes has been at the National Observatories. NSF's only other recent major telescope construction for a university is a 60-in. telescope completed in 1971 at the Hale Observatories. Although NASA is primarily involved in space-based research, and its funding for university research in general has been cut back by the Office of Management and Budget, NASA has constructed large astronomical telescopes at universities in support of its planetary exploration program. With the many small instruments supported by NSF included, NASA has provided an average of 60% of the federal funds for university-based optical telescopes and NSF an average of 40%, with the DOD less than 1% in recent years.

The principal large steerable paraboloid radio telescopes that have come into operation since the Whitford report are the 210-ft Goldstone antenna (NASA), which is partially available for astronomy (up to 5% of time), the Owens Valley 130-ft (NSF), and the modest Illinois 120-ft (NSF). Only the Illinois telescope was planned since the Whitford report. No other large paraboloid has been planned and approved for funding since the Whitford report.

A number of small accurately figured radio telescopes working at millimeter wavelengths have been built since the Whitford report. The DOD was the major supporter of these instruments and built the first four or five of them. Since then, the NSF has built one (36-ft) on Kitt Peak for the NRAO and has partially supported one (20-ft) at Berkeley, and NASA has built one (18-ft) at JPL.

Some very large radio telescopes have been recently phased out of operation by the DOD and are being supported by the NSF. Among these are the Arecibo 1000-ft fixed dish and the Haystack 120-ft antenna. NASA has increased the availability of the Goldstone antenna for radio astronomy and is constructing two additional 210-ft antennas as part of its deep-space tracking network. These will also be partially available for radio- and radar-astronomy studies.

In radio astronomy, the DOD has been the major support of the costs of telescope construction. NSF has supported construction largely at the national observatories. NASA has built a few radio telescopes, but unlike the two large optical telescopes it supported, they are not controlled by universities.

Table 9.39 summarizes the contents of a series of three annual reports (for fiscal years 1966, 1967 and 1968) prepared by the NSF for the White House Office of Science and Technology on the status of "ground-based" astronomy. These reports were prepared at the request of the OST, who designated the NSF the "lead agency" in ground-based astronomy. It is not clear whether such reports are still desired by the OST, but if they are to continue, it would be important for NASA to prepare comparable reports on space-astronomy research by various agencies, since it is the "lead agency" for space astronomy. For example, NSF support for space-astronomy investigations (ultraviolet rocket experiments at KPNO, the Stratoscope Balloon Telescope, and, in earlier years,

TABLE 9.39 Federal Support of "Ground-Based" Astronomy, Including Construction of Facilities ($Millions)[a]

	Fiscal Year						
	1966	1967	1968	1969	1970	1971	1972
NSF	22.9	22.7	30.4	26.4	23.2	(30.3)	(29.9)
Radio	[6.4]	[7.6]	[7.6]	[10.3]	[9.6]		
Radio %	28%	33%	25%	36%	44%		
NASA	9.4	10.0	10.0	(10.0)	(9.0)	(8.0)	(9.0)
Radio	[0.8]	[0.5]	[0.5]	[0.5]	[0.5]		
Radio %	(8%)	(5%)	(5%)	(5%)	(6%)		
Air Force	9.6	12.1	8.5	(8.0)	(7.3)	(4.0)	(2.0)
Radio	[1.2]	[6.4]	[3.7]	[3.4]	[3.1]		
Radio %	12%	53%	44%	(43%)	(42%)		
Navy	8.7	8.6	8.2	(7.4)	(6.7)	(4.0)	(5.0)
Radio	[1.9]	[2.0]	[1.4]	[1.3]	[1.1]		
Radio %	21%	23%	17%	(18%)	(16%)		
ARPA	2.6	2.8	2.7	(1.7)	(1.3)	(0)	(0)
Radio	[1.2]	[1.2]	[1.2]	[0.3]	[0]		
Radio %	46%	43%	44%	(18%)	–		
DOD SUBTOTAL	[20.9]	[23.5]	[19.4]	[17.1]	[15.3]	[8.0]	[7.0]
SI	1.1	1.5	1.6	1.7	1.9	1.9	(2.4)
Radio	0	0	0	0	0		
TOTAL	54.3	57.7	61.4	55.2	49.	(48.2)	(48.3)
Radio	[11.5]	[17.7]	[14.4]	[15.8]	[14.3]		
Radio %	21%	31%	23%	28%	29%		

[a] Sources: OST report and NSF Astronomy Program staff (1970), Smithsonian Institution numbers are from SAO staff (1972). Other numbers for fiscal years 1971 and 1972 are rough estimates by NSF staff with little information available directly from NASA or the DOD.

NRL's Hulburt Center) has averaged about $1 million annually in recent years.

H. AGENCY ASTRONOMY FUNDING OBLIGATIONS COMPARED WITH AMOUNTS REPORTED AS RECEIVED BY U.S. ASTRONOMICAL INSTITUTIONS

Of the $270 million support reported for basic research in astronomy in fiscal year 1971 only a fraction could be expected to appear in questionnaires returned from the survey of astronomical institutions (Section I) because of three areas in the NASA budget. First, the $100 million in "indirect" NASA costs (see Table 9.23) would not appear as part of any observatory budgets. Second, costs of the launch vehicle and the spacecraft—the outer shell that houses and points astronomical experiments—are generally not included in NASA contracts to scientific institutions; rather, they are contracted directly to the aerospace industry. Launch-vehicle costs have averaged over $10 million per year for astronomical earth-orbital missions. It is difficult to estimate accurately the breakdown between spacecraft costs and experiment costs. In a typical sounding-rocket experiment, for example, the rocket itself may cost between $90,000 and $200,000, the pointing controls between $60,000 and $90,000, and the experiment itself between $50,000 and $100,000. Only the experiment budget is likely to be contracted through an astronomical institution included in our survey. Similar considerations apply to the orbiting observatories and to the Apollo Telescope Mount on the Skylab earth-orbiting space station mission. Thus less than half of NASA's direct reprogrammable astronomy support (see Table 9.30) could be expected to appear in the institutional survey. Table 9.40 summarizes the results.

I. FEDERAL FUNDING SUPPORT PER U.S. ASTRONOMER

As predicted by the Whitford Panel on Astronomical Facilities, despite the fulfillment of all but one of its goals for large optical telescopes, there are fewer square inches of telescope mirror collecting area per astronomer today than there were a decade ago. The situation is more extreme in radio astronomy. This decreasing observational capability per astronomer is caused mostly by the increase of PhD's working in astronomy. Young people have been entering astrophysical research because astrophysics today presents some of the most exciting unsolved problems in the physical sciences. What kind of support can they expect to find for their research endeavors in the near future?

TABLE 9.40 Comparison of Federal Agencies' Reported Astronomy Obligations with Federal Funds Reported as Expended by U.S. Astronomical Institutions ($Millions)[a]

	Fiscal Year		
	1969	1970	1971
Obligated by NASA	125.	116.	111.
NASA funds reported expended by institutions	54.4	55.7	52.4
Obligated by NSF	29.5	26.0	32.7
NSF funds reported expended by institutions	28.0	26.7	37.7[b]
Obligated by DOD	17.4	17.8	17.3
DOD funds reported expended by institutions	12.3	12.5	10.7

[a] Sources: *Federal Funds for Research, Development, and Other Scientific Activities*, NASA, Table II.9; NSF, Table III.2; DOD, Table IV.1. Institutional Expenditures, from Section I, Appendix B.
[b] Expenditures, particularly for large facilities, may include money obligated several years earlier.

Probably the single most significant measure is simply the overall financial support per astronomer. Although an average number like this can be criticized, and its meaning for any individual astronomer questioned, its trend with time contains information about the present and future vitality of the field. A few cautionary notes are in order. Such a number is clearly not the astronomer's salary, which incidentally averaged about $15,000 in 1970. It is not money spent at the discretion of astronomers alone, since we are including part of the costs of the large government programs that have important reasons for existing besides astronomical research. Finally, it is not money spent in ways incomprehensible to the layman. The money is primarily expended for the services of people, highly skilled technical help, as well as maintenance men, working alongside the astronomer at his observatory, high-technology engineers in the advanced industries that supply research equipment as well as secretaries and printers at the publishing offices of research journals, people in many walks of life who back up each PhD astronomer and make his work possible.

Figure 9.23 shows the result of dividing the total federal funds for research in astronomy (NSF National Register corrected), adjusted for inflation to constant 1958 dollars, by the number of PhD astronomers (NSF National Register corrected) taken from the preceding section (Table 9.18) of this Statistical Panel report. The support per astronomer has declined from a maximum of $366,000 (1958 dollars) per astronomer at the time of the Whitford report (fiscal year 1964) to the recent level of $170,000 (1958 dollars) per astronomer (fiscal year 1971). Non-

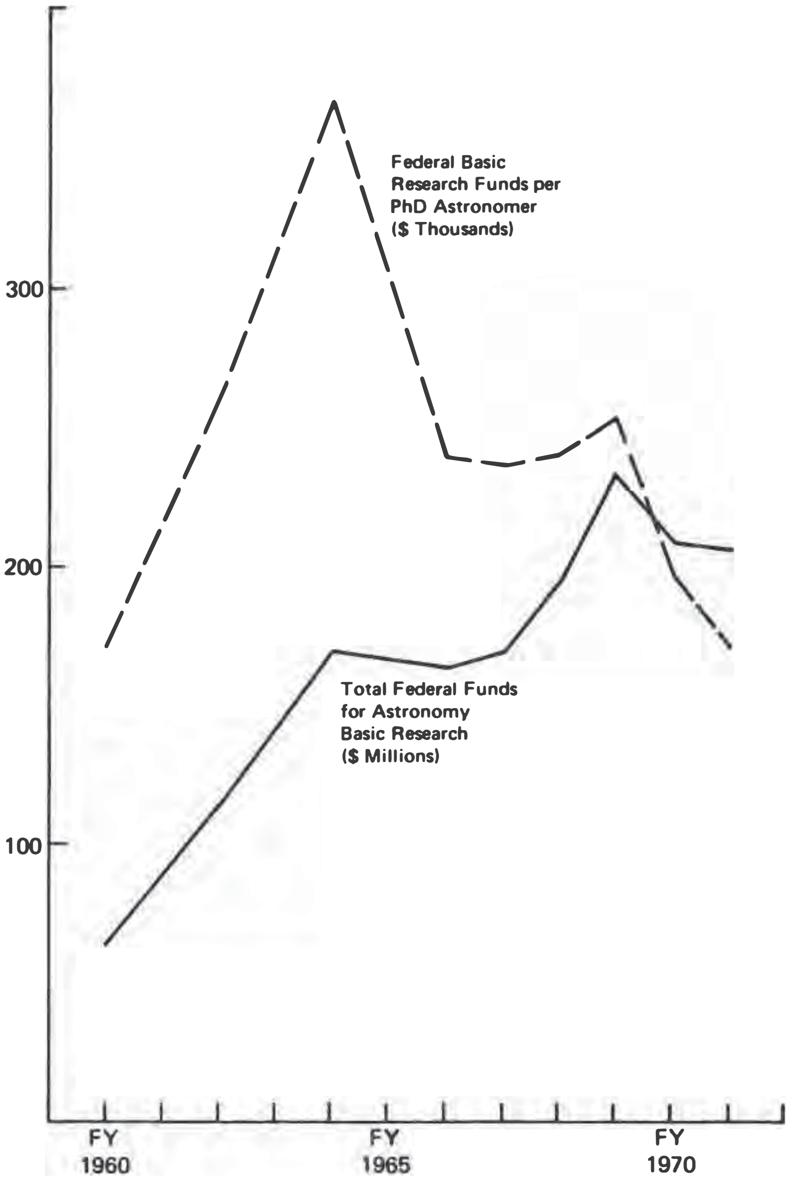

FIGURE 9.23 Federal basic research funds per PhD astronomer in 1958 dollars (inflation has been removed). [Sources: *Federal Funds for Research, Development, and Other Scientific Activities*, Vols. XIII–XIX; number of PhD astronomers (NSF National Register corrected) from Table 9.17 of this report; implicit price deflator for the GNP from Economic Report of the President, February 1970, Table C-3.]

federal funds added another $21,000 (in 1958 dollars) per astronomer for the fiscal year 1970. This figure shows a much more dramatic decline than is apparent in Figure 9.16 in Section II. This arises from including here the inflationary correction and counting only PhD astronomers, whose rate of increase was greater than that of total astronomers.

The total federal funds per PhD astronomer includes expenditures that do not appear in the budgets of U.S. astronomical institutions, e.g., spacecraft, launch vehicles, and tracking networks. A funding figure representing the annual expenditures more or less directly at the discretion of astronomers was determined from the Institutional Survey (Section I, Tables 9.3 and 9.8). It includes the total budgets of U.S. astronomical observatories, university astronomy departments, and the astronomy sections of various federal and private laboratories and consists of federal funds and funding from state, local, and private sources. If we divide this figure for fiscal year 1970 by the number of PhD astronomers that year we find $119,000 per astronomer (in current dollars).

We can further attempt to separate the observational astronomers from the theoreticians and other categories. Then we can divide the space observations budget by the space observers, the ground-based astronomy budget by the ground-based observers, and so forth. We use the FTE (full-time equivalent) distribution of interest reported above in the manpower survey (Section I, Appendix I. A, Item G) to estimate the FTE number of observers. Of the 1056 PhD astronomers in 1970, we estimate that there were 339 ground-based observers, 128 space-based observers, 187 theoreticians, and 61 laboratory astrophysicists. Of the 341 remaining, 150 were engaged in university instruction.

According to the estimate in Table 9.39, the federal government spent $49 million on "ground-based" astronomy in fiscal year 1970. State, local, and private sources supplied another $26 million, according to the Institutional Survey (Tables 9.7 and 9.8), that was not spent for space astronomy. By simply dividing this $75 million in "nonspace" funding by the 926 "nonspace" PhD astronomers, we find about $81,000 per "nonspace" astronomer. However, this cost is somewhat illusory, since laboratory astrophysicists and theoretical astrophysicists are partially supported by "space" funding sources. When these sources are considered, and when the low per capita cost of university instruction is included, it leads to an increased estimate of about $100,000 to $125,000 per "ground-based observational" astronomer.

As expected, a larger figure results if we divide space astronomy funding by the space observers only. Reducing the space-astronomy budget (Table 9.30) by the NASA amount that was included above in "ground-based" astronomy, we have about $105 million divided by 128 observers

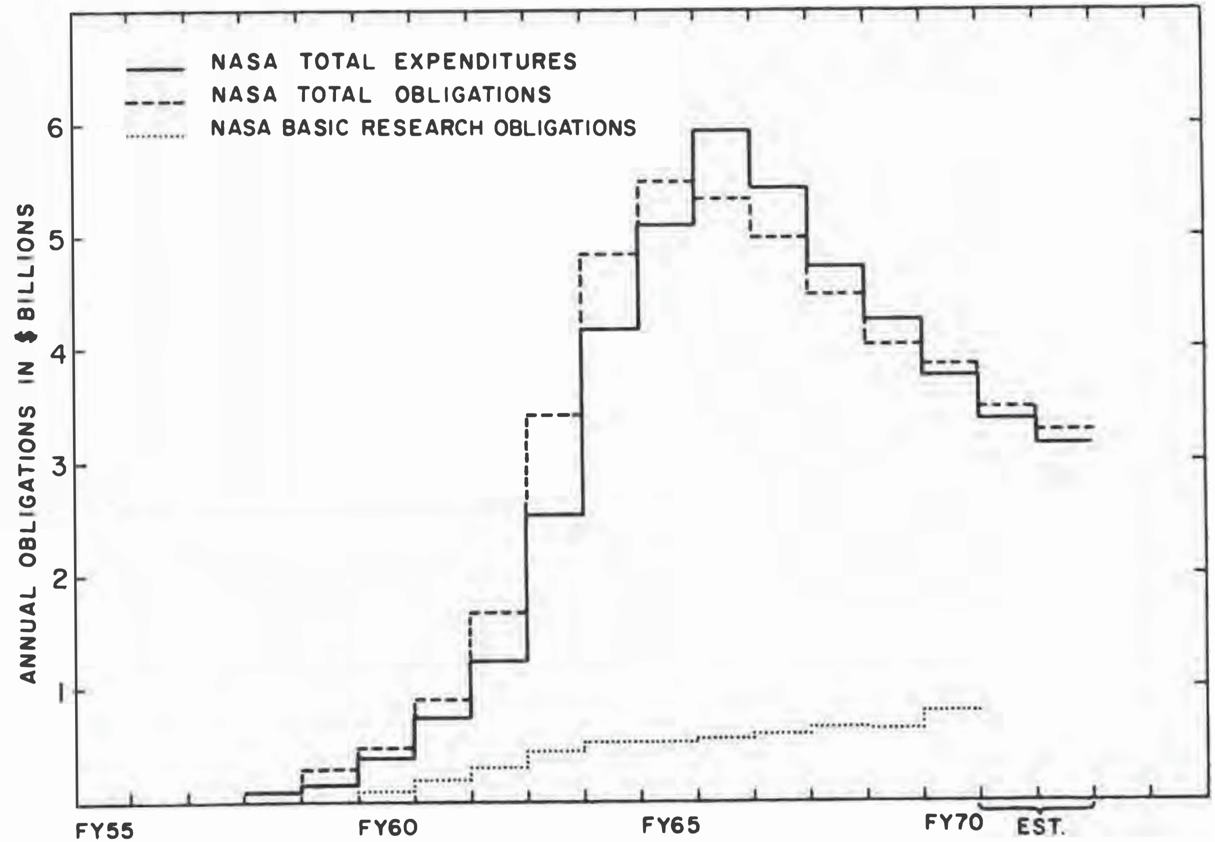

FIGURE 9.24 NASA total expenditures, total obligations, and basic research obligations. [Sources: Fiscal years 1958–1968 from *Federal Funds for Research, Development, and Other Scientific Activities*, Vol. XIX, Tables C-88, C-89, and C-94; fiscal years 1970–1972 from Obligations and Expenditures, Special Analyses of the FY72 Budget, pp. 273–274.]

or $820,000 per space observational astronomer. If we reduced our space-astronomy funding figure to include only that amount spent directly by astronomical institutions, as described above in Section G, we would divide $46 million in fiscal year 1970 by 128 observers and find $360,000 per space observational astronomer. These costs must be considered within the context of the all-wavelength capability of space techniques and the fundamental importance of phenomena that can be studied only from space.

J. ASTRONOMY FUNDING IN THE CONTEXT OF TOTAL FEDERAL FUNDS FOR RESEARCH

Federal support for astronomy must be seen in the light of a general slowdown in federally supported research and development since fiscal year 1967. When inflationary cost increases are included, the R&D budget, approximately level in current dollars, becomes a significant declining budget in constant dollars. Using the implicit price deflator for the gross national product,* the decline in real R&D funding has been about 5% per year since fiscal year 1967, or 3% per year since fiscal year 1964.

Moreover, since a major fraction (between 70 and 80%) of the total federal support for astronomy comes from NASA, we should take into account the even more rapid decline in total NASA obligations (about 7% per year in current dollars, or 10% per year in constant dollars) since fiscal year 1965, the peak year of spending associated with the Apollo goal (Figure 9.24). Indeed, if NASA obligations are excluded from the federal total R&D, the significant decline since fiscal year 1964 disappears, and obligations for federally funded R&D become essentially level in constant dollars.

The budget picture is less severe if we look only at "basic research," which accounts for about 90% of all federal funds for astronomy. Federal obligations for basic research have monotonically increased since fiscal year 1954, with an average annual rate since fiscal year 1964 of 7% in current dollars (or about 4% in constant dollars).

While the overall NASA budget peaked in fiscal year 1965 and has been declining rapidly since then, basic research in NASA has never declined and has averaged the same annual rate of growth since fiscal year 1964 (7.4%) as total federal basic research (7.4%) or the National Science Foundation (7.3%). This, however, represented a significant deceleration relative to past NASA performance; from fiscal year 1956 to

**Economic Report of the President* (U.S. Govt. Printing Office, Washington, D.C., Feb. 1970), Table C-3.

fiscal year 1964, while total federal basic research was increasing at 29% per year and NSF at 39%, NASA basic research was increasing 59% per year (see Table 9.41) Nevertheless, NASA management fortunately has been quite successful in protecting its basic research budgets from sharing the Agency's general cutback. Anyone interested in the impact of the Apollo goal on the sciences must take this into account.

Recent trends in the NSF budget for basic research have been similar to NASA basic research, as noted in Table 9.41. The basic research budget of the Department of Defense also has grown since fiscal year 1964 but only at about half of the rate of NASA and the NSF.

A most serious aspect of the science funding picture has been investment in capital facilities. The federal funding agencies, with private institutions, have become increasingly reluctant or unable to expand physical plant since fiscal year 1966. Federal funds for R&D plant have declined 7% per year since fiscal year 1964 in current dollars, or about 10% per year in constant dollars. This may be related to the all-time high costs of construction and of interest rates. The falloff by a factor of 15 in NASA facilities might be interpreted as Apollo-related, but it should also be remembered that a part of the NASA facilities budget went to construct buildings for "space-related" research centers—many on university campuses. This program, created when James Webb was NASA administrator, indirectly supported university research efforts without appearing in NASA's basic research budget. This effort has been terminated along with the Sustaining University Program, NASA Traineeships and Fellowships, and other similar university-based programs. See Figure

TABLE 9.41 Average Annual Percentage Growth Rates in Various R&D Budgets (Obligations)[a]

	Inflation in the GNP	Total Federally Funded R&D, and R&D Plant	Total NASA (NACA pre-1958)	Total NSF	"Basic" Research		
					Total	NASA	DOD
FY 56–FY64	1.8%	21%	71%	39%	29%	59%15	15%
FY64–FY70	3.5%	2%	–4%	7.3%	7.4%	7.4%	4.3%

[a] *Sources: Federal Funds for Research, Development, and Other Scientific Activities,* Vol. XVIII, p. 5 and Tables C-88, C-89, and C-94. Growth rate is x where, e.g.,

$$(1+x)^6 = \frac{\text{FY70 Budget}}{\text{FY64 Budget}} .$$

9.25. NASA's obligations to universities are now entirely research obligations.

Despite the overall similarity between the NASA and NSF R&D plant curves in Figure 9.26, an important difference exists between what NASA and NSF mean by "facilities." The NASA facilities (R&D plant) budget consists almost entirely of the costs of "bricks and mortar," i.e., the direct costs of construction of buildings. Even scientific research instruments–e.g., the MacDonald 107-in. telescope–are not regarded as facilities by NASA; they are included in "basic research."

The reverse is true at NSF. First, most of the NSF grants for scientific research equipment are budgeted under R&D plant. The NSF research budget includes salaries for grant personnel and operations costs, such as computing time, but all initial capital expenditures for major instruments are called facilities. Second, little of the NSF facilities budget consists of

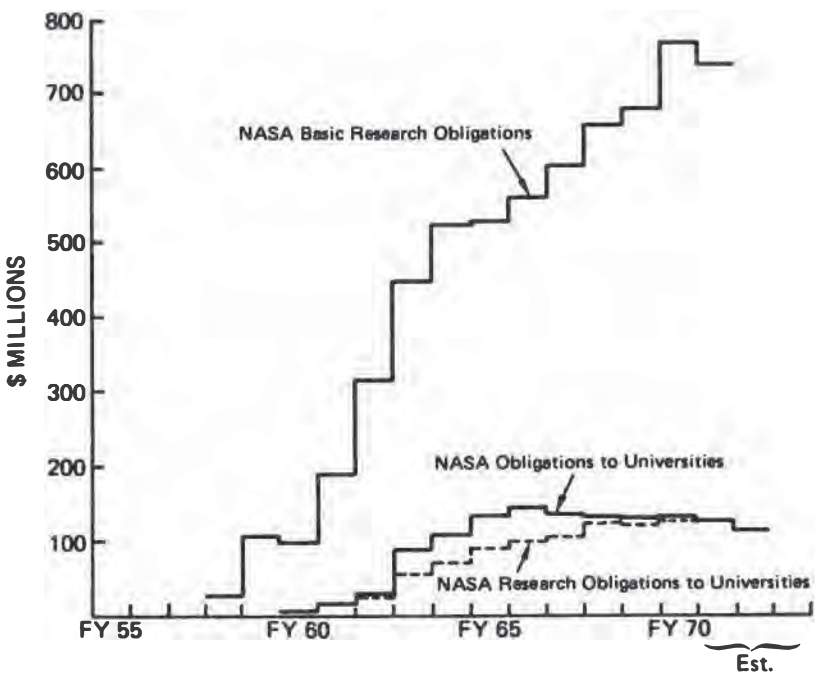

FIGURE 9.25 NASA research obligations and the university component. Institutional support broadly aimed at increasing the space-related capability of U.S. universities is now terminated. [Source: *Federal Funds for Research, Development, and Other Scientific Activities*, Vol. XIX, and NSF Publ. 70-27.]

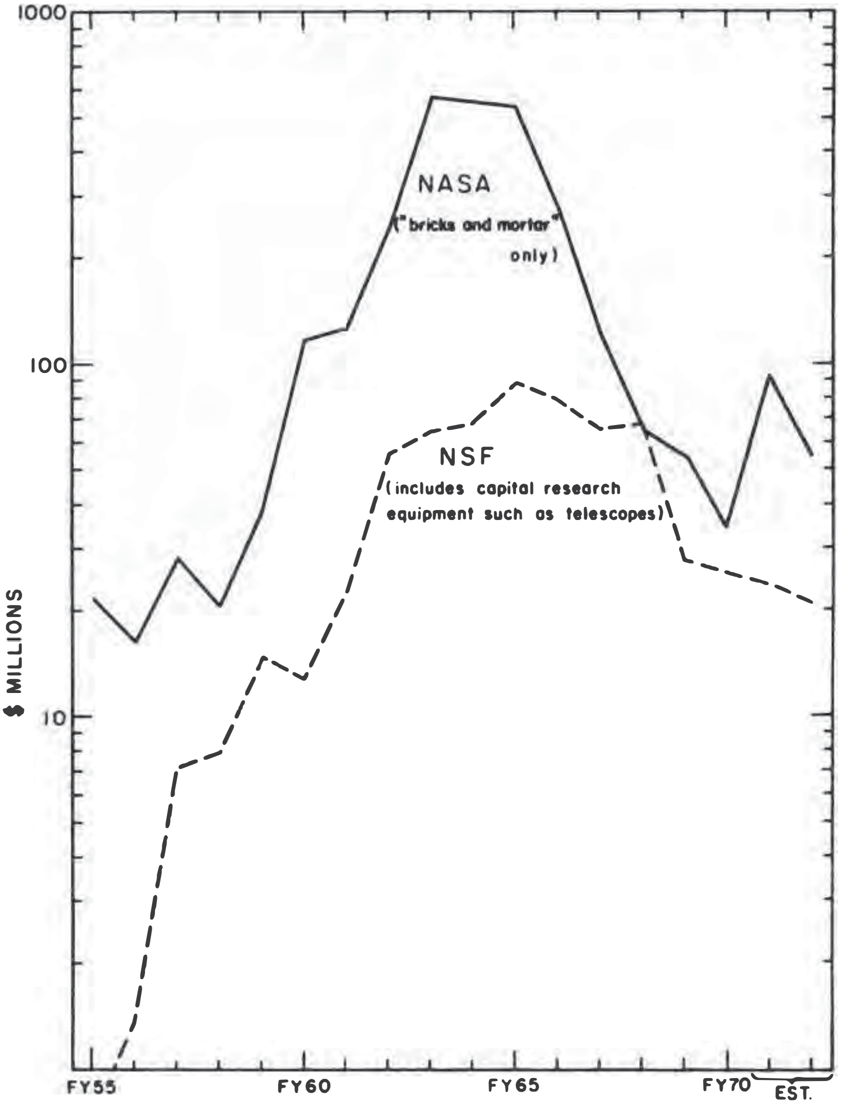

FIGURE 9.26 Federal obligations for R&D plant (facilities) for NASA and NSF. [Sources: *Federal Funds for Research, Development, and Other Scientific Activities,* Vol. XIX, Table C-9, and Special Analyses of the FY72 Budget, p. 274.]

bricks and mortar, because the NSF requires partial support of a project by the grantee institution, often in the form of contributing the buildings and land. NASA also requires this kind of partial support from the grantee, so that essentially none of NASA's R&D plant is astronomical-research-related.

The decrease in NSF facilities budgets has been accompanied by cutback or elimination of NSF programs whose specific function was capital equipment. The Graduate Science Facilities program was eliminated in the fiscal year 1971 budget, after it had spent about $200 million in the 1960's to finance construction projects valued, *with matching funds,* at more than half a billion dollars. Of comparable magnitude in the 1960's the Specialized Research Facilities and Equipment program was reduced to $5.8 million in fiscal year 1971, down from $19 million in fiscal year 1968, and it is now to be eliminated as a separate astronomy budget line item starting fiscal year 1973 .

Unlike the NASA budget; the DOD budget apparently conforms to the (NSF) Federal Funds guidelines and does not include capital research facilities such as radio telescopes in its basic research budgets.

K. TRENDS IN ASTRONOMY FUNDING

The fiscal environment in which the Whitford Panel formulated its ten-year program for ground-based astronomy differed markedly from the present one in which the Astronomy Survey Committee worked. The years of preparation of the Whitford report (1963–1964) came at the end of an era when basic research budgets had been climbing 20% per year, NSF budgets 39% per year, and NASA basic research budgets 59% per year.

The growth rate in current dollars of federally funded astronomy in the period since the Whitford report (fiscal years 1964–1970) has been essentially the same (7.5% per year) as basic research generally (7.4% per year). A closer inspection shows that the growth rate of basic research budgets decelerated further during the fiscal year 1967–1970 period as total R&D budgets actually declined somewhat.

Astronomy funding reversed that trend, growing at a somewhat slower rate (2% per year) than basic research generally (9% per year) in the fiscal years 1964–1967 period, and then growing at a faster rate (13% per year) than all basic research (6% per year) in the period fiscal years 1967–1970 (Figure 9.27). In recent years basic research in astronomy has been a marginally larger fraction of total federally supported basic

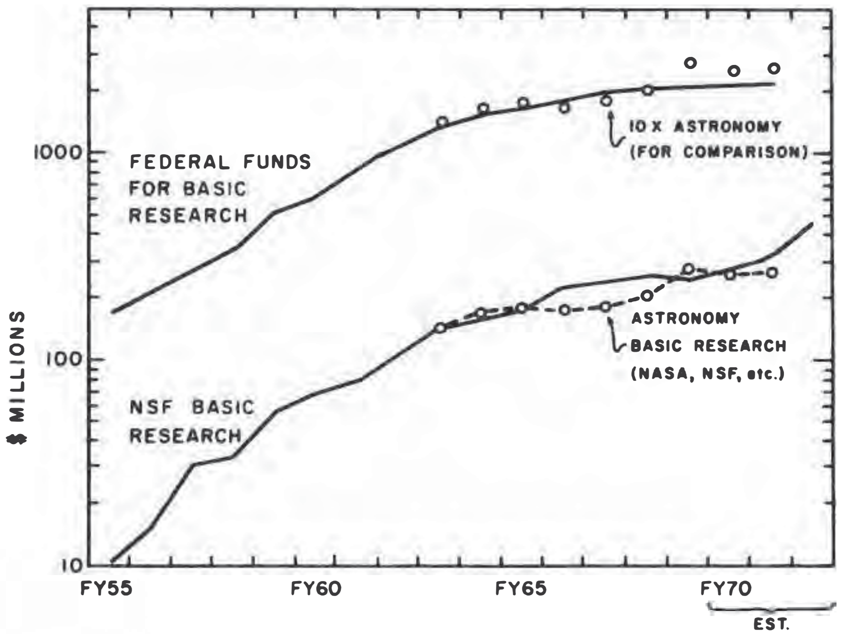

FIGURE 9.27 Federal obligations for basic research compared with astronomy basic research. [Source: *Federal Funds for Research, Development, and Other Scientific Activities*, Vol. XIX, Table C-94; astronomy numbers from Vols. XIII–XIX.]

research than it had averaged in the past few decades (12–13% of total basic research).

L. FUNDING HIGHLIGHTS IN CONCLUSION

In recent years, U.S. expenditures on frontier instrumentation for astrophysical research have risen so much that astronomy is regarded by many as a "big science." Costs for individual proposed items such as an Orbiting Astronomical Observatory ($100 million), a new 200-in. optical telescope ($25 million), a Very Large Array ($60 million), or the Apollo Telescope Mount ($200 million), are comparable with costs of the latest generation of particle accelerators. Moreover, the annual sum spent by the federal government in support of astronomy is comparable with the amount spent on the classic "big science"—high-energy physics.

Federal funding for astronomy continues to increase but at a rate significantly less than before the Whitford report, just slightly ahead of inflation, and considerably less than needed to offset increases in complexity of typical "frontier" instrumentation and techniques.

The Department of Defense has reduced its support for astrophysical research, although the NSF appears to be sponsoring most of the major installations dropped by DOD at the expense of freedom in starting its own new facilities. While overall NASA expenditures for astronomy continue to increase, "reprogrammable" or "direct" NASA astronomy spending has been declining since fiscal year 1968. In the same time period, indirect costs have risen 70%.

The percentage of NSF astronomy funds allocated for new capital equipment to be operated by individual astronomy departments or university observatories (as opposed to the national centers) has declined from 20% of the equipment budget at the time of the Whitford report to 6% recently.

Federal funding per *PhD astronomer* in constant dollars has declined to less than 50% of the level at the time of the Whitford report. Moreover, even if the entire recommended program of the Astronomy Survey Committee is adopted, the annual funding growth required, which has been estimated by the Committee as averaging 5.5% over the next decade, is much less than the current average manpower growth (nearly 20%). Thus, even if the manpower growth should taper to a more normal 7% per year, federal funding per astronomer will continue to decline throughout the next decade.

IV. LISTS OF GROUND-BASED ASTRONOMY TELESCOPES

These lists are reasonably accurate as of September 1971; but since additions and modifications occur fequently, some inaccuracies are bound to be present. Except where otherwise noted, cost estimates are in terms of the dollars actually paid at the time of construction, i.e., no adjustments for subsequent inflation have been made.

Many persons contributed to the collection of this information. The organization of the material for purposes of publication was done by William E. Howard of the National Radio Astronomy Observatory. The original lists were assembled by the following: Tables 9.42 and 9.43, W. L. Reitmeyer and R. Rodman; Table 9.44, W. E. Howard and B. Balick; Table 9.45, J. Becker; Table 9.46, R. Y. Dow; Table 9.47, M.R. Kundu; Table 9.48, W. E. Howard and B. Balick.

TABLE 9.42 Current 20-Inch and Larger Optical and Infrared Telescopes of U.S. Institutions
Abbreviations: N, Newtonian focus; P, prime focus, C, Cassegrain focus; D, coudé focus; S, Schmidt; R, Refractor; E, Estimated.

Location	Aperture (in.)	Type	Owner and/or Operator	Completion Date	Cost in $1000's	Remarks[a]
Part 1. Apertures Greater Than 36 Inches						
Palomar Mt., Calif.	200	P-C-D	Caltech	1948	25,000	Replacement cost in 1971 with modern instrumentation
Kitt Peak, Ariz.	158	P-C-D	NSF	E1973	10,000	
Cerro Tololo, Chile	158	P-C-D	NSF, Ford Foundation	E1975	10,000	
Mt. Hamilton, Calif.	120	P-C-D	U. of Calif.	1959	3,000	
Mt. Locke, Tex.	107	C-D	U. of Tex.	1969	5,900	
Mt. Wilson, Calif.	100	N-C-D	Carnegie	1917		
Las Companas, Chile	100	C-D	Carnegie	E1975	5,000	
Kitt Pk. Sta., Ariz.	90	C-D	U. of Arizona	1969	2,500	
Mauna Kea, Hawaii	88	C-D	U. of Hawaii	1970	4,200	Very-high-altitude site
Kitt Peak, Ariz.	84	C-D	NSF	1964	2,500	
Mt. Locke, Tex.	82	P-C-D	U. of Tex.	1939		
Flagstaff, Ariz.	72	C	Ohio State–Ohio Wesleyan U.–Lowell	1932		Moved to Flagstaff in 1961
Mt. Wilson, Calif.	62	–	Caltech	1962		IR Survey Telescope (see note below)
Harvard, Mass.	61	N-C-D	Harvard U.	1934		
Flagstaff, Ariz.	61	C	USN	1963		Astrometric
Catalina Mt., Ariz.	61	C	U. of Ariz.	1965		
Catalina Mt., Ariz.	60	C	U. of Ariz.	1965		IR–metal mirror has been moved to Baja Calif.

Catalina Mt., Ariz.	60	C	AFCRL	1969	
Catalina Mt., Ariz.	60	C	U. of Minn.–U. of Calif., San Diego	1970	IR – metal mirror
Boyden Sta., S. Africa	60		SAO/Consortium of European Obs.		
Mt. Wilson, Calif.	60	N-C-D	Carnegie	1908	
Catalina Mt., Ariz.	60	C	NASA (Goddard, Huntsville, Inst. for Space Studies), SUNY (Stony Brook), and U. of Arizona	1969	
Kitt Peak, Ariz.	60–82	–	NSF	1962	Solar
Haleakala, Maui, Hawaii	60	C	ARPA (Everett Labs)	1967	IR tracking
Cerro Tololo, Chile	60	C-D	NSF, USAF	1967	
Palomar Mt., Calif.	60	C-D	Carnegie	1971	
Mt. Hopkins, Ariz.	60	C-D	SAO	1970	
Peach Mt. Sta., Mich.	52	C-D	U. of Mich.	1970	
Kitt Peak, Ariz.	50	C	NSF	1966	
Palomar Mt., Calif.	48–72	S	Caltech	1952	
Arbuckle Neck, Va.	48	C	Lincoln Lab.	1962	Tracking
Haleakala, Maui, Hawaii	48,48	C	ARPA (Everett Labs)	1967	IR tracking
Cloudcroft, N.M.	48	N	USAF	1966	Alt-azimuth
Flagstaff, Ariz.	42	C-D	Lowell Obs.	1970	
Williams Bay, Wisc.	40	C-D	U. of Chicago	1969	
Williams Bay, Wisc.	40	R	U. of Chicago	1897	
Flagstaff, Ariz.	40	C	USN	1934–1955	
Bethany Sta., Conn.	40	C-D	Yale U.	1966	
Toledo, Ohio	40	C-D	U. of Toledo	1969	
Evanston, Ill.	40	C-D	Northwestern U.	1968	
Oakland, Ill.	40	C-D	U. of Ill.	1968	
Las Campanas Mt., Chile	40	C-D	Carnegie Southern Obs.	1971	
Fan Mt. Sta., Va.	40	C	U. of Va.	1971	Astrometric
Indianapolis, Ind.	38	C	Butler U.	1954	
Ann Arbor, Mich.	37.5	P-C	U. of Mich.	1911	

TABLE 9.42 (*Continued*)

Location	Aperture (in.)	Type	Owner and/or Operator	Completion Date	Cost in $1000's	Remarks[a]
Part 2. Apertures Less Than or Equal to 36 Inches (list is probably less complete beyond this point)						
Mt. Hamilton, Calif.	36	R	U. of Calif.	1888		
Mt. Hamilton, Calif.	36	P-N	U. of Calif.	1894		
Kitt Peak Sta., Ariz.	36	N-C	U. of Arizona	1922–1963		
Brooklyn, Ind.	36	C	Indiana U.	1939–1966		
Mt. Locke, Tex.	36	C	U. of Texas	1957		
Cleveland, Ohio	36	C	Case Western Reserve U.	1957		
Pine Bluff, Wisc.	36	C	U. of Wisc.	1958		
Kitt Peak, Ariz.	36	C	NSF	1961		
Palestine, Tex.	36	C	Princeton U./NSF/ONR/NASA	1962		Stratoscope II, balloon flights 1963–1970
Kitt Peak, Ariz.	36	C-D	NSF	1966		
Princeton, N.J.	36	C	Princeton U.	1966		
Greenbelt, Md.	36	C-D	NASA	1967		
Cerro Tololo, Chile	36	C	NSF	1967		
Atlanta, Ga.	36	C	Fernbank Sci. Center	1968		
Baton Rouge, La.	36	C	La. State U.	1970		
Delaware, Ohio	32	C	Ohio State U.–Ohio Wesleyan U.	1943		Moved to Delaware in 1961
Fan Mt. Sta., Va.	32	C	U. of Va.	1964		
Pittsburgh, Pa.	31	C	U. of Pittsburgh	1906		
Pittsburgh, Pa.	30	R	U. of Pittsburgh	1914		
San Gabriel Mt., Calif.	30	N-C	Stony Ridge Obs.	1963		Amateur, largest in world

Socorro, N.M.	30	C	N.M. Inst. of Mining and Tech.	1971	Remotely controlled by computer
Flagstaff, Ariz.	30	C	U.S. Geolog. Surv.	1964	
Minneapolis, Minn.	30	C	U. of Minn.	1967	
Rosemary Hill, Fla.	30	C-N	U. of Florida	1967	
Lafayette, Calif.	30	C	U. of Calif., Berkeley	1966	
Mt. Locke, Tex.	30	C	U. of Texas	1970	
Sacramento Pk., N.M.	30	–	USAF	1969	Vacuum solar
Philadelphia, Pa.	28	P-N-C	U. of Pa.	1956	
Catalina Mt., Ariz.	28	C	U. of Ariz.	1963	
Palestine, Tex.	28	C	U. of Ariz.	1966	Polariscope, balloon flight 1966
Lamont–Hussey Obs.	27	R	U. of Mich.	1927	
Washington, D.C.	26	R	USN	1873	
Charlottesville, Va.	26	R	U. of Virginia	1883	
Tampa, Fla.	26	S-C	U. of So. Fla.	1968	
Chardon, Ohio	24–36	S	Case Western Reserve U.	1941–1957	
Cerro Tololo, Chile	24–36	S	Loaned to CTIO	1950–1967	Loaned from U. of Mich.
Flagstaff, Ariz.	24	R	Lowell Obs.	1896	
Swarthmore, Pa.	24	R	Swarthmore Col.	1911	
Flagstaff, Ariz.	24	N	Ariz. State U.	1953	
Nashville, Tenn.	24	N	Vanderbilt U.	1953	
Portage Lake, Mich.	24	C	U. of Mich.	1940–1958	
Provo, Utah	24	P-N-C	Brigham Young U.	1959	
Flagstaff, Ariz.	24	C	Lowell Obs.	1960	
Albuquerque, N.M.	24	–	Sandia Corp.	1962?	
Mt. Wilson, Calif.	24	C	Carnegie Inst.	1963	
Williams Bay, Wisc.	24	C	U. of Chicago	1964	Polarization
Mt. Hamilton, Calif.	24	C	U. of Calif.	1964	
San Diego, Calif.	24	C	San Diego State Col.	1964	
Corralitos Obs., N.M.	24	C	Northwestern U.	1965	

TABLE 9.42 (*Continued*)

Location	Aperture (in.)	Type	Owner and/or Operator	Completion Date	Cost in $1000's	Remarks[a]
Part 2. (Continued)						
Ojai, Calif.	24	C	UCLA	1965		
Iowa City, Ia.	24	–	U. of Iowa	1965		
Gannett Hill, N.Y.	24	C	U. of Rochester	1965		
Mt. Cuba, Del.	24	P-C	U. of Del.	1965		
Table Mt., Calif.	24	C-D	JPL	1966		
Wellesley, Mass.	24	C	Wellesley Col.	1966		
Albuquerque, N.M.	24	C	U. of N.M.	1966		
Alliance, Ohio	24	D	Mt. Union Col.	1966		
Hanscom Field, Mass.	24	–	USAF	1966		
New Canaan, Conn.	24	–	Perkins Obs.	1966		
Ames, Ia.	24	C	Iowa State U.	1967		
Las Cruces, N.M.	24	C	N.M. State U.	1967		
Waltham, Mass.	24	–	Brandeis U.	1967		
Pine Mt., Ore.	24	C	U. of Ore.	1968		
Mauna Kea, Hawaii	24	C	USAF, U. of Hawaii	1968		
Magdalena Peak, N.M.	24	C	N.M. State U.	1969		
Cerro Tololo, Chile	24	C	Lowell Obs. (NASA, NSF)	1969		
Mauna Kea, Hawaii	24	C	NASA–Lowell Obs.	1969		
Harriman, N.Y.	24	–	Columbia U.	1969		
Westford, Mass.	24	C-D	MIT	1971		
Mt. Hamilton, Calif.	24	C (Vertical)	U. of Calif.	1971		Aux. tel for 120-in. coudé, fed by coelostat
San Fernando, Calif.	24	C	Aerospace Corp.	1969		Solar
Flagstaff, Ariz.	24	C	USN	1970		

Flagstaff, Ariz.	23	R	USN	1882	Moved from Princeton to Flagstaff in 1965, to be re-installed
Beloit, Wisc.	22	–	Beloit Coll.	1969	
Mt. Hamilton, Calif.	22	C	U. of Calif.	1956	
Stamford, Conn.	22–26	S	Stamford Museum	1961	
Tucson, Ariz.	21.5	C	U. of Ariz.	1963–1965	
Catalina Mt., Ariz.	21	C	U. of Ariz.	1962	
Flagstaff, Ariz.	21	C	Lowell Obs.	1953	
Chattanooga, Tenn.	20.5	C	Jones Obs.	1936	
Denver, Colo.	20	R	U. of Denver	1894	
Middletown, Conn.	20	R	Wesleyan U.	1922	
Mt. Hamilton, Calif.	20	N	U. of Calif.	1940	
Mt. Hamilton, Calif.	20,20	R,R	U. of Calif.	1946–1962	Double astograph
Palomar Mt., Calif.	20	C	Carnegie Inst.	1962	
Louisville, Ky.	20	N-C	Star Lane Obs.	1956	
Lafayette, Calif.	20	N-C	U. of Calif. (Berkeley)	1956–1965	
Twelve Instruments	20–31	S	SAO/NASA	1957ff.	Baker-Nunn tracking cameras in various locations
New Haven, Conn.	20	C	Yale U.	1962	
College Park, Md.	20	C	U. of Md.	1964	
El Leoncito, Argentina	20,20	R	Yale U./Columbia U.	1965	
Mt. Hopkins, Ariz.	20	C	SAO	1968	Laser ranging

[a] Where a telescope carries the remark "IR" (infrared), it usually implies that the instrument is not suitable for most kinds of observations that are made with conventional optical telescopes.

TABLE 9.43 Approximate Locations of Field Stations for U.S. Telescopes

Station	Location
Palomar Mt.	21 mi. NE of Escondido, Calif.
Kitt Peak	42 mi. SW of Tucson, Ariz.
Cerro Tololo	33 mi. SE of La Serena, Chile
Mt. Hamilton	14 mi. E of San Jose, Calif.
Mt. Locke	15 mi. N of Ft. Davis, Tex.
Mt. Wilson	9 mi. NE of Pasadena, Calif.
Ohio State U.–Ohio Wesleyan–Lowell Obs.	12 mi. S of Flagstaff, Ariz.
Agassiz, Harvard	25 mi. WNW of Boston, Mass.
U.S. Naval Obs.	5 mi. W of Flagstaff, Ariz.
Catalina Mt.	20 mi. NE of Tucson, Ariz.
Mt. Hopkins	35 mi. S of Tucson, Ariz.
Peach Mt.	15 mi. NW of Ann Arbor, Mich.
Bethany	8 mi. NW of New Haven, Conn.
Prairie Obs.	30 mi. S of Urbana, Ill.
Las Campanas Mt.	100 mi. N of La Serena, Chile
Fan Mt.	16 mi. SW of Charlottesville, Va.
Pine Bluff	15 mi. W of Madison, Wisc.
Rosemary Hill	25 mi. SW of Gainesville, Fla.
Leuschner Obs.	10 mi. E of Berkeley, Calif.
Lamont–Hussey Obs.	Bloemfontein, S. Afr.
Chardon, Nassau Sta.	30 mi. E of Cleveland, Ohio
Portage Lake	15 mi. NW of Ann Arbor, Mich.
Corralitos Obs.	30 mi. W of Las Cruces, N.M.
Ojai	≈ 75 mi. WNW of Los Angeles, Calif.
Gannett Hill	40 mi. S of Rochester, N.Y.
Mt. Cuba	6 mi. NW of Wilmington, Del.
Hanscom Field	Bedford, Mass.
Tortugas Mt.	5 mi. E of Las Cruces, N.M.
Pine Mt.	30 mi. W of Bend, Ore.
Magdalena Peak	30 mi. NW of Las Cruces, N.M.
El Leoncito	SW of San Juan, Argentina

Organizational Abbreviations:

AFOSR	Air Force Office of Scientific Research
AFCRL	Air Force Cambridge Research Laboratories
ARPA	Advanced Research Projects Agency (Dept. of Defense)
BRL	Ballistics Research Laboratory
Caltech	California Institute of Technology
Carnegie	Carnegie Institution of Washington
CTIO	Cerro Tololo Inter-American Observatory
ESSA	Environmental Science Services Administration
GSFC	Goddard Space Flight Center

HAO	High Altitude Observatory
IGY	U.S. Committee for the International Geophysical Year
JPL	Jet Propulsion Laboratory
Marsh. SFC	Marshall Space Flight Center
MIT	Massachusetts Institute of Technology
NASA	National Aeronautics and Space Administration
NSF	National Science Foundation
OAR	Office of Aerospace Research
ONR	Office of Naval Research
Private	Private organization
PSU	Pennsylvania State University
SAO	Smithsonian Astrophysical Observatory
UCLA	University of California at Los Angeles
USAF	United States Air Force
USN	United States Navy

TABLE 9.44 Major Foreign Optical Telescopes of Aperture Greater Than 70 Inches
Abbreviations: N, Newtonian focus; P, prime focus; C, Cassegrain focus; D, coudé focus; S, Schmidt; R, Refractor; E, Estimated.

Country	Observatory or Location	Aperture in. (cm)	Type	Completion	Altitude (m)
Australia	Mt. Stromlo	74 (188)	N-C-D	1955	808
Canada	David Dunlap	74 (188)	N-C	1935	244
Canada	Dominion	73 (185)	N-C	1918	229
Czechoslovakia	Ondřejov	79 (200)	P-C-D	1967	534
Egypt	Helwan	74 (188)	N-C-D	1960	115
France	Haute Provence	77 (193)	N-C-D	1958	580
Germany (DDR)	Obs. of Ger. Acad.	80 (203)	C-D	1960	330
Great Britain	Herstmonceux	98 (250)	P-C-D	1967	34
Japan	Okayama	74 (188)	N-C-D	1960	370
S. Africa	Radcliffe	74 (188)	N-C-D	1948	1542
U.S.S.R.	Crimea	104 (264)	P-C-D Nasmyth	1960	570
Under Construction					
U.S.S.R.	Caucasus	236 (600)			
Australia/U.K.	Australia	150 (382)			
France, W. Germany, Holland, Sweden	Chile	140 (357)			
U.S.S.R.	Burakan Obs.	102 (260)			

TABLE 9.45 Ground-Based Solar Optical Telescopes in the United States with Apertures Greater Than or Equal to 5 Inches

State	Observatory	Sponsor	Aperture (in.)	Image Size (cm)	Type[a]	Attachments[b]	Feed[c]	Cost in $1000's[d]	Date of First Operation	Remarks
Alabama	Marsh. SFC (Huntsville)	NASA	12	4.0	GPT	MG	–	30	1971	
Arizona	Kitt Peak	NSF	63	82.0	GPT	SG, SHG, MG			1962	
	(Tucson)	NSF	36	37.0	GPT	SG, SHG, MG	HS	5000 (1962)	1965	One feed
		NSF	36	33.0	GPT	SG, MG			1970	
California	Aerospace	Aerospace	24	11.4	VT	SHG		575 (1969)	1969	Main opt. system
	(Sylmar)	Aerospace	12	5.1	VT	SHG	EQ		1969	Aux. image system
		Aerospace	12	9.1	CHR	LF		125 (1970)	1971	H-α and K
		Aerospace	6	7.6	CHR	LF	EQ	80 (1966)	1966	
		Aero/NASA	6	7.5	CHR	LF, MG, VID	EQ	260 (1970)	1971	Real-time mag. field
	Big Bear	NASA	16	12.5	GPT	SG			1970	
		NASA	9	12.5	COR	SG	EQ	300 (1969)	1970	On same spar
		NASA	10	10.0	CHR	LF			1970	
		NASA	10	10.0	CHR	LF			1970	
	Caltech	NASA	VAR	VAR	GPT	SG, VID	–	–	1970	Downs Lab
	(Pasadena)	IIE	26	VAR	GPT	–	CS	–	1936	Robinson Lab
		NSF	5	5.0	FP	LF	EQ	30 (1967)	1967	Robinson Lab
	Hale Lab. (Pasadena)	Carnegie	18	42.0	GPT	SG	CS	–	1926	Largely unused
	Lockheed	–	6	7.5	–	–	EQ	–	–	
		–	7	7.5	–	–	EQ	–	–	
		–	7	7.5	–	–	EQ	–	–	
		–	16	7.5	–	–	CS	–	–	
	Mt. Wilson	Carnegie	12	42.0	GPT	SG, MG	CS	–	1912	
		Carnegie	12	16.0	GPT	SHG		–	1908	
		Carnegie	16	9.0	GPT	SHG	CS	–	1908	Same feed
		Carnegie	12	5.0	GPT	SHG		–	1908	

		Carnegie	24	16.0	GPT	SG	CS	–	1904	Largely unused
Colorado	NOAA (Boulder)	NASA/NOAA	8	5.7	GPT	SHG	–	10	1970	
	HAO (Climax)	NSF	5	2.8	COR	SG	EQ	–	1956	
	(Climax)	NSF	16	10.0	COR	SG, MG, LF	EQ	–	1964	
	(Horse Creek)	NSF	10	–	VT	–	EQ	–	1970	
Hawaii	Haleakala	NASA	10	7.0	COR	SG	EQ	–	1969	
	(Maui)	NASA	6.5	8.0	CHR	LF	EQ	–	1969	3-cm image avail.
		ONR	6.5	2.5	COR	POL	EQ	–	1970	
Massachusetts	Hadley (Mt. Holyoke)	–	6	–	PHO	–	–	–	–	Occasional sunspot photo
Michigan	McMath–Hulbert	IGY								
	(Pontiac)	Private	5.5	1.4	FP	LF	EQ	–	1958	
		Private	16	14.0	GPT	SG, SHG		–	1936	Uses same feed,
		Private	16	11.4	GPT	SG, SHG		–	1936	recent changes funded
		Private	12	5.7	GPT	SG, SHG	CS	–	1936	by ONR
		Private	6.5	2.2	GPT	SG, SHG		–	1936	
		Private	10	7.0	GPT	SG, SHG	–	–	1940	Largely unused
		Private	18	28.0	GPT	SG, LF	CS	–	1941	14-cm image avail.
		NASA	6	1.4	FP	LF	EQ	71 (1970)	1970	
New Mexico	Sacramento Peak	Air Force	16	25.0	COR	SG, SHG, LF		–	1961	
	(Sunspot)	Air Force	12	25.0	GPT	SG, SHG, LF		–	1960	
		Air Force	16	7.5	GPT	SG, MG	EQ	–	1967	On same spar
		Air Force	9	7.5	COR	SG		–	1961	
		Air Force	15	7.5	CHR	LF		–	1954	
		Air Force	30	51.0	VT	SG, LF, SHG	ALT	4000 (1969)	1970	
		Air Force	6	2.5	PHO	–	EQ	–	1967	
		Air Force	6	4.0	COR	LF	EQ	–	1955	

[a] Type: GPT, general-purpose telescope; PHB, photoheliograph; CHR, chromospheric telescope; COR, coronograph; FP, flare patrol; VT, vacuum telescope.
[b] Attachments: SG, spectrograph; SHG, spectroheliograph; LF, Lyot filter; MG, magnetograph; POL, polarimeter; VID, video system.
[c] Feed: CS, coelostat; EQ, equatorial mounting; ALT, alt-azimuth mounting; HS, heliostat.
[d] Cost: The number in parentheses refers to year.

TABLE 9.46 Major U.S. Radio-Astronomical Telescopes

State	Observatory	Sponsor	Antenna Description	Frequency Monitored	Date of First Operation	Cost in $1,000's	Remarks
Alaska	Chena Valley	NSF	18.6-m (61-ft) steerable paraboloid Two 8.53-m (28-ft) steerable paraboloids used as interferometer	130–400 MHz			
Arizona	National Radio Astron. Obs. (Tucson)	Assoc. U., Inc., under contract with NSF	11-m (36-ft) steerable paraboloid	$\lesssim$260 GHz	1967	1,000	High-precision reflector mounted in dome
California	Clark Lake (Borrego Sprgs.)	NASA, NSF, and U. of Maryland	(1) Array of 16 log periodic elements on 3300-m E–W baseline[a]	20–60 MHz	1968	60	(1) Swept frequency
			(2) T-shaped array of 720 helical antennas, 3000-m E–W, 1800-m N–S	(10–110 MHz)	1961		
			(3) Array of dipoles, 3800-m E–W, 250-m N–S	26.3 MHz			(3) Daytime interference
			(4) Array of five Yagi antennas	16–22 MHz			(4) Jupiter monitor
	Hat Creek (Cassel)	NSF, ONR, and U. of Calif.	(1) 26-m (85-ft) steerable paraboloid (2) 10.1-m (36-ft) steerable paraboloid	1420–3200 MHz	1962	350	
			(3) 6.5-m (20-ft) steerable paraboloid, cass. feed	$\lesssim$38 GHz	1968		(3) High-precision reflector
	Owens Valley (Big Pine)	Caltech under grant from ONR and NSF	(1) Two 27.4-m (90-ft) steerable paraboloids usable as interferometer	30 MHz 11 GHz	1958	~1,000 ea.	
			(2) 39.6-m (130-ft) steerable paraboloid			1,600	
	Stanford (Radio Astron. Inst.)	AFOSR, NSF	(1) Thirty-two 1-m (3-ft) paraboloids in cross array	3300 MHz	1960	600	Primarily for solar work
			(2) Five-element array	10.7 GHz	1970	<2000	

	Stanford (Center for Radar Astron.)	NASA	45.7-m (150-ft) steerable paraboloid	412 MHz	1960	350	Used for radio and radar astronomy
	NASA/JPL (Goldstone)	JPL/Caltech under contract with NASA	(1) 64-m (210-ft) steerable paraboloid, Cass. feed	∼2300 MHz		12,000	(1) Mars site[b]
			(2) 25.9-m (85-ft) steerable paraboloid, Cass. feed	∼2300 MHz	1958	750	(2) Venus site
			(3) 9.15-m (30-ft) steerable paraboloid, Cass. feed	22 GHz	1963		(3) Venus site
			(4) 25.9-m (85-ft) steerable paraboloid, Cass. feed	2295 MHz	1958	750	(4) Pioneer site
			(5) 25.9-m (85-ft) steerable paraboloid, Cass. feed	2295 MHz			(5) Echo site. Radio astron. secondary usage
California	Table Mountain (Wrightwood)	JPL/Caltech under contract with NASA	5.49-m (18-ft) steerable paraboloid, Cass. feed	<135 GHz	1970		High-precision reflector
	Aerospace (El Segundo)	Aerospace	4.6-m (16-ft) steerable paraboloid, Cass. feed	<94 GHz	1963		High-precision reflector
Colorado	Table Mountain (Boulder)	NOAA	(1) 19.7-m (65-ft) steerable paraboloid	610 MHz	1968		
			(2) 2-m (6-ft) steerable paraboloid	10.7 GHz	1965		
	U. of Colorado (Boulder)	NOAA, NSF, and U. of Colo.	Two corner reflectors, used as array[a]	7.5–80 MHz	1959	200	Swept-frequency interferometer
Florida	U. of Florida (Dixie County)	–	(1) Four five-element Yagi antennas	15–22 MHz			
			(2) Seven-element Yagi antenna	28 MHz			
			(3) Pair of crossed five-element Yagi antennas	18 MHz and 22 MHz	1959		(3) Polarization measurements
			(4) N–S array of 20 dipoles	20 MHz			
			(5) Filled rectangular array of 640 dipoles	26 MHz	1972		
Hawaii	Kauai Obs.	NASA	Two 5-element Yagi antennas, used as interferometer	16–22 MHz			Station in GSFC–Jupiter Monitor Network

TABLE 9.46 Major U.S. Radio-Astronomical Telescopes (*Continued*)

State	Observatory	Sponsor	Antenna Description	Frequency Monitored	Date of First Operation	Cost in $1,000's	Remarks
Illinois	Vermilion River (Danville)	NSF, ONR, and U. of Illinois	(1) 183 m × 122 m (600 ft × 400 ft) fixed-aperture parabolic cylinder, meridian transit	611 MHz (74 MHz)	1962	400	(1) N–S focal line
			(2) 36.6-m (120-ft) steerable paraboloid	($\lesssim$2 GHz)	1970	500	
Iowa	North Liberty	U. of Iowa	(1) 1.2-m (4-ft) paraboloid, Cass. feed	15.4 GHz	1967	70	
			(2) Dual 5-element Yagi antennas	40 MHz	1967		
			(3) 18.3-m (60-ft) steerable paraboloid	<5 GHz	1968		(3) and (4) to be instrumented for radio astron. research
			(4) 8.5-m (28-ft) steerable paraboloid	<3 GHz	1964		
			(5) 16-dipole array, 75 ft × 300 ft	26.3 MHz	1970		
	Ogden	Iowa State U.	Two 16-dipole arrays, 75 ft × 300 ft	26.3 MHz	1970		
Maryland/ D.C.	Derwood	Carnegie	18.3-m (60-ft) steerable paraboloid	1420 MHz	1959		
	Goddard (Greenbelt)	NASA	Two 5-element Yagi antennas, used as interferometer	16–22 MHz			Station in GSFC–Jupiter Monitor Network
	Maryland Point (Riverside)	ONR	(1) 25.9-m (85-ft) steerable paraboloid	$\lesssim$31 GHz	1957	250	
			(2) 25.9-m (85-ft) steerable paraboloid	1720 MHz			
	NRL (Washington)	ONR	15.2-m (50-ft) steerable paraboloid	–	1951	100	
Maryland/ D.C.	U. of Md. (College Park)	U. of Md.	(1) 6.1-m (20-ft) steerable paraboloid	1420 MHz	1970		Used mainly for training
			(2) Three-element interferometer	327 MHz	1969		Used mainly for training
Massachusetts	Agassiz Sta. (Harvard)	Harvard U., NSF, and SAO	25.9-m (85-ft) steerable paraboloid, Cass. feed	$\lesssim$6 GHz	1956		

	Five College (New Salem)	Five College Astron. Dept.	Four 36.6-m (120-ft) spherical reflectors	73–614 MHz	1970		To be expanded to 32-element array
	Sagamore Hill (Hamilton)	USAF, OAR	(1) 45.7-m (150-ft) steerable paraboloid	63–410 MHz	1963		
			(2) 25.9-m (85-ft) steerable paraboloid	74–430 MHz	1958		
			(3) 8.5-m (28-ft) steerable paraboloid[a]	611–415 MHz	1965	75	Solar patrol
			(4) 8.5-m (28-ft) steerable paraboloid[a]	(100–406 MHz)	1968	90	Solar patrol
			(5) 2.4-m (8-ft) steerable paraboloid[a]	2.7–8 GHz	1959, 1966	15	(5) (10–12) Solar patrol
			(6) 9.1-m (30-ft) steerable paraboloid	15.4 GHz			
			(7) 3.0-m (10-ft) steerable paraboloid	34.5 GHz	1961		
			(8) Three 18.3-m (60-ft) steerable paraboloid array	5 GHz			(8) Solar polarization
			(9) Two-element interferometer	20–(50) GHz			(9) Sweep freq., solar patrol
			(10) 1-m (3-ft) steerable paraboloid	15.4 GHz	1968	20	
			(11) 0.3-m (1-ft) steerable paraboloid	35.4 GHz	1969	<30	
			(12) Two biconical antennas	24–48 MHz	1970	–	
	AFCRL (Waltham)		9.5-m (31-ft) steerable paraboloid	100 GHz	1965		High-precision reflector
	Haystack (Tyngsboro)	NASA, NSF, and USAF	(1) 36-m (120-ft) steerable paraboloid	<38 GHz	1967	6,500	In radome
			(2) 8.5-m (28-ft) steerable paraboloid	$\lesssim$90 GHz	1961		
	Millstone Hill	Mass. Inst. Tech.	25-m (84-ft) steerable paraboloid	$\lesssim$5 GHz	1961		
Michigan	U. of Mich. (Dexter)	NSF	(1) 25.9-m (85-ft) steerable paraboloid[a]	$\lesssim$16 GHz	1959	375	
			(2) 8.5-m (28-ft) steerable paraboloid[a]	($\lesssim$8 GHz)		60	
New York	Cornell U. (Danby)	Cornell U., NSF, and ARPA	(1) 26-m (85-ft) spherical reflector	$\lesssim$2.7 GHz	1970		(1) Inclined 13° from zenith
			(2) 5.2-m (17-ft) steerable paraboloid	430 MHz			

TABLE 9.46 Major U.S. Radio-Astronomical Telescopes (*Continued*)

State	Observatory	Sponsor	Antenna Description	Frequency Monitored	Date of First Operation	Cost in $1,000's	Remarks
Ohio	Ohio State, Ohio Wesleyan (Delaware)	NSF	103.8 m × 21.4 m (340 ft × 70 ft) standing paraboloid with tiltable flat reflector, meridian transit	612 MHz–2.7 GHz	1961		
Pennsylvania	Penn. State U. (U. Park)	NASA, NSF, ONR, and BRL	(1) Four-element interferometer (Arsac array), fixed lobes[a]	108 MHz	1968	25	(1) Solar obs.
			(2) Four-element interferometer (Arsac array), fixed lobes	(408 MHz)			(2) Galactic obs. plan
			(3) Stacked Yagi antennas[a]	408 MHz	1970	10	(3) Burst patrol
			(4) Stacked Yagi antennas	(408 MHz)			(4) Burst patrol
			(5) 2.44-m (8-ft) steerable paraboloid	(33–38 GHz)			(5) Tunable traveling wave maser
			(6) 1.83-m (6-ft) steerable paraboloid[a]	960 MHz	1963	11	(6) Solar burst patrol
			(7) 1.83-m (6-ft) steerable paraboloid[a]	2.7 GHz	1963	11	(7) Solar burst patrol
			(8) 1.83 m × 0.25 m (6 ft × 0.8 ft) steerable cylindrical paraboloid[a]	100 GHz	1970	25	(8) Solar burst patrol and eclipse project
			(9) 1.0-m (3-ft) steerable paraboloid[a]	10.7 GHz	1963	11	(9) Solar burst patrol
			(10) 0.3-m (1-ft) steerable paraboloid[a]	37.5 GHz	1969	15	(10) Eclipse project and burst patrol
			(11) 9-m (30-ft) steerable paraboloid	1200–1500 MHz	1972	90	(11) Source monitoring
Puerto Rico	(Arecibo Obs.) National Astronomy and Ionosphere Center	Cornell U. under contract with NSF; financial assistance from NASA, ARPA, and AFOSR	305-m (1000-ft) spherical reflector with steerable feeds	40–611 MHz (1420 MHz)	1963	9,000	

Texas	Harvard Sta. (Fort Davis)	Harvard U. under contract with USAF	(1) 26-m (85-ft) steerable paraboloid[a]	0.5–2 GHz	1961		
			(2) 8.5-m (28-ft) steerable paraboloid[a]	2.1–3.9 GHz			
			(3) 8.5-m (28-ft) steerable paraboloid[a]	100–600 MHz	1956		
			(4) Fixed broadside array[a]	25–100 MHz	1959		
			(5) Log periodic steerable array	10–25 MHz			
	Mt. Locke (Fort Davis)	NASA	4.85-m (16-ft) steerable paraboloid	$\lesssim$140 GHz	1963		High-precision reflector
	U. of Texas (Marfa)	NASA, NSF, and U. of Texas	(1) Sixteen-element array	330–400 MHz			
			(2) Five E–W line arrays, used as a 3-km interferometer	(330–400 MHz)			
			(3) Decameter interferometer	10–30 MHz			
Virginia	Wallops Sta. (Wallops)	NASA	18.3-m (60-ft) steerable paraboloid	158 MHz–(10 GHz)			
West Virginia	National Radio Astronomy Observatory	Assoc. U. Inc., under contract with NSF	(1) 91.4-m (300-ft) meridian transit paraboloid	50 MHz–(5 GHz)	1962	900	
			(2) 42.7-m (140-ft) steerable paraboloid	234 MHz–24 GHz	1965	13,500	
			(3) Three 25.9-m (85-ft) steerable paraboloids used as synthesis interferometer; 14-m (45-ft) portable antenna used as remote sites	(14–20 MHz), 2.7,8 GHz	1965	1,400	

[a] Included in table of solar radio telescopes.

[b] Primary use is for communication with space vehicles. Up to 5% of the time is available for radio-astronomy research.

TABLE 9.47 U.S. Solar Radio-Astronomy Telescopes

Institution	Location of Instrument	Frequency	Antenna Size	Type	Compl. Date	Cost in $1000's Ant.	Cost in $1000's Rec. Equip.	Sponsor	Remarks
Harvard Col. Obs.	Fort Davis, Tex.	25–100 MHz	Two dipoles	Patrol instrument for dynamic spectral studies of solar bursts	1959	–	–		
		100–580 MHz	28-ft paraboloid	Patrol instrument for dynamic spectral studies of solar bursts	1956	–	–		
		2100–3900 MHz	28-ft paraboloid	Patrol instrument for dynamic spectral studies of solar bursts	1960	–	–		
		550–2000 MHz	85-ft paraboloid	Patrol instrument for dynamic spectral studies of solar bursts	1970	–	–		
U. of Michigan	Ann Arbor, Mich.	100–580 MHz	28-ft paraboloid	Patrol instrument for dynamic spectral studies of solar bursts	1957	60	75	ONR	Cost does not include salaries
		8000 MHz	85-ft paraboloid	Fast recording of burst activity	sporadic since 1968	–	–		

Stanford U.	Stanford, Calif.	3300 MHz	32 3-ft paraboloids used as a cross (16 in E–W and 16 in N–S)	Produces daily spectroheliograms with a 3-min arc pencil beam	1960	600 (2/3 in salaries)		AFOSR	
U. of Maryland	Clark Lake, Calif.	20–60 MHz	16 log-periodic ant. used as a E–W sweep freq. grating interferometer	Swept-frequency grating interferometer	1968	60	60	NASA	Cost does not include salaries
Air Force Cambridge Res. Labs.	Sagamore Hill, Mass.	606 and 1415 MHz	28-ft. paraboloid	Patrol instrument	1965	75	60	AFOSR	Cost does not include salaries
		245 MHz	28-ft paraboloid	Patrol instrument	1968	90	15	AFOSR	
		8800, 4995, and 2695 MHz	8-ft paraboloid	Patrol instrument	1959 1966 1966	15	45	AFOSR	
		3-ft paraboloid	15,400 MHz	Patrol instrument	1968	20	15	AFOSR	
		1-ft paraboloid	35,400 MHz	Patrol instrument	1969	30		AFOSR	
		2 biconical ant.	24–48 MHz	Dynamic spectral obs.	1970	–	75	AFOSR	
High Altitude Obs., U. of Colorado	Boulder, Colo.	2 corner reflectors ea. 500 sq. m in area	7.5–80 MHz	Swept-frequency interferometer	1959	100	100	AFOSR	

TABLE 9.47 U.S. Solar Radio-Astronomy Telescopes (*Continued*)

Institution	Location of Instrument	Frequency	Antenna Size	Type	Compl. Date	Cost in $1000's Ant.	Cost in $1000's Rec. Equip.	Sponsor	Remarks
Pennsylvania State U.	State College, Pa.	100,000 MHz	1-ft × 6-ft cyl. paraboloid	Patrol instrument	1970	25		NSF	
		37,500 MHz	1-ft parab-oloid	Patrol instrument	1969	15		NSF and ONR	
		10,700 MHz	3-ft parab-oloid	Patrol instrument	1963	11		NSF, ONR, PSU	
		2700 MHz	6-ft parab-oloid	Patrol instrument	1963	11		NSF, ONR, PSU	
		960 MHz	6-ft parab-oloid	Patrol instrument	1963	11		NSF, ONR, PSU	
		328 MHz	Pair of Yagis	Patrol instrument	1970	10		NSF	
		100 MHz	1500-ft-long 4-element array	Ang. resolution 20 min of arc, used only occa-sionally when sun is active	1968	25		NASA	

TABLE 9.48 Major Radio-Astronomical Facilities outside the United States

Type	Country	Observatory	Description	Freq. Monitored	Date of First Operation	Remarks
Steerable paraboloid	Australia	Parkes	64-m (210-ft) diam	$\lesssim$ 5 GHz	1961	Sometimes used with movable 60-ft paraboloid as interferometer
	Canada	Algonquin Park	46-m (150-ft) diam	$\lesssim$ 15 GHz	1965	
	Great Britain	Nuffield Labs. (Jodrell Bank)	76-m (250-ft) diam (Mark I)	$\lesssim$ 3 GHz	1957	Mark I and II or III sometimes used as interferometer
			38 m × 25 m (125 ft × 85 ft) diam (Mark II)	$\lesssim$ 10 GHz	1964	
			38 m × 25 m (125 ft × 85 ft) diam (movable) (Mark III)	$\lesssim$ 2 GHz	1967	
	U.S.S.R.	Crimea	22-m (72-ft)-diam millimeter-wave telescope	$\lesssim$ 150 GHz	1966	
	W. Germany	Max Planck Inst. für R.A.	100-m (328-ft) diam	$\lesssim$ 15 GHz	1972	
Interferometer	Australia	Culgoora	(96) 14-m (45-ft) paraboloids in 3000-m-diam circ. array	80 MHz (20–2000 MHz possible)	1967	Solar array, effective area = 6000 m^2
		Molonglo	1600-m (5280-ft) cross array of dipoles	111,408 MHz	1968	Effective area = 25000 m^2
	Canada	Algonquin Park	(40) 3-m (10-ft) paraboloids in 874-m E–W array	2800 MHz	1967	Solar array
		Penticton	1300 m × 440 m T dipole array	22.25 MHz	1968	
			1242 m × 725 m T dipole array	10 MHz	1965	
	Great Britain	Malvern	(2) 25-m (82-ft) paraboloids one on 750-m E–W track, one on 1500-m approx. N–S track	$\lesssim$ 5 GHz	1961	
		Cambridge	(3) 18-m (60-ft) paraboloids on 1600-m E–W array	$\lesssim$ 5 GHz	1965	

TABLE 9.48 Major Radio-Astronomical Facilities outside the United States (*Continued*)

Type	Country	Observatory	Description	Freq. Monitored	Date of First Operation	Remarks
Interferometer (*Continued*)			5-acre dipole array	178 MHz	1968	Scintillation studies
			(6 or 8) 15-m (50-ft) paraboloids on 5000-m E–W array	$\lesssim 5$ GHz	1971	Nearing completion
			(2) 9-m (30-ft) paraboloids on 800-m E–W array	$\lesssim 2$ GHz	1968	Line array
			700-m E–W cylindrical paraboloid and 800-m N–S track with movable element	178 MHz	1962	4C array
	India	Ooty	530-m N–S dipole array with steerable cylindrical parabolic reflector	327 MHz	1971	Mainly for occultation work, 13-m paraboloids to be added
	Italy	Bologna	T array of cylindrical paraboloids (564-m E–W arm) and paraboloids (320-m N–S arm)	408 MHz	1968	E–W arm operational since 1964
	Netherlands	Westerbork	(12) 25-m (85-ft) paraboloids in 1600-m (E–W array)	$\lesssim 2$ GHz	1970	Continuous operation since 1971
	Peru	Jicamarca	288 m × 288 m (1000 ft × 1000 ft) broadside dipole array	50 MHz	1962	
	U.S.S.R.	Kharkov	1800 m × 900 m (5900 ft × 3000 ft) T dipole array	26 MHz	1970	
Ground plane reflectors	France	Nancay	Dimensions 40 m × 200 m, collecting area 7000 m^2	$\lesssim 5$ GHz	1964	Collecting area of 47-m paraboloid
	U.S.S.R.	Caucasus	Radius 288m; Height 7.4 m	$\lesssim 40$ GHz		Under construction
		Pulkova	Radius ~ 100-m (348 ft) Collecting area ~ 9000 m^2	$\lesssim 10$ GHz	1960	Collecting area of 17-m paraboloid

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