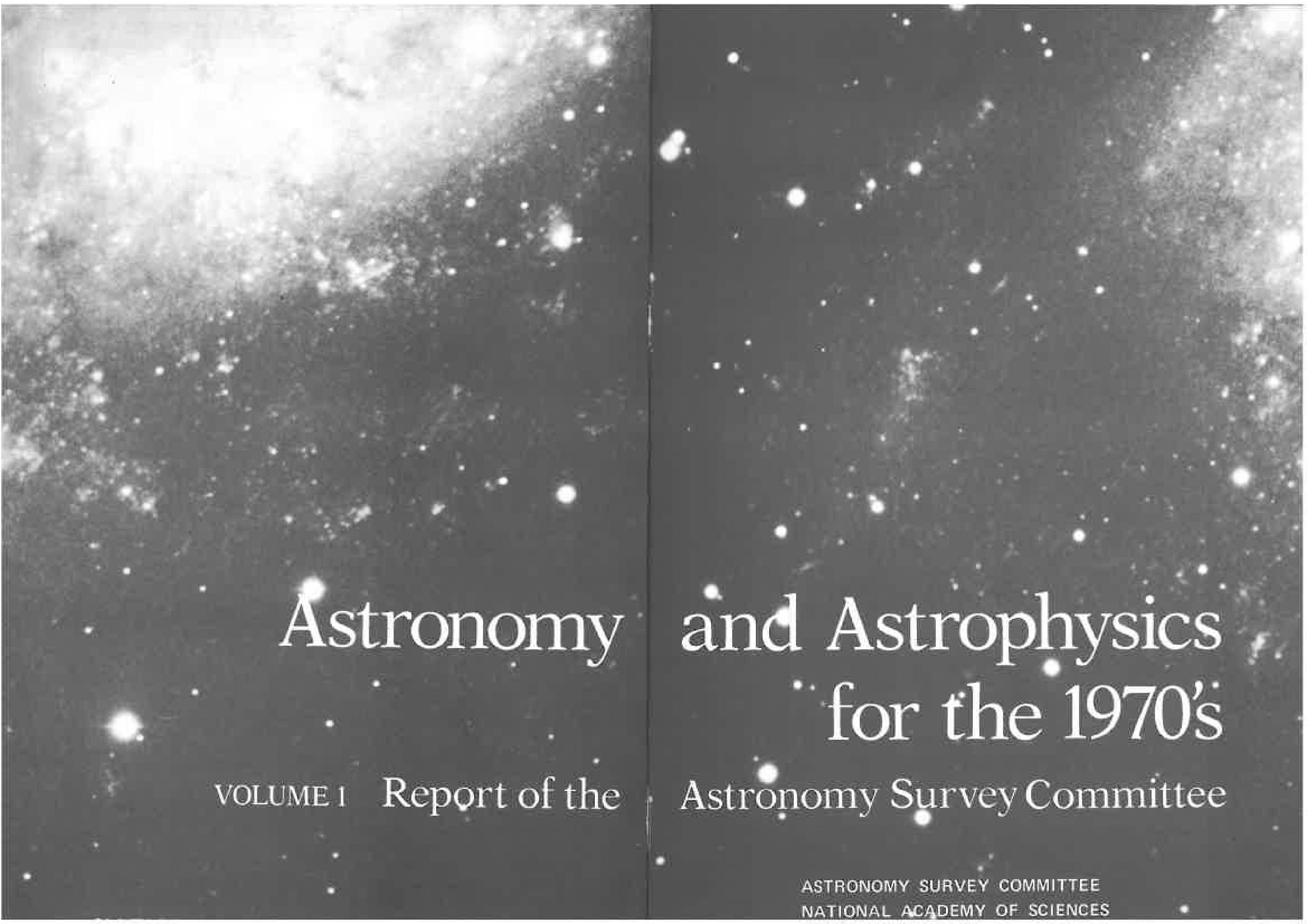

Astronomy and Astrophysics for the 1970's

VOLUME 1 Report of the Astronomy Survey Committee

ASTRONOMY SURVEY COMMITTEE
NATIONAL ACADEMY OF SCIENCES

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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.*)

April 1972

Dear Dr. Handler:

I take pleasure in transmitting to you herewith the final report of the Astronomy Survey Committee chaired by Dr. Jesse Greenstein. This report was reviewed by COSPUP at its October 1971 meeting and has been slightly revised in the light of the comments made to Dr. Greenstein on that occasion or subsequently in writing by individual members of the committee.

In the opinion of COSPUP, this report breaks new ground in a number of respects. It makes a serious attempt to identify priority programs over a very broad range of science, which embraces all of ground-based astronomy and that part of the space program that directly contributes to the resolution of primarily astronomical questions, excluding planetary exploration and manned space flight. All techniques of observation and calculation are treated within a common priority framework, and the priorities are oriented to scientific questions to be attacked by several observational techniques in parallel. Thus, for example, in connection with HEAO, the importance of associated expansion of ground-based optical and infrared facilities to identify x-ray sources observed from space is emphasized, and an intermediate optical telescope for this purpose is assigned to the same priority category as the HEAO itself.

In integrating a large part of space science into its priority scheme, this report goes well beyond *Ground-Based Astronomy: A Ten-Year Program* (the Whitford report) and should be particularly useful for planning purposes of the government at a time when a stronger attempt is being made to integrate NASA planning into the general national scientific effort. The report brings out very clearly the interrelationships of the several observational techniques and instruments and provides a good sense of the unity of astronomy and its increasingly close relationship to virtually every subfield of physics and, increasingly, to chemistry. It points out that many of the newer areas of astronomy are pursued by very young scientists having their original training in physics.

The report also brings out the remarkable vitality of the field, as evidenced both by the discoveries of the last five years and by the influx of new young and very talented research workers. This flowering has occurred despite a virtual moratorium on funding of major new equipment since the publication of the Whitford report, especially in the field of radio astronomy. Nothing could provide more cogent evidence of the ripeness of the field for major advances. Despite funding limitations, the United States still holds a commanding position, especially in the newer experimental areas of research, such as infrared and millimeter-wave astronomy, long-baseline interferometry, and high-energy astronomy. Moreover, because of the technological support programs, which have already greatly increased the efficiency of existing optical and radio telescopes, this country is in a superior position to exploit the new scientific opportunities opened up by recent discoveries made with relatively modest instrumentation.

One of the most striking conclusions of the report is the high probability it assigns to the existence of intelligent life elsewhere in the universe. This conclusion, so different from any that would have been reached only a few years ago, is largely a consequence of the entirely new picture of the origin of the stars and their attendant planetary systems. This picture has emerged from discoveries of the existence and properties of complex interstellar molecules and the prevalence of solid particles in regions of star formation, leading to the realization that planetary systems probably often accompany star formation. In this and other respects, astronomy ranks with molecular biology in its potential impact on man's philosophical conception of himself and his place in the universe.

In evaluating the report, COSPUP felt that the Astronomy Survey Committee had presented an extremely persuasive case for the scientific opportunities in the field and for the validity of the difficult priority choices it had made. The present rate of PhD production will supply enough new astronomers for the recommended expansion of effort, especially since there is much transfer from physics. The report does not predict where these PhD's will eventually find a place in the existing national programs. The future of the very large population of talented astronomers in the middle or late twenties is dubious without an expansion of federal support.

For reasons that are readily understandable, in view of the present enormous promise of the field, the report has given, perhaps, inadequate attention to the probable scientific consequences of more constricted fiscal support than is implied by even its first four priorities or to how the national program would be reoriented to minimize the damage from such austerity. Other COSPUP surveys under way have gone into considerably more detail on the consequences of limited budgets, and such an analysis may be needed to provide a fair comparison with other fields of science.

I should also like to call attention to some overlap between the astronomy and the physics surveys. In fact, there was a panel on astrophysics and relativity chaired by Professor George Field that reported to both survey committees. Physics and astronomy intersect in this domain, which is one of the most exciting and dynamic ones in both disciplines. The recommendations of the survey

committees are reasonably consistent with each other in their areas of intersection.

COSPUP has not attempted and cannot attempt, at this stage, to compare the priorities with those in others fields of science. Our endorsement of the report, therefore, should not be taken as unqualified endorsement of each element of the recommended program in competition with elements in other scientific fields. Astronomy has served as an ultimate testing ground of technology at very low light and signal levels. By most of the tests we consider relevant—scientific opportunity, ripeness for exploitation, availability of supporting technology, ability and recent accomplishments of the people in the field, philosophical and cultural impact—this program rates a high relative priority. Of course, its possible impact on technology or on the resolution of current societal problems cannot now be foreseen, but we believe that this fact is more than outweighed by its extraordinary scientific promise, which we consider the more pertinent consideration for this particular field.

Sincerely yours,

Harvey Brooks
for the
COMMITTEE ON SCIENCE AND PUBLIC POLICY

This comprehensive report of the Astronomy Survey Committee, like its predecessor report on ground-based astronomy, conveys the excitement and challenge inherent in the deepening understanding of the universe offered by modern astronomy. While making an impressive case for the values of astronomy itself, it also convinces us once more of the powerful connections among astronomy, the other natural sciences, and philosophy. Today, more than ever, "Astronomy is everyone's second science."

In recounting in so thorough and scholarly a way the promise of astronomy, the distinguished membership of the Astronomy Survey Committee has also revealed again the continuing promise of all science. For this we are most grateful.

PHILIP HANDLER

President,
National Academy of Sciences

Washington, D.C.
April 1972

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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 discussions begun in 1968, a 23-member Survey Committee was appointed in July 1969. Special 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. One thing that became apparent with the formation of these panels was the youth of the astrophysics community—the median age is less than 34. It also became apparent that the road to a career in astronomy is not only through graduate schools of astrophysics: whereas in 1966 only 26 percent of those now calling themselves astronomers received their PhD's in physics, this had grown to above 40 percent by 1970.

We are deeply grateful to the nearly 100 panel members who did much of the technical work and, particularly, to Richard Berendzen, Robert Doyle, Gerald H. Newson, and Terry P. Roark, who gathered and analyzed the statistical material that formed the basis of our study. Each panel report was critically reviewed by the entire Survey Committee, altered or returned for desired changes, with extensive discussions between the Committee and the panel chairmen. We felt that a frank and even corrosive review by scientists representing all fields of astronomy was the best method for sharpening the vision and reducing the priority list prepared by the specialists in each technique or field of study. We needed to achieve both a balanced program and an over-all priority assessment, over an enormous range of techniques.

While the final choice of items of the highest priority was made by the Survey Committee, it is weighted heavily by recommendations of panelists in each special technique or subject. This over-all priority assessment has not been reviewed by individual panel members and is the responsibility of the Survey Committee. It represents our best attainable consensus and provides a short list of major research and facilities goals that should be implemented for a well-rounded pursuit of current opportunities in astrophysics and astronomy. In the individual panel reports we give detailed lists of items of high priority as chosen by the individual panels. Clearly, as techniques improve and discoveries are made, these technical panel reports will provide important guidance. As the science develops, we may also expect changes of emphasis and new fields to appear.

Astronomy has become a fruitful area of modern experimental physics. The universe has provided a laboratory with extreme conditions of temperature and density to test (if not to strain) the laws of physics under most unusual circumstances.

The discoveries in this strange universe remain, at least in part, communicable to the educated, nontechnical public. Some 52,000 undergraduate students take an astronomy course each year. This is often the only science course they will take, so that astronomy remains basic to the communication of science to its ultimate audience—the public. We hope that for them, as for us, this Survey will provide an exciting insight into the larger and beautiful world in which we live.

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*

Contents

Astronomy is useful because it raises us above ourselves; it is useful because it is grand; . . . it shows us how small is man's body, how great his mind. His intelligence can embrace the whole of this dazzling immensity, in which his body is only an obscure point, and enjoy its silent harmony. Thus we attain self-insight, something which cannot cost too dear, since this insight makes us great.

HENRI POINCARÉ, *1903*

CHAPTER ONE

Introduction

For thousands of years men have looked into the sky, long with wonder and fear but eventually with comprehension. The regular motions of the sun and stars, the wandering of the moon and the planets, provided early insights into cause and effect and the regularity of nature. With understanding came the hope of controlling nature and the beginning of science and of technology. In this century, the rapid growth of science and technology has increased the depth of our insight and also our wonder; although we have found much, we still have too few explanations. We know from past history that much new is yet to be found. Navigators and explorers of the terrestrial globe found new continents inhabited by strange and different peoples. The explorers of the sky, however, have an almost unlimited sample of nature to study. They have found not merely interesting new details about individual stars or other objects but entirely new classes of objects undreamed of ten years ago. As each new technology was applied to study light (photons) of different colors or energetic particles of different charge and mass (cosmic rays, neutrinos), new types of worlds were revealed. The previously well-organized universe, which for ancients was a planetary system centered on the earth, exploded into a bewildering universe of new types of objects, large and small, with exotic new names and marvelous new natures. Technology, theoretical insight, deeper understanding of the properties of matter, and the large computer, together with hard work, have made the last decade of astronomy one of the truly greatest periods in its history. Man has landed on his first planet—the moon. But his mind and eye have traveled billions of light years into the past and in the next decade will penetrate unimagined new worlds.

Astronomy is a union of the science of the very small and of the very large. Astronomers are interested in the properties of nuclei, atoms, molecules, solids, planets, interstellar matter, and stars. But stars are themselves units in larger aggregates—galaxies (Milky Ways) that agglomerate into clusters and extend throughout space as far as we can probe. There are more stars in our Milky Way (200 billion in number) than there are people who ever have lived on earth. How many other "earths" and what other types of intelligent beings exist out there? There are probably nearly as many galaxies in the observable universe as there are stars in our Milky Way. What strange new types of objects do they contain? Are there forces and energies at work that we do not yet know of? We are bathed from all directions by weak radio signals, apparently a remnant of the creation of the universe, degraded from an enormous burst of light at the beginning of time ten billion years ago. What was it like then? Does time stretch backward forever, or was there a beginning? What, if anything, came before? Where do energy and matter come from? Is the total amount of energy and matter constant in time? The astronomer's daily life deals with such difficult questions. How many other planetary worlds are there, are they inhabitable, and are any inhabited? How long will the sun shine and the earth survive?

The actual universe is stranger than that of science fiction; its exploration is one of the nobler adventures of the human mind. Like a child at play, the astronomer busies himself with strange toys—white dwarfs and red giants, pulsars and quasars—and in his theories moves the building blocks of atoms about to model something like the world he sees, with an imagination and a courage like a child's but with the resources of modern science and technology. It is fortunate that the pleasure he finds in the profession and the excitement of his discoveries are joys of science that are still communicable to the public. The optimism accompanying the exploration of our own West ended with the disappearance of the frontier; but exploring the external frontier of the heavens is endless, and its reward is knowledge, a more humbling wealth.

Most astronomers have worked from the ground, through a thin but unsteady atmosphere, using visible light, near- and far-infrared radiation, and radio frequencies. With technological development of balloons, rockets, and satellites, we first rose above our turbid atmosphere to observe the different radiation from the sky—gamma rays, x rays, and ultraviolet radiation—that are invisible from the surface of the earth. Soon we will observe stars and galaxies extensively at the far-infrared wavelengths that are also blocked from reaching the surface of the earth, as are long radio wavelengths. We will grow high-accuracy, high-resolution, x-ray eyes. The high cost of space technology and the initial relatively short life of satellites, compared with the cost and lifetime of ground-based instruments, make astronomy in space seem risky and expensive but fully rewarding in its fantastic panoply of discovery.

The present survey of astronomy attempts to cover the entire range of astronomy, ground-based and space-based. Allowing for the broadened scope of enquiry, we report on a wider range of topics than did the Whitford report. The scientific hopes expressed in that report underestimated what has been found in seven years. The facilities proposals of the Whitford report have in part been fulfilled by construction of two 150-in. telescopes for the national observatories in the northern and southern hemispheres. In radio astronomy, the large facilities proposed were not built. Nevertheless, radio astronomy made startling advances because of the ingenuity of the observers and the success of higher-frequency receivers. Especially active now is the new field of the chemistry of interstellar space; in a brief period, 22 new molecules and 7 isotopes were discovered. Modern technology, the large computer, and existing facilities combined to produce much of this recent progress. The universe is much more surprising than we thought it would be; technology improved more than was expected in that period. Objects were found that had not been thought of, and explanations based on new fields of physics were advanced. We firmly expect that the decade for which our program is prepared will be at least as exciting. We can almost certainly guarantee that there will be more surprises than we contemplate and even more novel explanations of the mysterious universe into which our eyes and minds are just beginning to penetrate.

There has been at least one central theme of the last seven years, which makes it like the age of Galileo. It is the discovery of the existence, almost omnipresence, of a high-energy, explosive universe. Much of our program is concerned with this new and violent world. Two universes coexist—hot and cold. The "hot" involves phenomena of explosion; very high temperature; energetic cosmic rays; strange events in galaxies; new types of hot, dense, possibly young galaxies. By "cold" we mean ordinary stellar and interstellar matter, with temperatures from 50 million degrees down to a few degrees above absolute zero. Radio, cosmic-ray and x-ray observations, gravitational waves, and large parts of optical and infrared astronomy relate to the hot universe. Stellar, galactic, and extragalactic astronomy, in the large, refer to a cold universe. Much of what is found in the infrared points to even colder stars, dust, and gas than we had expected would exist. The most rapid and currently exciting growth is in the realm of the hot universe. But over 90 percent of the mass of stars is in cool K and M dwarfs, a hundred times fainter than the sun, which can live uneventfully for hundreds of billions of years.

From the time of the ancient Greeks to the mid-twentieth century, the universe was conceived as an unchanging, or at best slowly changing, cosmos of fixed stars. The first few decades of this century replaced this view with a steadily expanding universe of galaxies—each galaxy a majestic, slowly rotating collection of stars intertwined with dust and gases. Astronomers searched for origins and life histories of stars (including our sun) with time scales of billions of years.

The last decade of exciting discovery has added to that picture a general cosmic violence, exploding galaxies and quasars, an almost universal presence of high-energy particles and magnetic fields, and events suggesting relativistic collapse. Much of this new knowledge derives from years of observations with radio and optical telescopes and interpretations with theories from modern physics.

The discovery of these explosive events has been made possible by a dramatic growth in the tools and techniques of observational astrophysics. In 1972, we can observe the universe in virtually any part of the electromagnetic spectrum, from gamma radiation to long-wave radio radiation. We measure flux of high-energy particles and may even have detected neutrinos and gravitational waves. In 1963, the Whitford report addressed itself to the needs of optical and radio astronomers, with a sidewise glance at the field of space astronomy, then barely under way. The present study evaluated research programs using the complete range of observational techniques, many of which did not exist a decade ago. Space-based gamma-ray telescopes are measuring radiation from the center of our galaxy. X-ray instruments are observing a pulsar and many variable, mysterious x-ray stars. Some disturbed galaxies are strong x-ray emitters. An ultraviolet stellar space observatory has operated for four years, yielding new information on a previously inaccessible region. Advanced electronic methods of photon detection are used on optical telescopes to make observations in minutes that required hours a decade ago, thereby making possible an order-of-magnitude increase of information on very faint objects. Cryogenically cooled bolometric and solid-state devices fly in balloons and stratospheric aircraft, to measure invisible infrared heat photons. They find that some energetic objects radiate most of their power at these wavelengths. Millimeter-wave radio telescopes have discovered completely unexpected complex organic molecules in the interstellar medium, not unlike essential constituents of biologically active terrestrial molecules. Centimeter-wave radio telescopes halfway around the world from each other operate together as interferometers to give much higher resolution than the finest optical photographs. They reveal features in the structure of quasars that change their appearance in a few months. Radar systems make topographic maps of the invisible surface of the planet Venus, detecting mountains less than 1 km in height. Space solar telescopes study the outer solar atmosphere where the temperature suddenly rises to tens of millions of degrees in hot spots in the extended solar corona.

What do we say about the future? Chapter 2 contains our major recommendations, as selected from the extensive programs of our technical panels. These programs are presented in full detail with scientific justification in Volume 2 of this report. The cost breakdown of new programs follows, roughly separated into space and ground-based and by experimental or theoretical techniques. The costs of the new programs given are incremental to those of the existing programs of the National Aeronautics and Space Administration, the National Science Foundation, the Department of Defense, the Smithsonian Institution, and private and state universities and foundations, details of which costs are found in Chapter 4.

One essential aspect of the future should be discussed prior to consideration of financial requirements. If the epigraph of these pages has any meaning, it is that the human mind vivifies science, creates and uses the instruments, rides in space, guides the telescope, the computer, or the theoretician's pencil. Does the required high-quality manpower exist? Do we have too much? The details of our study (see Chapter 4) show that the quality is high indeed, the diversity of training and experience wide, and the numbers adequate. But any further increase in the rate of production of new PhD's or of the numbers of institutions offering that degree should be encouraged only as an increase in the foreseeable employment demand justifies it. The excitement of the field of astronomy has been a major factor in attracting many brilliant young men and women into the ranks of its professionals. The rate of production of new PhD's in astronomy has increased by a factor of 10 in the last decade. At latest count more than half of the PhD's working in astronomy received their degrees in other disciplines—primarily in physics, recently some in chemistry. All in all, the *current* rate of production of highly qualified astronomers is better than adequate to meet the foreseeable needs of this country during the next decade, even if our entire set of new programs recommended in this report were to be carried out on schedule. The supply would be adequate, indeed, for the demands of known astronomy-related areas, as well as a reserve for new ones that might open up in the coming decade.

The failure of federal support to increase significantly during the past few years has had a particularly great impact on the support for young astronomers. In addition to large-scale facilities, we most strongly recommend increased funding of modest research grants to young people, as well as support of optical telescope auxiliaries and novel experimental facilities and programs in radio astronomy. We hope that this program will permit smaller research groups and younger scientists to remain competitive in some areas with larger institutions.

From our entire study, we conclude that a balanced effort is essential. It must contain ground-based optical and radio telescopes and auxiliaries, which greatly increase their efficiency. There should be a well-planned space-astronomy program, one ultimate goal of which is a large space telescope. It must provide adequate computational facilities for the theoretical astrophysicist. New and fruitful opportunities will be opened by new technologies in all areas. We conclude also that both large national centers and strong university groups are essential for health, balance, and innovation.

Finally, we consider costs and tradeoffs. We find that our recommendations require a continued growth, in constant dollars, of about 5½ percent a year, if we are to take advantage of the opportunities before us.

CHAPTER TWO

Recommendations for a Major New Program in the Next Decade

The explosion of knowledge in astronomy demonstrates how extremely difficult it is to predict directions that the science will take in the next decade. If astronomy is to continue to make progress, new facilities must be constructed and new directions in research must be pursued. What facilities are most needed, and what directions would seem to be most profitable? To obtain the broadest possible view of the options available, the Committee formed panels that eventually involved some 100 astronomers.

The panel reports (published in Volume 2) provide a comprehensive review of the past and projections for the future of astronomy from the point of view of the constituent disciplines. The Committee's task was to review these reports and produce a coherent program for the next decade. The Committee selected approximately 30 items and, to the best of its ability, established an order of priority, weighing in its decisions the issues of scientific promise, technological state of the art, availability of funding, and availability of skilled personnel. It was necessary to come to grips with an enormous range in the size and cost of programs, from modest ground-based instruments to the most sophisticated space experiments.

While costs are measurable, evaluating the importance of various programs becomes an exercise in comparing incommensurables. A theoretical program may yield a new concept inexpensively, and a space telescope may uncover a new kind of object at relatively great cost. Unique concepts and information are obtained in many ways, and a balanced over-all program contains contributions from programs of various sizes.

In spite of the diversity of interests and specialties of its membership, the Committee succeeded in defining with remarkable unanimity four programs of highest priority. In order of importance, these are

1. A very large radio array, designed to attain resolution equivalent to that of a single radio telescope 26 miles in diameter; this should be accompanied by increased support of smaller radio programs and facilities at the universities or other smaller research laboratories;

2. An optical program that will vastly increase the efficiency of existing telescopes by use of modern electronic auxiliaries and at the same time create the new large telescopes necessary for research at the limits of the known universe;

3. A significant increase in support and development of the new field of infrared astronomy, including construction of a large ground-based infrared telescope, high-altitude balloon surveys, and design studies for a very large stratospheric telescope;

4. A program for x-ray and gamma-ray astronomy from a series of large orbiting High Energy Astronomical Observatories, supported by construction of ground-based optical and infrared telescopes.

The following items were also identified as being of high scientific importance, but the Committee agreed that their funding, although urgent, should not create a delay in funding the above items:

5. The construction of a very large millimeter-wavelength antenna to identify new complex molecules, to study their distribution in interstellar space, and to study quasars in their early, most explosive phases;

6. A doubling of support for astrophysical observations from aircraft, balloons, and rockets, at wavelengths ranging from the far infrared to gamma rays;

7. A continuation of the Orbiting Solar Observatories through OSO-L, -M, *and* -N, *together with an updating of existing ground-based solar facilities;*

8. A sizable increase of support for theoretical investigations, including an expansion of capability for numerical computation;

9. An expanded program of optical space astronomy, including high-resolution imagery and ultraviolet spectroscopy, leading to the launch of a large space telescope at the beginning of the next decade;

10. A large, steerable radio telescope designed to operate efficiently at wavelengths of 1 cm and longer to obtain observations with high angular resolution and record emission from more distant objects than is now possible;

11. Construction of several modern astrometric instruments at geographic locations chosen to permit systematic measurement of accurate positions, distances, and motions in both northern and southern hemispheres.

These 11 programs are described in Chapter 5.

Funding of less than the above 11 programs would seriously impede our efforts to capitalize on the recent past. Our studies evaluated what is possible from current technology and what is important scientifically. To implement less than this program would constitute a retrenchment to below the rate of progress recently established in astronomy.

The Committee has been mindful of costs in the present time of restricted availability of funds. In light of the outstanding progress of recent years, we feel that this program represents a relatively modest increase over current funding. Such increases are justified, in any area of basic research, by both their extrinsic and intrinsic rewards. Astronomy and astrophysics represents one of the most rapidly advancing frontiers of human knowledge. The 11 items above, if implemented during the next decade, would result in a growth rate in funding of astronomy of approximately 5.5 percent per year. A truly desirable program, unencumbered by such severe constraints in growth rate, is discussed in the final section of Chapter 5.

A complete list of major panel recommendations, which formed the basis of the Committee's deliberations, will be found in Volume 2 of this report. Each set of recommendations represents a thoughtful assessment of the potential for development of that discipline during the coming decade.

The projects listed above do not constitute a total program for astronomy. They include only new initiatives and not ongoing operation and maintenance, research projects, support for individual universities and projects, or the support of the national centers. The construction of a major new facility also involves operating expenditures, which we estimate to be roughly 10 percent per year of the total capital cost of the facility. The existence of a major new facility will also mean an increased opportunity for the scientific community, an opportunity that can only be realized if expanded project support is available. That is not to say that all the work at a new facility will be supported by increasing the level of project support; some support will undoubtedly come from redirection of effort within the present program. However, since the present level of effort is already inadequate to support the demand, the rate at which new facilities are built and progress made will depend critically upon the rate at which support for individual projects grows. This problem is common to all the basic sciences in this country, and ultimately funding decisions must be based upon the needs of different sciences and their value and interest to society.

NEW PROGRAM COSTS

SPACE		
A series of four High Energy Astronomical Observatories (Recommendation 4)	$380 M	
Increased observations from aircraft, balloons, rockets (Recommendation 6)	13 M/yr	
Continuation of Orbiting Solar Observatories, OSO-L, -M, -N (Recommendation 7)	(15 M/yr)*	
Program of optical astronomy in space, leading to a Large Space Telescope after the next decade (Recommendation 9)	(35 M/yr)*	
TOTAL NEW SPACE		$510 M
GROUND-BASED		
Radio		
The Very Large Array (Recommendation 1)	$ 62 M	
Very Large Array operations	6 M/yr†	
Expansion of university radio facilities	2.5 M/yr	
Large Millimeter-Wave Antenna (Recommendation 5)	10 M	
Large Millimeter-Wave Antenna operations	1 M/yr†	
Large Centimeter-Wave Antenna (Recommendation 10)	35 M	
Large Centimeter-Wave Antenna operations	3.5 M/yr†	
Total New Radio	$185 M	
Optical		
Electronic auxiliaries for large optical telescopes (Recommendation 2)	$ 15 M	
Test of multielement optical array concept	5 M	
Three 100-in. class telescopes—infrared, x-ray support, conventional	15 M	
Large optical array or 200-in. telescope	25 M	
New optical telescope operations	4.5 M/yr†	
Total New Optical	$ 83 M	
Infrared		
Doubling support for infrared astronomy—ground-based, aircraft, balloons, rockets, laboratory (Recommendation 3)	$ 2 M/yr	
Total New Infrared	$ 20 M	
Solar		
Improvement of existing ground-based facilities (Recommendation 7)	$ 1 M/yr	
Total New Solar	$ 10 M	
Theory		
Increased support for theoretical investigations, including expanded capabilities for computing (Recommendation 8)	3 M/yr	
Total New Theory	$ 30 M	
Astrometry		
New astrometric instruments (Recommendation 11)	$ 6 M	
Total New Astrometry	6 M	
TOTAL NEW GROUND-BASED		$334 M
TOTAL NEW CAPITAL AND OPERATING PROGRAM		$844 M

*Continuation of ongoing programs at near present level—no increase over current expenditures.

†Estimated contribution to total assumes 5 years of operation during next decade.

CHAPTER THREE

Astrophysical Frontiers

COSMOLOGY

For centuries man has struggled to gain a broader and deeper understanding of the world around him. Studying forms of life, he has come to appreciate, on the one hand, the fantastic diversity of living things and, on the other, the extraordinary similarity of the minute cells that constitute all organisms. Digging into the earth, he has discovered layer after layer of rock, laid down by unseen processes in the remote past. Peering through the telescope, he has probed far beyond the planets to find billions of stars just like the sun, clustered into vast galaxies, which themselves stretch without number into the depths of space.

The earth and living things upon it evolve. Each year there are small changes in the landscape as sea, rain, and wind reshape the earth. Each year there are imperceptible changes in the species as genetic mutations propagate into new generations. Thus change and evolution are the basic themes of geology and biology.

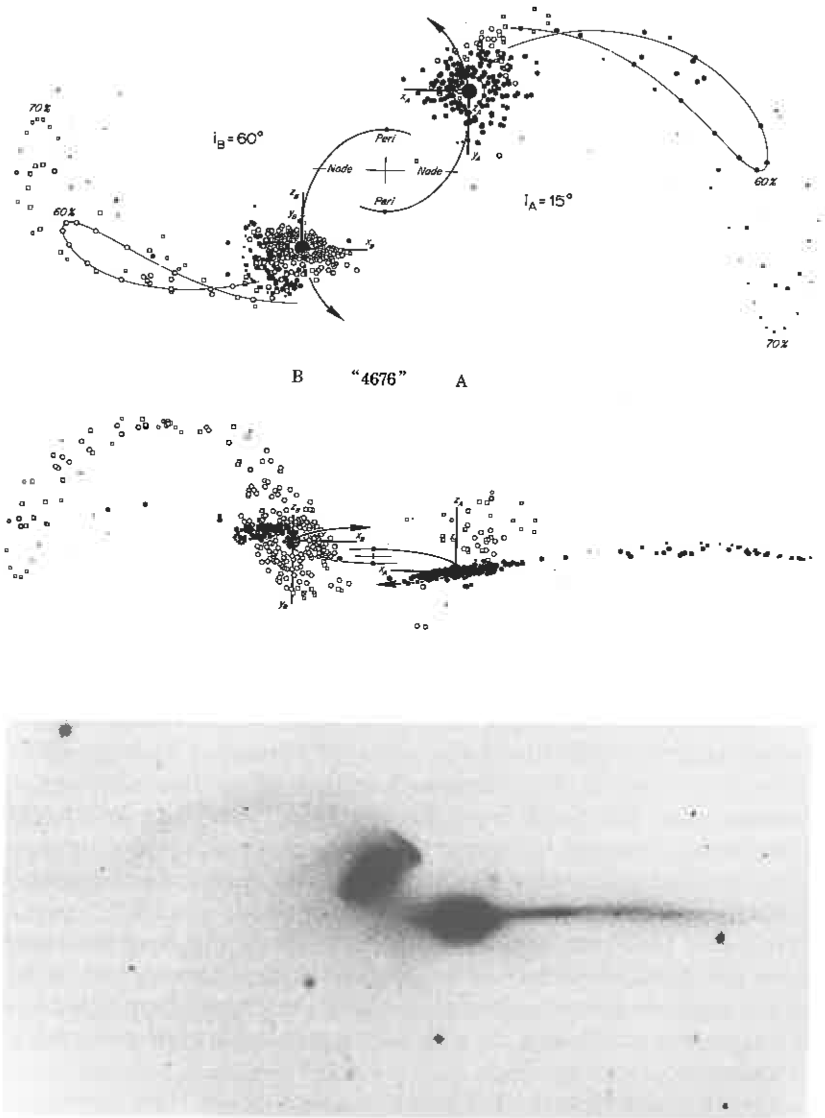
The early astronomers perceived an opposite tendency—the apparently unchanging quality of the stellar universe. But in the twentieth century when astronomers began to apply the laws of physics to the stars, they realized that the prodigious energy stars emit cannot be sustained indefinitely. Even with the most efficient nuclear power, based on the fusion of hydrogen into helium nuclei, stars like the sun can shine at most about 10 billion years. In spite of the apparent steadiness of a star's light, evolution must be occurring as the structure of a star responds to the loss of its energy supply. So astronomy has followed biology and geology in perceiving the importance of evolution. Even the time scales—billions of years—suggested by the astronomers and geologists for the evolution of the earth and stars are similar.

The great optical telescopes of the Western United States have shown that space is filled as far as they can see with galaxies rather like our own. Too faint to be seen with the naked eye, the most distant ones at 5 billion light years take many hours to register on film even with telescopes 10 million times as sensitive as the eye. The galaxies are cities of stars, crowded together in nearly empty intergalactic space. They appear to contain most of the matter and to be the building blocks of the universe. But we do not really know, yet, how much matter may be found between them or whether galaxies are still being formed.

When Slipher studied the spectra of galaxies in the 1920's, he was astounded to see that the spectral lines of most galaxies were shifted to the red, indicating that these galaxies seemed to be moving away from us. Why would the galaxies recede from us? From the time of Copernicus, man had learned to suspect the explanation of any phenomenon that required the earth to be the center of things. Unknown to the astronomers working on this problem in the 1920's, a young Russian mathematician, Friedmann, was constructing a model of the universe based on Einstein's theory of general relativity. The model, he reasoned, should be uniform, with equal numbers of galaxies everywhere. Otherwise, there would be a center, contrary to the Copernican principle. But when he applied Einstein's equations, he found, as did Einstein, that his model universes could not sit still. Friedmann's models expanded, either indefinitely or for a finite time, after which they collapsed to a point. The reason for this behavior is gravitation—the basic force in Einstein's theory—which continually tries to pull the galaxies back on themselves. The only way this tendency can be withstood is to give the universe a mighty kick outward at the beginning. Whether the universe expands forever (open models) or eventually collapses (closed models) depends on the degree of gravitational pull and, hence, on the mean density of matter.

Friedmann predicted that all the galaxies should be observed to be moving systematically either toward or away from us (contraction or expansion, depending on the time elapsed). Furthermore, he pointed out that to preserve uniformity, the more distant galaxies must move faster with respect to us.

Lemaitre, Robertson, and Tolman, among others, had developed various predictions of general relativity. Following Slipher's pioneering efforts, the extensive new data on velocities and distances of galaxies assembled by Hubble at Mt. Wilson supported the theoreticians of general relativity. Not only are all the galaxies moving away from us, but they obey



Understanding observations often requires numerous calculations. Here two views of a computer-generated model of an encounter between two galaxies are compared with a photograph of NGC 4676. (*Photo courtesy of NASA Institute of Space Studies and Hale Observatories.*)

Friedmann's predicted proportionality between distance and red shift. For many years Hubble and his collaborator, Humason, extended these measurements with many instruments and techniques. Successors verified this relationship with great accuracy out to enormous distances (5 billion light-years) and red shifts (about 0.5, corresponding to nearly half the speed of light). The velocities are more easily determined than the distances; with the present scale, from the proportionality constant in the Hubble law, one can calculate an age of 10 billion to 20 billion years. If no other forces were acting, this would be just the time for the observed galaxies to attain their current distance at their present speeds. Actually, because gravitation continually slows them down, the age of the universe is somewhat less. In any case, the time scale of billions of years is comparable with that of the age of life, of the earth, and of the stars.

Much of what we now know about the galactic universe is consistent with Friedmann's predictions. The galaxies are distributed uniformly, they move apart, and they almost always obey Hubble's law relating velocity and distance. Some exceptional peculiar galaxies seem not to obey the Hubble law, having quite different red shifts from galaxies quite close to them in the sky. This puzzling phenomenon defies explanation at present. Astronomers using the largest optical telescopes have therefore been straining to determine which of the Friedmann models describes the universe. Do we live in a closed universe, with an inevitable collapse scheduled for the future, or are we in an open universe, which will expand forever? Or do we live in a steady-state universe, quite different in nature from other relativistic models?

Because of the finite speed of light, as one probes deeper into space, one is looking back into time. One therefore sees galaxies moving as they did in the past, and that motion is slightly different for different models. Unfortunately, these effects are discernable only for red shifts of the order of unity and, hence, for speeds approaching that of light, and there just are not enough galaxies of large red shift observed to discriminate between rival cosmologies. Construction of one or more additional large optical telescopes and instrumenting all large telescopes with fast electronic systems will permit progress on this problem.

Radio astronomers in the 1950's discovered distant, strong radio sources like Cygnus A, which is at a distance of 500 million light-years. Because of great improvements, present radio instruments can detect sources like Cygnus A at enormous distances. The Friedmann model predicts a maximum distance of roughly 10 billion light-years—the distance light can go in the age of the universe. Objects at that distance would have extremely large red shift and could be useful for determining the model of the universe. Unfortunately, even though tens of thousands of faint radio sources have been pinpointed, relatively few of the radio galaxies among them have proved to have large red shifts. This may be because most of them are intrinsically faint and rather close. On the other hand, there is some controversial evidence that the number of distant radio galaxies is smaller than one would predict from uniform density—it is as if, in the evolution of the universe, radio galaxies simply did not exist before a certain cosmic time.

A subclass of radio sources, the quasars, is much more puzzling. Extremely small in comparison with normal galaxies, these objects are observed to emit large fluxes of radio and optical energy and in some cases infrared and x radiation as well. Generally, they have large red shifts—up to nearly 3 in one case. If they are at the huge distances indicated by their red shifts (as supported by observations of one quasar in a group of galaxies having the same red shift), they must be emitting unprecedented amounts of energy. The physics by which such immense energy would be released is completely obscure but may be connected with violent events in very massive general-relativistic configurations of matter.

Some scientists believe that the quasar red shifts are not of cosmological origin, noting that some quasars seem to be located on the sky close to objects believed to be at relatively small distances. One quasar is connected by a luminous bridge to a relatively nearby galaxy with a different red shift. It is difficult with this view to explain the large observed red shifts. If they are Doppler shifts, it is strange that they are always positive. Unknown and extraordinary processes must operate in order to accelerate huge masses of matter to relativistic speeds.

Whether quasars are cosmological or not, they pose serious problems for current physics. It is possible that solution of these problems will require almost revolutionary new ideas in cosmology.

In 1965, a truly sensational breakthrough occurred in cosmology. Scientists of the Bell Telephone Laboratories, in attempting to eliminate all sources of noise from a sensitive radio telescope, concluded after a year of effort that a very faint signal from space was confounding their best efforts. They estimated its intensity at their operating wavelength of 7 cm to be equivalent to that of a blackbody of 3 K. It seemed to come equally from all directions, as expected for a cosmological effect. Hearing of this discovery, scientists recalled that Gamow, a cosmologist, had predicted such an effect in the 1940's. Reasoning that if the early phases of the universe were very dense, they likely were hot, we would expect the radiation from the primordial fireball to still be visible today as we look back in time to the "big bang," as Gamow called the fireball.

A key point is the spectrum of the radiation. Gamow predicted that it should be truly blackbody, like that of a star or an incandescent light bulb. So far this has been verified with roughly 20 percent accuracy in the region from 21 cm to 2.6 mm, although some discrepancies may have been observed in the far infrared. The best estimate of the temperature is 2.7 K, and the radiation is uniform in all directions to within 0.1 percent, as predicted.

Discovery of the cosmic blackbody radiation has given a tremendous impetus to cosmology. If this radiation was produced in a big bang, it may have last interacted with matter when the universe was only 100,000 years old and only 1/1000 of its present size. At that time, matter had a temperature of 3000 K. We can think of the radiation as propagating from a "cosmic photosphere," rather like the surface of the sun but, because of the expansion of the universe, receding from us at a speed differing from the speed of light by only one part in a million. If it were not for this recession, and the consequent red shift of the photons from the visible to the radio range, life on earth would be impossible because of the intense heat from this fireball.

Unfortunately, the differences in the spectrum of the cosmic blackbody background associated with different Friedmann models are extremely small, so there seems to be little hope of determining the correct cosmological model by such observations. On the other hand, nuclear physicists have shown that the temperature of the radiation is related to the nuclear reactions that would have taken place in the fireball. Indeed, subject to certain qualifications, they predict that about a quarter of the hydrogen should be converted to helium if the radiation temperature is 2.7 K. The sun and most of the nearby stars contain about that amount of helium and so does the interstellar gas, both in the Milky Way and in nearby galaxies. The trouble is, some of this could have been produced in stellar interiors, and what is needed therefore is an assessment of the helium content of old stars born at the same time as the galaxy. So far, the indications here are contradictory, and further attacks using optical methods are necessary.

While cosmic blackbody radiation is of great significance, recent experiments have cast some doubt upon the most straightforward interpretation. Difficult measurements are now in progress at wavelengths of 1 mm and below, where atmospheric attenuation makes it imperative to use balloons and rockets. Until these are completed, the cosmological interpretation is uncertain.

Another phenomenon of cosmological interest is the diffuse x-ray background, now observed over a wide range of energy, from 0.1 to 1000 keV. The spectrum in the 1–1000-keV range is described by two power laws joining at about 10 keV. An additional component below 1 keV may be due to emission from a hot gas at 3,000,000 K. As yet the location of this gas is unknown—for example, it could originate in our own galaxy—and detailed maps made from large spacecraft will do much to clarify this. One hypothesis is that it is distributed uniformly in intergalactic space, where it is heated by the fast particles ejected from quasars. If this is so, it is of great importance, because the required density of gas is calculated to be about equal to that which would close the universe and yields a mass that is a factor of 30 more than seen in galaxies.

As a result of these discoveries, the outlines of a possible evolutionary history of the universe are becoming visible, although, as we have pointed out, some puzzling phenomena remain.

First, there was a big bang about 10 billion years ago, which flung matter out with tremendous speed. During the first few minutes it was so hot that part of the hydrogen fused to helium, which is still seen in our galaxy today. After 100,000 years, the gas had cooled to about 3000 K, and the radiation that we now detect as the cosmic blackbody radiation was launched on its way. As the gas cooled, clumps were then drawn together to form galaxies; among them our own Milky Way was born. Some of these galaxies exploded, and by looking back in time with large radio telescopes we can now detect these events as quasars and radio galaxies. The fast particles that they ejected heated the intergalactic gas that still remained from the big bang and that may provide the bulk of gravitation of the universe. The x rays from this gas may be what we see today. As each galaxy settled down, generations of stars were born, among them the sun about 5 billion years ago, which was about 5 billion years, at least, after our galaxy was formed. Countless planets came into being. Geological processes churned up the young planets, and on our small, rocky world, with water and an atmosphere, eventually life emerged about 3 billion years ago. Life evolved, here and perhaps elsewhere, to a state of intelligence.

Through the vast reaches of space and time, part of the matter of the universe has evolved into living matter, of which a tiny part is in the form of brains capable of intelligent reasoning. As a result, the universe is now able to reflect upon itself. In this respect, at least, the whole evolutionary chain of events is endowed with meaning.

We see a major task for astronomy in the next decade to test this broad picture of the evolution of the universe critically at every point. Of course, flaws may be found, and even the main concepts may be found to be seriously in error. There are critics who find that the available data move them to suggest more radical cosmologies. Some question the interpretation of the red shifts themselves. Others argue for creation of new matter in explosive small objects. Intergalactic matter has not yet been found, but it may be detected through its x-ray emission. Apparently recently formed galaxies, or even groups of galaxies, do exist. Perhaps some type of "new physics" may yet be required to understand the universe of cosmic rays, radio sources, and quasars. But with or without such an upheaval, the cosmological questions remain of central importance.

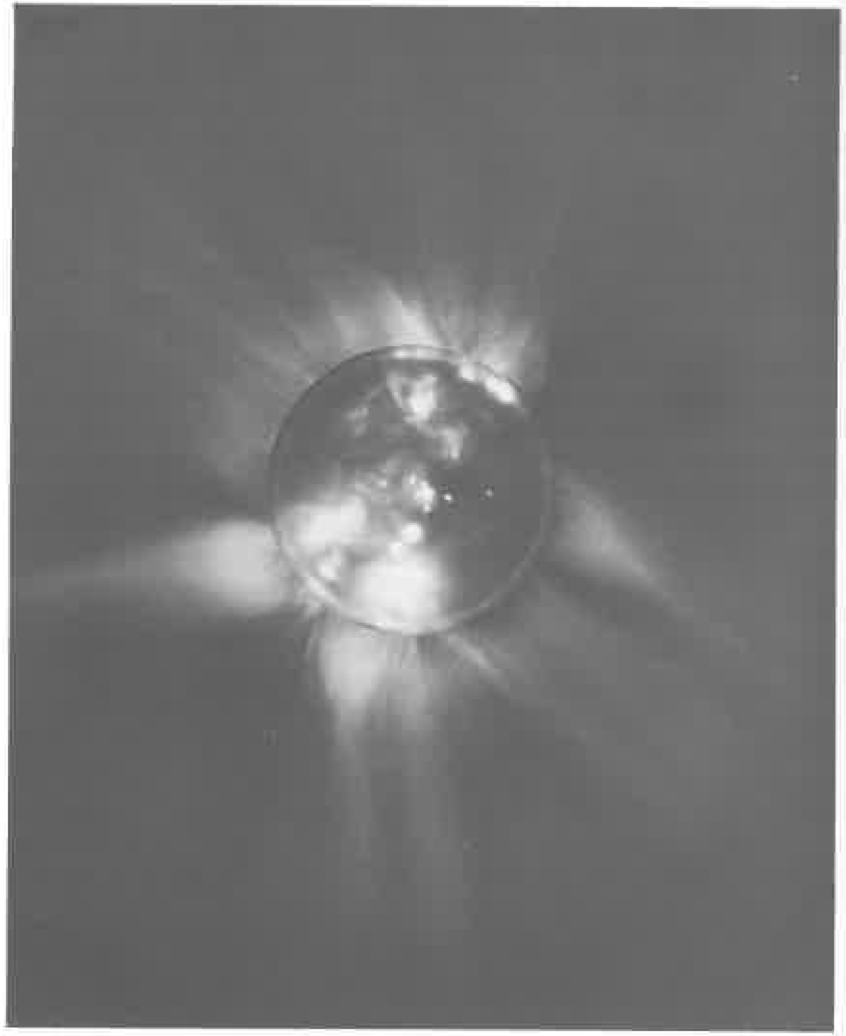
Even if the basic picture survives, we are left with an extraordinary puzzle. Earlier, we spoke loosely of the big bang—the fireball at the beginning of time. Einstein's equations tell us that matter was extremely dense and hot at that moment—so much so that the equations themselves break down. Basic problems of particle physics and gravitation remain to be solved before we can describe adequately what happened at that moment and understand why there is no meaningful way to discuss what preceded it. In the final analysis, the same questions of beginnings and endings will remain. The contribution of cosmology may well be simply to push back the limits of our ignorance to the basic problem of creation itself.

THE SUN

Among all the astronomical objects, the sun occupies a very special place for mankind. As the prime source of our energy and light, it is indispensable to our existence. The earth, moving in its orbit, is immersed in the outer atmosphere of the sun. Solar physics has, therefore, a practical aspect not shared by all areas of astrophysics. Within astrophysics, the sun occupies a unique position as the closest and, therefore, brightest star. It can be studied in far greater detail than any other star. Knowledge gained from studying the sun therefore often has direct application to the understanding of the rest of the universe.

Observations of the solar chromosphere, solar flares, and magnetic fields stimulated the search for these phenomena in the stars. Analogs have been found, and in some stars the effects are present on an enormously greater scale. The solar cycle has parallels in many stars, and measurements of the intensity of chromospheric activity lead to a method for age-dating stars. The magnetic fields in neutron stars are enormously large and play a fundamental role in the plasma physics of these objects.

Major areas of physics have found important applications in solar physics and have developed in new directions because of this interaction. The interpretation of the complex absorption line spectrum of the sun led to the theory of complex spectra and determination of the selection rules and the effects of magnetic and electric fields that culminated in quantum mechanics.



The solar corona photographed in visible light and x rays—a montage showing the relationship between ground- and space-based observations. *(Photo courtesy of American Science and Engineering, Inc., and High Altitude Observatory.)*

The second most abundant element in the universe was actually discovered through spectroscopy of the sun and consequently was named helium. Today, the question of distribution of helium in the universe is one of the most important cosmological questions. The best present-day determination of helium abundance comes from a chemical analysis of the composition of the solar wind and cosmic rays by means of satellites. Abundance determinations from the solar spectrum, verified by the chemical analysis of the solar corpuscular radiation, gives the tool for a similar analysis of stellar spectra. In particular, isotopic ratios, seldom determinable for stars, are measured in the solar wind.

Why is the chemical composition of a star important? Because it allows us, provided we know the stellar mass and radius, to determine the energy production rate in the star's nuclear furnace and to predict the future development and life of the star. The sun's energy production rate is known from the energy received at the earth. In addition, its mass, radius, and chemical composition are also well determined. Thus the sun serves as a useful check on theories of stellar structure and evolution.

Recently, an exciting new observational check of the theory of solar structure has been developed. Deep in a mine in South Dakota, physicists have been able to capture the elusive solar neutrino radiation with what must be the most remarkable telescope in existence, a 30,000-gallon tank of cleaning fluid. Such an exotic detector is needed because of the low interaction rate of neutrinos with matter; they pass almost unimpeded through the enormous mass of sun and earth. Occasionally, however, a neutrino reacts with a chlorine atom in the cleaning fluid and causes a measurable nuclear transmutation. The cleaning fluid is housed deep in a mine to avoid accidental transmutations from stray cosmic rays. Because the few neutrinos that are captured are generated as a consequence of the nuclear reactions that produce energy in the solar core, they give a direct measurement of the structure of the solar interior. This check shows that the sun produces six times fewer neutrinos than theory had predicted. The consequences of this experiment for solar physics are great. Either the central temperature is lower than expected, or the weak-interaction theory of particle physics is called into question.

Ever since Carrington in 1859 observed the first solar eruption, solar flares have been studied intensely. We now know that the fantastic energies released in flares must have been bottled up in the magnetic fields of the solar active regions in which flares always occur. The source of this energy is thought to lie below the solar surface, perhaps inside sunspots. It takes about a day to store enough energy to cause a large solar flare. The energy release, which takes place in only minutes, is a dramatic example of a nonthermal phenomenon in astrophysics. During the explosive release, magnetic fields are apparently annihilated; and the resulting large electrical fields accelerate electrons, protons, and other charged particles to very high velocities corresponding to energies up to and sometimes exceeding a billion electron volts.

The generation of cosmic rays by the sun is such an improbable event that it might never have been imagined had it not actually been observed. The conversion of the disorganized slow motions of gas in the convective layer of the sun into the ordered motion of a few particles with velocities close to that of light stands as one of the most unexpected natural events in the universe. Solar physicists are fascinated by the complex physical processes that cause the acceleration of protons and electrons to cosmic-ray energies. The study of particle events in the sun opens up the possibility of understanding similar but more energetic phenomena throughout the universe. Interaction of fast particles with the plasma and with the imbedded magnetic fields produces a wide variety of phenomena that are interesting from the standpoint of plasma physics and, moreover, offer us the opportunity to interpret comparable events in more distant and less-well-resolved objects.

This interaction between plasmas and magnetic fields is well observable in the very-short- and very-long-wavelength regions of the solar spectrum. With the availability of satellite observatories outside the earth's atmosphere, radiation from solar flares has been observed with wavelengths as short as 10^{-3} and as long as 10^{12} Å. This wide spectral distribution of flare radiation is possible because of the very nonthermal nature of solar flares. The x- and gamma-ray radiation between 10 and 0.001 Å is caused by the impact of the very fast solar electrons with the dense plasma of the lower solar atmosphere. This impact radiation is very impulsive, the radiation showing fluctuations as rapid as 1 sec and perhaps shorter. Apart from x and gamma rays, these impacts should also cause a very energetic but as yet unobserved neutron radiation. The accelerated electrons interact with their environment also in another way. The local magnetic fields make them travel in spirals centered on the magnetic field lines. The resulting accelerations are the origin for part of the long-wavelength radio radiation.

The impact of the fast particles with the plasma and their interaction with the magnetic field cause the surroundings of the solar flare to heat up. This so-called thermalization takes only a few minutes. Because of the fantastic energies involved, the resulting temperatures are as high as 10 million to 100 million degrees, exceeding the temperature in the very center of the sun.

In a flare we have, therefore, an exceptionally hot plasma, which, because of its nearness and brightness, invites extensive study by refined spectroscopic techniques. Spectra of flares made with satelliteborne telescopes have revealed lines from highly ionized atoms, such as 25 times ionized iron and 19 times ionized calcium. These observations have encouraged theoretical and laboratory studies of spectra of plasmas at very high temperatures. As was the case with the spectroscopy of the neutral or moderately ionized atoms a few decades ago, we expect this new effort to provide us with powerful diagnostic tools for the examination of very hot plasmas wherever they may exist—on the sun, elsewhere in the universe, or in the laboratory in controlled fusion experiments, for example.

A fraction of the cosmic-ray particles escapes from the flare. Before arriving in the vicinity of the earth, they, and a slower traveling blast wave also generated in the flare, upset the solar corona and the interplanetary medium. Various types of radio emission originate when the particles and the blast wave pass through these very outer regions of the sun. An ingenious new type of radio telescope recently constructed in Australia clarifies how these disturbances propagate. The resulting physical picture is again applicable to similar, but more intense, radio bursts that have been observed on stars.

The earth moves in its orbit through the outer corona and cannot fail to sense the tremendous changes induced by flare radiations. The cosmic-ray storms, the enormous enhancement of x rays and ultraviolet radiation, and the interplanetary blast wave play havoc with the earth's upper atmosphere, changing its ionization balance, affecting the geomagnetic field, and possibly influencing climate, large-scale weather, and human well-being in subtle ways. Among the better-known influences of the solar flares on our human environment are the interruptions of radio communications and the disturbance of space weather affecting many of the satellite experiments and especially human spaceflight.

Until now, most observations of flares have been limited to secondary phenomena, such as the x and gamma rays emitted by the particles accelerated by the large electric fields occurring in the flaring region. Most evidence indicates that the flaring region is very small—of the order of 100 km. None of the satellite or even ground-based observatories has even approached the spatial resolution necessary to resolve these regions. In order to make further progress in our understanding of the origin of solar flares, it will be necessary to increase the spatial resolution of the observations by an order of magnitude, especially in the x, gamma, and radio region of the spectrum. Improvements in the Orbiting Solar Observatories seem to make it possible to approach the required resolution in the foreseeable future.

In the absence of solar flares, the solar corona near the earth is steadily expanding outward with a velocity of about 500 km/sec—the so-called solar wind. By carrying away angular momentum, this solar wind tends to slow down the rotation of the surface layers of the sun. The observed rotation of the solar surface is indeed much less than that observed in other stars, which are believed to have no stellar winds. A recent observation of the shape of the sun has led to the suggestion that the core of the sun still rotates at its primeval rapid rate, invisible beneath the slowly rotating surface. The ramifications of this concept are great. It would restore the majority of the angular momentum in the solar system to the sun and make its angular momentum consistent with that of stars without convection zones or stellar winds. The quadrupole moment of the sun's gravitational field produced by the rapidly rotating core would cause a small change in the motion of the perihelion of the planet Mercury. This would destroy the agreement between the observed advance and the prediction of Einstein's general theory of relativity. However, the observed advance could instead agree with the prediction of the Brans-Dicke scalar–tensor theory. The careful investigation of such an apparently simple question as "Is the sun really round?" may therefore provide a major clue to the understanding of our physical universe at its most fundamental level.

STELLAR EVOLUTION

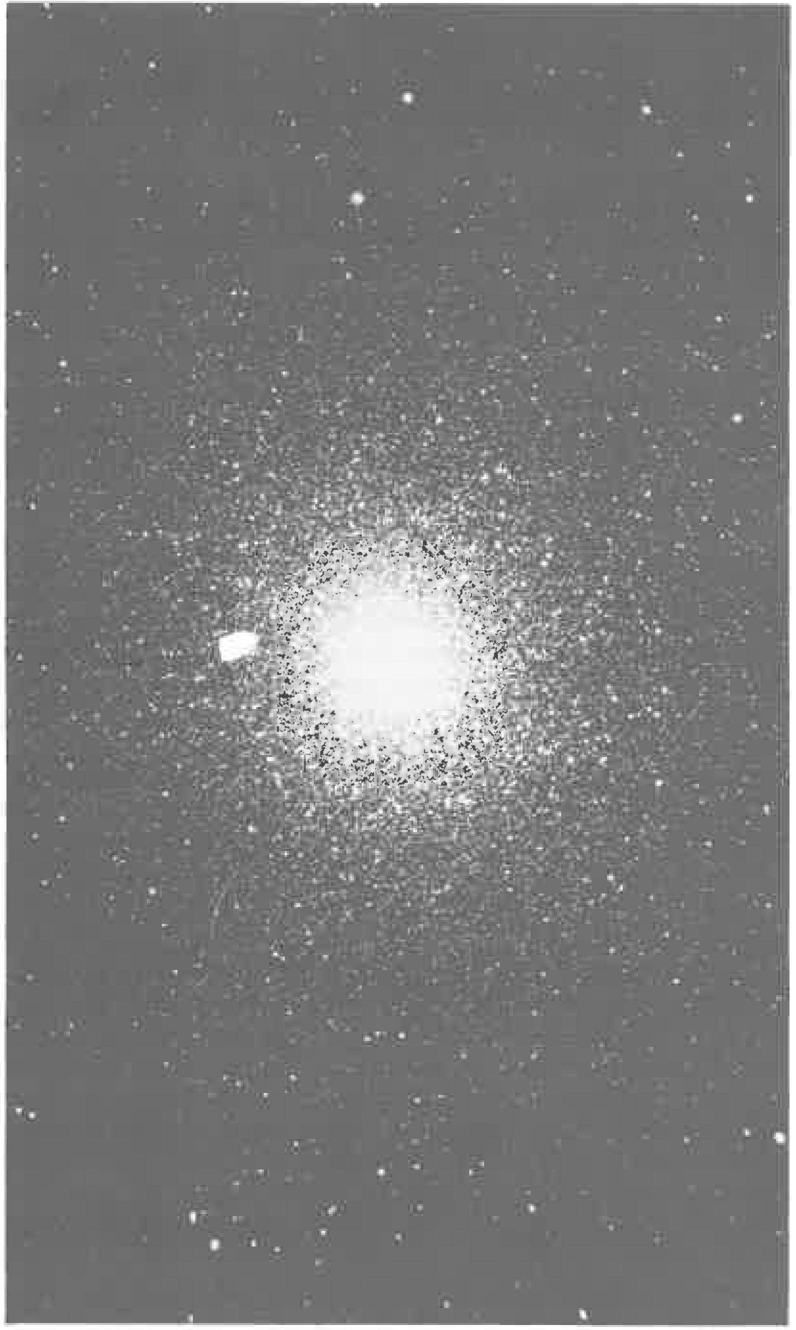
As recently as the last century, astronomers were almost totally ignorant of the basic nature of stars, but by 50 years ago the first tentative guesses were being made as to how stars might change with time. More daring were those astrophysicists who began to ask whether the relative abundance of the chemical elements themselves—the building blocks of the universe—instead of being given once and for all at a moment of creation, might have evolved with time, and indeed might still be changing. The last few years have made it abundantly clear that even the stately, stupendous, seemingly eternal galaxies have their own evolutionary processes, and that at least in the beginning, such events proceeded at what must be considered, astronomically speaking, a breakneck pace.

The basic physical laws and processes governing the structure and evolution of stars are now quite well known. Stars condense from the gas and dust of the interstellar medium, spend typically a few million years deriving their initial energy and form through gravitational collapse, attain in their deep interiors the multimillion-degree temperature needed to ignite processes of nuclear fusion (primarily the conversion of hydrogen to helium), and thereby become almost stationary configurations that show little outward change for intervals typically of from 1 million to potentially 100 billion years. However, in the core where the temperature is highest and the reactions proceed most rapidly, the central supplies of hydrogen are eventually exhausted. A new phase of rapid evolution then begins, during which hydrogen fusion is restricted to a shell around the core; the core—now helium—contracts, the outer surface of the star expands and cools, and the star becomes a red giant or even a supergiant. Subsequent evolution depends principally on the mass of the star; the greater the mass, the greater the tendency for further processes of fusion to occur in the deep interior. Thus helium "burns" to carbon, then to oxygen, neon, magnesium, and so on up the periodic table toward iron—the process of nucleosynthesis. These steps are in general well correlated with results obtained in laboratories. Eventually, when central supplies of accessible fuel are exhausted, its final evolutionary act is to contract to an extremely dense configuration. Depending on mass, it may become a white dwarf, a neutron star, or, in extreme cases, possibly a relativistic "black hole."

The sun and its planetary system condensed from the interstellar medium nearly 5 billion years ago. We are learning more about the "solar nebula," from which we came. During the last brief accelerating stages of gravitational collapse, the planets and remainder of the solar system were formed from a small percentage of debris not used in building the sun. Five billion years from now the sun will become a red giant, remaining in that state for a few hundred million years. Any life then remaining on earth would experience a huge red sun looming across more than 30 percent of the sky and an environment of evaporating air and oceans, at a temperature that would melt lead. Still later, a frozen and presumably lifeless earth will swing bleakly around a faint white-dwarf sun appearing no larger in the sky than the tiny planet Mars.

Astronomers are increasingly confident of the basic validity of this picture of stellar evolution. We cannot follow the life history of any given star, but we see stars in various stages of evolution. We see vast irregularly concentrated clouds of swirling interstellar gas and dust. Here and there we see individual stars or, even more conspicuously, clusters of stars condensing out of this medium—the galactic clusters that lie in the layer of gas and dust in the plane of the galaxy. In many of these still-forming, or recently formed, galactic clusters we can directly study details of recent stellar contraction and evolution. By noting differences between stars in different clusters, we can deduce some of the effects of age and of initial chemical composition. Fortunately, the galaxy also contains a few very large and ancient clusters, forming a vaguely spheroidal distribution about the rest of the galaxy. These globular clusters appear to be more than 10 billion years old and are possibly the oldest recognizable survivors from the time when our galaxy was condensing out of whatever primordial medium may then have permeated the universe.

Insight into the state of truly primitive matter can thus come directly from the study of these globular-cluster fossils. In particular, we can hope



Globular cluster—a 10-billion-year-old fossil remnant of the formation of the Milky Way. *(Photo courtesy of the Hale Observatories.)*

to learn the original proportion of helium produced by the big bang. Hydrogen, still overwhelmingly the most abundant element in the universe, was formed preferentially in the bang; probably, no element heavier than helium could be brewed under those conditions. But the precise percentage of helium that emerged from this aboriginal cosmic cooker offers us the most specific information we are likely ever to obtain about the detailed physical properties of the universe a few moments after its "creation."

It appears that about one fourth of the original material was helium, but this fraction could still be seriously in error. Even as early as the time when the oldest of globular clusters were formed, it seems that already some process in the galaxy had created and distributed a small but spectroscopically detectable amount of the heavier elements (about 1 percent of present abundance) plus an unknown amount of helium. Was this process a quasarlike outburst at the galactic center or a sudden efflorescence of supermassive supernovae among the very first stars to form, or was it a process as yet quite unknown to us?

In any event, it appears that the initial collapse of the gas and dust of the galaxy, from the original spheroidal form into a spinning flattened disk, took place in the rather short time of less than a billion years—less than a tenth of the age of the oldest known stars. During each successive stage in the collapse, stars and clusters of stars formed with material progressively more highly enriched in the heavier elements. By the time of formation of the youngest globular clusters, still some 10 billion years ago, the proportion of heavier elements seems to have approached the present value. This is a painful point for astronomers to try to explain. We are certain that nucleosynthesis occurs in all normal stars, and we are nearly certain that such stars, during late stages of their red giant evolution, rather placidly shed much of their matter back into space. At least a fraction of this matter should be enriched in helium and in slightly heavier elements. And we know that supernovae must have been exploding in quite appreciable numbers for all the billions of years since the globular clusters were formed. Why then do we fail to find incontrovertible evidence of a steady enrichment of elements heavier than hydrogen in the younger objects of the galaxy, for example, in the galactic clusters?

The more closely one looks at details of nucleosynthesis and stellar and galactic evolution, the clearer it becomes that our present understanding of these fundamental topics is incomplete at best. Thus, for example, observations of luminosity and surface temperature agree fairly well with predictions from the theory of stellar evolution for the commoner kinds of stars in globular clusters. RR Lyrae stars, which are dynamically unstable and pulsate rhythmically with periods of approximately half a day, are frequently found in globular clusters. By quite independent arguments of classical physics, it is possible to calculate the pulsation period of an RR Lyrae star of given mass and luminosity. This prediction gives results radically different from observations in globular clusters. Is the classical theory wrong, has it been misapplied, or are the stars different from what stellar evolutionary theory would have us believe?

Or, consider red giant stars located relatively near the center of our galaxy. There is observational evidence that most of them are rather similar in age and composition to red giants in our solar neighborhood. Yet there are many RR Lyrae stars in the solar vicinity of a type that seems to be completely lacking near the galactic center. Clearly, some major factors of stellar evolution are quite unknown.

New spectroscopic and photometric observations are needed to understand the most basic evolutionary processes in globular clusters and to test our notions of how nucleogenesis enriches the heavy element content of the interstellar medium. For example, theory predicts the loss of 20 percent of the mass of the giant stars of globular clusters over a time of 100 million years. Very-high-resolution spectroscopic observations are needed to detect this source of heavy metals pouring out of the oldest stars.

Supernovae in external galaxies are so far away that the late stages of the outburst become exceedingly faint and very difficult to observe. Yet these stages are just the ones in which spectroscopic evidence for the buildup of very heavy elements would be expected to be found.

Despite these circumstances and needs, the over-all picture of stellar evolution is well understood, and we should be able to apply it with confidence to nearby galaxies. Some parts indeed do fit, but outstanding anomalies remain in the few detailed observations that it has so far been possible to make of these nearest galaxy neighbors, the closest of which still range from hundreds of thousands to millions of light-years. In particular, some appear to have globular clusters that differ considerably from those of our own galaxy, suggesting different original conditions and possibly subsequent evolution.

Reaching a secure understanding of the formation of the elements and stellar evolution, with all of their implications for the rest of astronomy and cosmology, will require decades of the most careful and detailed work on objects mostly so distant as to be beyond the reach of any but the largest telescopes equipped with the most modern electrooptical devices. It also will require energetic pursuit of the most classical of astronomical studies, astrometry, which provides the positions and apparent motions of the stars. Only in this way can we derive unambiguous data on the distances and motions of stars, on which all the rest of the astronomical pyramid is built. It will require the closest attention to nearly every other branch of astronomy, including especially radio astronomy and ultraviolet space observations of interstellar gas, which can help to determine the properties of the swirling masses of gas destined to become stars.

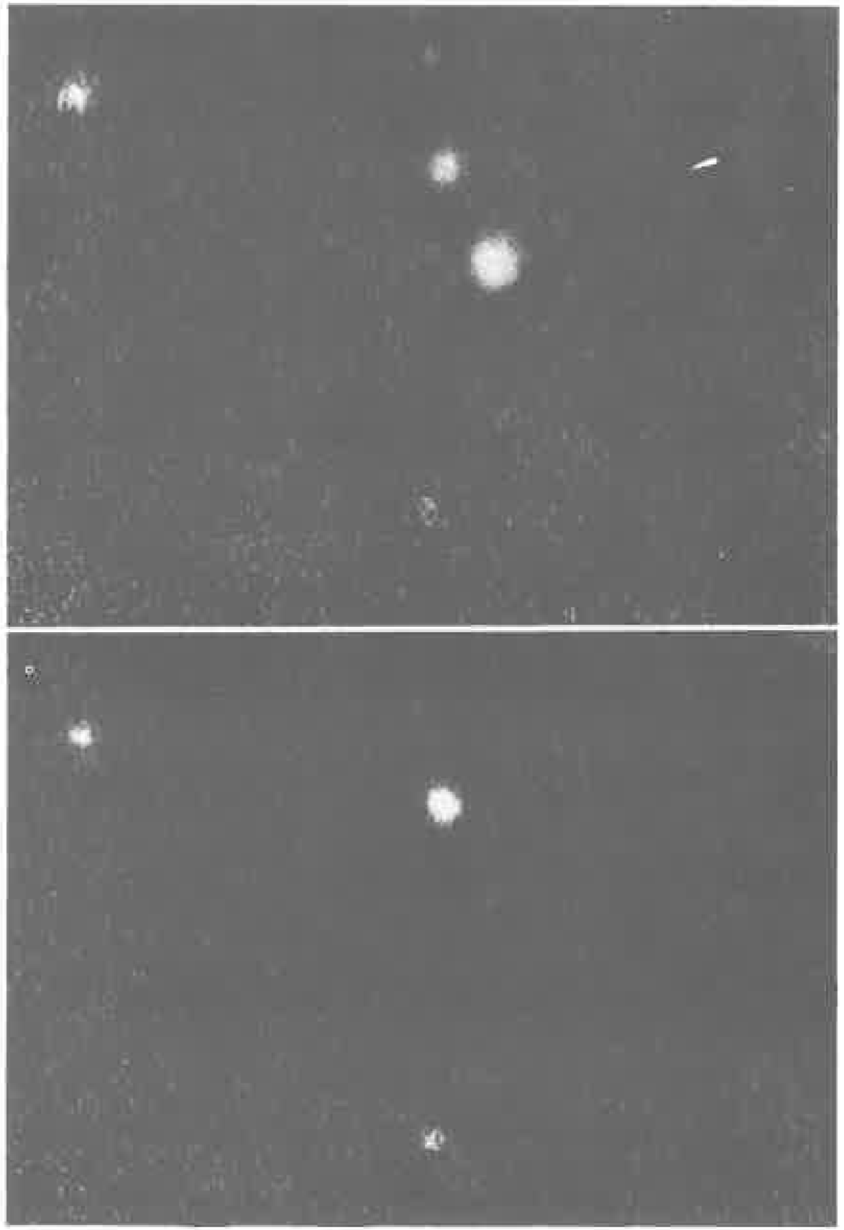
THE DEATH OF STARS AND THE BIRTH OF NUCLEI

The past few years have seen the growth of an enormous astrophysical interest in stellar deaths: how stars die and the nature of the corpses. This interest stems in part from growing confidence in theories and calculations of stellar evolution but even more from the recognition of the enormous variety and significance of the phenomena associated with stellar demise—the probability that almost all the chemical elements heavier than carbon were formed in the few seconds of a violent supernova explosion; the discovery of pulsars and their identification with rotating, collapsed stellar corpses; clues that dying and collapsed stars may be the source of cosmic rays, perhaps even of gravitational radiation and of other high-energy activity in our galaxy and in other, more explosive, large, and disturbed stellar systems.

There are many suggested ways in which a star, after its nuclear fuel has been burned, either explosively or gradually, contracts to its final state. But there are only four possible types of stellar corpse; only two have been observed so far.

1. In some cases, nearly all the matter in a star may be blown off into the interstellar medium. This is an extremely important process, because it is a major way in which heavy elements formed within the star are distributed for incorporation into other younger stars and solar systems. Partial disruption by steady flow or explosive ejection has been observed.

2. The star may implode toward a "black hole," a collapsed state in which the pull of gravity is so strong that even light signals cannot escape. A nonrotating, uncharged black hole has an external gravitational field, which may lead to observable x rays produced by falling charged particles, but no other contact with the universe around it. The state of matter in its interior cannot be known and has no reference to the rest of the universe. It is conjectured that a rotating black hole is axially symmetric with no external properties that can change with time, and so far none of the observable effects that might be associated with a rotating black hole have been identified. A black hole is the only possible final state for a stellar mass greater than approximately two to four solar masses. Stars with still greater masses may eject their outer layers and collapse in the center to



A supernova remnant—the Crab pulsar photographed by the 120-in. telescope. The upper picture near maximum light; in the lower, nearly invisible; the pulsar flashes 30 times a second. (*Photo courtesy of Lick Observatory.*)

form a black hole; again, we have no firm evidence for this process, except for a suggestion of a black hole as a component of an x-ray source.

3. The contraction of a star under gravity, when other internal pressures fail, can be halted by the motion of high-velocity electrons—a necessary consequence when electrons are forced close to each other. Such objects were discovered long ago; they are the white dwarfs whose mass can extend up to slightly greater than that of the sun. The central density of a white dwarf can reach 10^9 g cm^{-3}, where the properties of matter still can be calculated with reasonable confidence. Beyond this limit, the pull of gravity is sufficient to crush such stars toward another even denser final state. There remain various unresolved problems associated with white dwarfs, such as the origin and stabilization of the huge magnetic fields that seem necessary to produce the circularly polarized light and strange spectra of some white dwarfs or the effects of differential rotation, convection, and crystallization in such superdense matter.

4. Finally, when a star has contracted so far that the atomic nuclei within it actually touch, further contraction is resisted by the same combination of nucleon motion and repulsive nuclear forces that make the nuclei themselves almost incompressible. This can happen only for stellar densities exceeding 10^{14} g cm^{-3}. At such densities, all except a few percent of the electrons have been absorbed by the constituent protons, converting them to neutrons. Since these are the main components in such ultradense matter, the star is called a neutron star or a pulsar. The upper limit to the mass of a neutron star depends on nuclear forces at short range and the presence of the normally unstable, strange particles. It probably lies between one and two solar masses.

These theoretical pictures are confirmed by observations of stars that lose mass, or explode violently, and of white dwarfs, the classical stellar remnants. The search for direct observational evidence for black holes may never succeed even though they may contain much of the mass of the universe. But the neutron star has been found, and its properties can best be understood by study of the best-known remnant of a stellar explosion, the Crab nebula.

The Crab illustrates in a most vivid manner the value of historical, accumulative data, of observations made at all possible wavelengths, from radio to x-ray, and the interaction of theory and observation in modern astronomy. In A.D. 1054, on the fourth of July, the court astronomers of ancient China noted the appearance of a new star, a supernova, bright enough to be seen in daytime and visible for most of a year. Its place was taken by what Messier described in 1758 to be a "nebulosity. It contains no star; it is a whitish light, elongated like the flame of a taper. It was observed by Dr. Bevis in about 1731." The star had been replaced by a filamentary, gaseous nebula, expanding at a thousand miles a second. The nebula has an inner amorphous mass, whose optical radiation was not understood until it was shown to be almost completely polarized and until the radio astronomers found it to be the third strongest radio source in the sky. The amorphous mass consists of tangled magnetic fields in which high-energy electrons spiral, at speeds approaching that of light, with energies of billions of electron volts. Their total rest mass is about one millionth the mass of the sun. The filamentary expanding gas cloud has about a solar mass and will eventually be slowed down by plowing into ordinary interstellar gas. It is now a few light-years in diameter, about ten thousand light-years distant. Both the amorphous mass and the high-energy electrons bring messages from the initial explosion of the supernova. The deceleration of an electron as it swings about the magnetic fields produces synchrotron radiation, over a wide band of optical, infrared, and radio wavelengths, when the electron energy is high. The Crab nebula emits x rays, which were observed during one brief rocket flight; during another, while the source was being eclipsed by the moon, it was found to be an extended rather than a point source.

Most importantly, the high-energy electrons lose energy rapidly by emitting radio to x-ray photons and must, in fact, be renewed every few decades from the central source. Photographs taken with large telescopes had revealed the expansion of the filamentary nebula, and spectra had shown it to be of slightly peculiar composition, lacking hydrogen (presumably consumed as nuclear fuel and converted into helium). The center of expansion was located by accurate positional measurements of the shorter filaments, on photographs taken over many years. Nearly at this center were two faint stars, one of which had no detectable spectral lines, absorption, or emission; its visible light output made it slightly brighter than the sun, while the nebula was hundreds of times brighter—not unusual for a very hot star immersed in a normal gaseous nebula. But direct photography of the amorphous mass, near the center star, added a new mystery. Small, diffuse clouds moved as if struck by waves of excitation (or high-energy electrons) and changed in brightness and structure. The disturbances propagated at nearly the speed of light.

The discovery of pulsars in England in 1967 revealed the existence of variable radio sources with periods from 0.033 to 2 sec. The 0.033-sec pulsar was near the Crab. The timing of the pulses was extraordinarily accurate and soon revealed that the periods were slowly lengthening. Three young and ingenious astrophysicists, using only a 36-in. telescope, searched in 1969 for the 0.033-sec period in the light of one of the central stars of the Crab nebula and immediately found it. Other telescopes determined very accurate light curves, which resembled the radio light curves, and found that the star disappeared at pulse minimum; the spectrum was re-examined and found again to be featureless, with a continuous energy distribution that could be an extension of the radio-frequency pulse spectrum. Re-examination of rocket-flight data with the now known period showed that the x rays also pulsed, i.e., that the star as well as the amorphous continuum produced x rays. The supernova had left behind an observable stellar remnant, radiating enormous energies, from radio to x rays.

There is extremely strong support for the notion that such a pulsar is a rotating neutron star containing a huge (perhaps 10^{12} G or greater) magnetic field. The precise observations possible for pulsar signals, and the period between pulses, suggest many ways of exploring their internal structure. Neutron stars are objects of an intense interest, which is unlikely to diminish. They supply the only known mechanism for efficient conversion of enormous amounts of gravitational potential energy into very-high-energy cosmic rays. The remnant pulsar at the center of the Crab nebula is still emitting over ten thousand times the solar luminosity into that nebula 900 years after the supernova explosion in which it was formed.

During the collapse of a dying star, angular momentum conservation demands that it spin faster. Much of its gravitational potential energy is converted into the kinetic energy of its increasingly rapid rotation, some is used to compress and thereby greatly amplify whatever magnetic fields it contains. The "dead" star together with its huge field spins many times per second. This comprises an enormous electric power generator that efficiently accelerates surrounding electrically charged particles to cosmic-ray energies. It slowly converts the rotational energy that it gained from gravitational contraction to that of cosmic-ray protons, nuclei, and electrons.

Understanding a neutron star will require extending present frontiers in nuclear physics, elementary-particle theory, solid-state theory, and low-temperature physics. A typical neutron star of one solar mass has a radius of only 10 km. It would fit inside Los Angeles! The outer layer, of the order of a few kilometers, until the density reaches about 3×10^{14} g cm^{-3}, contains nuclei in a crystalline lattice well below the melting temperature. Thus the neutron star has a thick crust. The inner part of the crust is 10^{16} times stiffer than steel and 10^{5} times better an electrical conductor than copper; it is almost entirely free of impurities and contains a neutron fluid that together with free electrons fills the region between nuclei. Below the crust, neutron star matter consists mostly of neutrons and a small percentage of electrons and protons. The neutrons form an anisotropic superfluid, similar to that of terrestrial liquid helium at a temperature close to absolute zero: it has essentially no viscosity, negligible heat capacity, and remarkable flow properties. The protons probably constitute a superconductor in which electric currents producing a magnetic field flow without loss. When the density exceeds 10^{15} g cm^{-3}, even the constituents of matter are not well understood; in the ultradense environment of a neutron star core, normally unstable heavy particles become quite stable and mesons and strange heavier nucleons will be present. The nature of matter, and its pressure, is not yet known at such high densities.

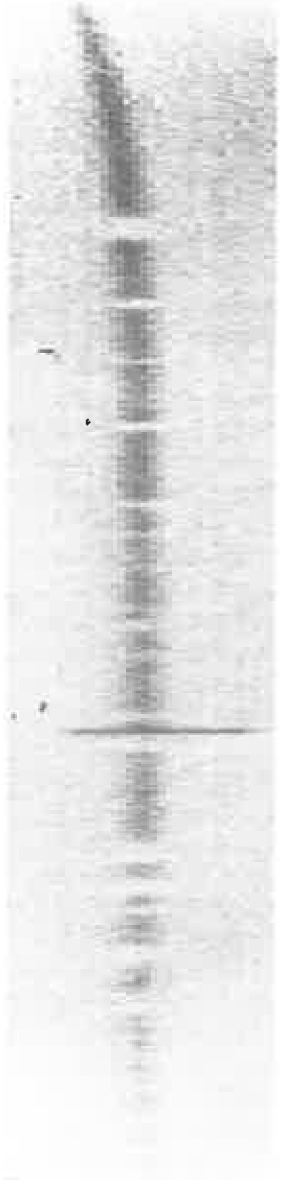
What happens to the matter ejected from a star that has suffered a violent central collapse? The observed supernova spectra, for many classes of supernovae, show essentially normal composition, except that the hydrogen has been depleted and the heavy elements somewhat increased. (For one type, the brightest of all, no spectral features have yet been identified.) Velocities of expansion up to 15,000 km sec^{-1} are found. But the light fades before material from the deeper interior can be spectroscopically observed. Advances in theoretical study of the hydrodynamics of the collapse and the nuclear reactions that occur currently suggest that stars in the range of four to eight solar masses are the source for most of the nucleosynthesis of heavy elements. The details of the collapse are somewhat speculative. When the core has burned all its hydrogen and helium, it is left with too much mass to be supported by the degenerate electron gas pressure. Rapid contraction of the hot core causes a thermal runaway, igniting thermonuclear reactions of carbon and oxygen. The detonation wave races through the core, burning additional nuclear fuel and creating a shock wave that ejects the outer layers of the star. A neutron star remnant may be left at the center of the exploding supernova. Study of the details of nuclear reactions that occur in the shocked, rapidly exploding envelope has been carried through, using a complicated network of hundreds of possible nuclear reactions. For many of these, laboratory data exist; for others, nuclear cross sections must be computed or estimated. The outer part of the star (two to six solar masses, if a neutron star is left behind) is exploding in a little bang like primal matter of the big bang. The density and temperature vary as a function of time and initial location within the star. All the envelope material will be ejected and the reactions terminated in a few seconds. Material that started near the core, at high density, will build heavy elements; slightly lower initial temperatures yield the iron group, and still lower, the elements from silicon to scandium. There is striking similarity to details of the abundance curve of the elements and isotopes found on earth, the moon, and in the meteorites. Certain old stars, with low total metal content, show elemental abundances remarkably similar to those predicted by explosive nucleosynthesis. The theoretical predictions can be improved by better knowledge of reaction cross sections and the dynamics of the collapse. But the astronomers must also provide better data on supernova light curves at all wavelengths, including observations in the infrared and in the far ultraviolet from space. They must also obtain the spectrum at least two or three years after the explosion, when it has faded into near invisibility at optical wavelengths, since only then will the cloud be transparent and will material from the hot, inner regions, containing heavier elements, become observable. As the cloud density drops, the spectrum is expected to change until it will show only the "forbidden" lines of partially stripped ions, as in the solar corona; spectroscopic observations of supernovae years after the explosion will be possible only with the largest telescopes, equipped with the modern electronic auxiliaries now becoming available—television finders, image intensifiers, and digital-output TV tubes. Clearly, more than one large telescope is needed. Are there optical pulsars in other supernova remnants? Are there superpulsars in the very luminous core of a galaxy? If so, how are the abundances of the elements changed? Again, only the largest telescopes can supply the answers. Or, in space, accurate mapping of x-ray sources and detailed study of very-high-energy brightness may reveal a neutron star core.

Thus, from the supernova outburst, to pulsar, to the origin of the elements out of which our bodies are made, our minds must traverse a continuous line of observation, search, thought, speculation, calculation, and measurement. Could we bypass all this speculation by traveling through space ten thousand years and studying the pulsar as we fly past it? A young astrophysicist has written the following:

> We will never see well a neutron star. Imagine that one has seen a distant light. Is it emitted by a flashlight or a bonfire? One does not know, for one has only seen the light. Similarly, we see only flashes, not the neutron star emitting them. It is not because it is invisible in some magical way, but because it is small. If an astronaut, more foolhardy than brave, were to venture sufficiently close that it loomed as large as the moon in our sky he would be irradiated (high-energy particles), burnt to a crisp (thermal x rays), torn in shreds (tidal force), and blown away. If he were to live, he would be traveling so fast that he would have only a thousandth of a second for a glimpse of it with the naked eye as he sped by.

EXPLODING CORES OF GALAXIES

Large optical and radio telescopes have discovered extraordinary phenomena in the centers of galaxies, the nearly pointlike nuclei. Observed first visually as a point, photographed next as a tiny region, and measured by very-long-baseline interferometry as still essentially un-



The spectrum of a quasar with one of the largest known red shifts, obtained in a 6-hour exposure on the 200-in. Hale telescope with an integrating TV camera. More than a week of exposure on an ordinary photographic plate would be required to obtain this spectrum. The dark line is light pollution—mercury arc lights from San Diego. *(Photo courtesy of Princeton and Hale Observatories.)*

resolved, the nuclei of certain galaxies seem to be the site of unbelievably energetic explosions.

To the astronomy of the first half of the twentieth century belongs the revelation that we live in an expanding universe. Galaxies rush away with ever-increasing speed the farther out we look, as the universe expands from the cataclysmic event of whose wrenching paroxysm we are but dimly aware in the most remote and oldest reaches of space. To the astronomers of the 1930's and 1940's, these galaxies, grand and stately, seemed immutable, great beacons that rushed through space changing slowly in response to the evolution of their stars but otherwise as solid as the rocks of the ancient earth from which they were observed. To most astronomers of that era, explosive events were confined to a few curious and peculiar stars and to the colossal singular moment at the beginning of time.

But with the advent of radio astronomy, the discovery of quasars, and the closer optical examination of many strange galaxies, astronomers of the 1960's and 1970's have found that the universe teems with explosive events, as bewildering, outrageous, appalling, amusing, and sobering as they are ubiquitous. The tiny, remote, but fantastically energetic quasars show evidence for the ejection of matter at high velocity (100,000 km per sec). Many powerful radio galaxies are doubled sources, with evidence that two great magnetized clouds of gas, with masses of millions of suns, are exploding away from each other with velocities close to the velocity of light. Embedded in the centers of certain galaxies, called Seyfert galaxies, are tiny sources, whose dimensions are only ten times that of a supergiant star but that act like a permanent supernova, ejecting matter into space at a velocity of 1000 km per sec. In one such object, ejection has been going on for at least 40 years, but the significance of the early observations was not appreciated until recently.

Something like 2 to 5 percent of galaxies show evidence for explosions and violent events in their nuclei, and it is estimated that, if all galaxies evolve through a stage in which such violent events take place, the explosive stage lasts some 100 million years. During that time, some galaxies may eject quantities of matter equal to that of a small or possibly even a normal galaxy. What mechanism is responsible for the ejection of such a seemingly inexhaustible cornucopia of matter? What is the matter and energy balance in the nucleus? Could the energy of the nucleus be supplied by collisions between millions of stars confined to a volume of space less than a light-year in diameter? Is the nucleus itself, having a mass of about 100 million suns, some strange massive object whose physics is only dimly understood?

Though truly violent events seem to be confined to the nuclei of Seyferts and quasars and to certain kinds of disturbed radio galaxy, even "normal" galaxies show evidence for the ejection of matter from their nuclei on a smaller scale and at a slower rate. Our own galaxy ejects matter from its nucleus at the rate of about 1 solar mass per year, as revealed by radio-astronomy observations. Infrared, radio, and x-ray observations show that this tiny nucleus has some of the properties of a modest, but disturbed, Seyfert nucleus. Because of the enormous obscuration of the center of our galaxy by interstellar dust, we cannot "see" it in optical wavelengths. It may be that all spiral galaxies show evidence for at least some modest activity in their nuclei. What role, then, does the nucleus of a galaxy play in its evolution? Could it be that all the matter of a galaxy is ejected (even, possibly, created) from its nucleus, and that as galaxies age, the explosive natures of the nuclei gradually die away with time? Further study of the astrophysics of these nuclei not only will tax the ingenuity of the theoretician dealing with new and unexpected states of matter but will require significantly greater observational effort with the largest telescopes and the most sophisticated electrooptical technology.

What plausible theory can explain this implausible behavior? Speculations are many; since the concentration of so much energy into a small volume is common to the observed compact parts of galaxies, or quasi-stellar objects, some theoreticians believe that gravitational collapse is involved. A dense many-star system could evolve thermodynamically by ejecting stars, supplying work done in the ejection from gravitational contraction by the balance of the stars. The system becomes denser, the near collisions and ejections more rapid, until the remaining stars undergo relativistic collapse into a black hole, releasing enormous amounts of gravitational energy. This dramatic picture is less radical in its physics than the speculation that a very large concentration of matter, such as is found at the center of a galaxy, may be the source of rapid, continuing creation of new matter. Here a new physical principle is required, not testable on the small scale of the earth, working only on the large scale—requiring deep changes in general relativity. In still another model, the dense core of a galaxy may contain some matter of extremely high density, not yet expanded from the early moments of creation; matter and energy equivalent to a hundred million suns have been formed in some unknown way, the equivalent of a single giant atom, which eventually explodes. In many other ways explosions in galaxies have strange and not yet understood effects. Evidence for the recent appearance of whole galaxies has been derived from study of radio-quiet compact galaxies, dominated by the emission of hot gas and very young stars. Where did their matter come from?

A fascinating link with the theory of the origin of chemical elements comes from the observations of the spectra of exploding stars or supernovae. Chains of nuclear reaction occurring in the stars have been suggested as leading to the synthesis of nearly all the chemical elements. Have we found any classes of stars, or of galaxies, so young that only the hydrogen and helium of the big bang exist in their atmospheres? The answer seems to be no; even the most metal-poor stars have at least half a percent of the solar metal abundance. What happened in our own galaxy to load the primal hydrogen and helium with all these heavy elements? Was our galaxy filled with quasarlike explosions, or was its nucleus so enormously active that the heavy elements synthesized there provided this initial metal content? The emission-line spectra of quasars show most of the expected chemical elements from carbon to iron. The apparently fast-moving shells producing the absorption lines found in quasars were also found to show the common and expected elements. The quasars should have been the likely candidates, if any objects might be, to be young or to have had radically different types of nucleosynthesis.

Perhaps the strangest feature of astronomical explosions, besides their prevalence, is their association with high-energy electrons and possible cosmic rays. Ordinary novae emit radio noise and excessive infrared. Supernovae, as evidenced by the Crab nebula and its pulsar, emit x rays, light, and radio waves. Exploding galaxies are radio sources and emit excess infrared. Yet the velocities of explosion are relatively low, 10^3 to 10^4 km sec^{-1}, corresponding to 6 to 600 keV per nucleon. The cosmic-ray electron energy required, however, to produce the radio noise is in the billion-volt range, and perhaps higher for x rays. Thus observation from space, as well as from the ground, of the sun, stars, and galaxies leads to one fundamental question—how organized motion of many particles at relatively low energies can result in such concentration of energy into the relativistic motions of a few particles. Are large electric field gradients created? Do magnetic fields accelerate a few fast particles to nearly the speed of light? Or is some mechanism like that of the pulsars (a rotating magnetic field) present on a huge scale in the nuclei of galaxies as well as on a small scale in the heart of a supernova? One lesson, for possible terrestrial application is that rapidly moving clouds of ionized gas are almost always connected with the very-high-energy phenomena.

MOLECULES, DUST, AND LIFE

There was a time when the formation of the earth and the solar system was believed to be an extraordinary event, occasioned by a chance encounter with a passing star that ripped off the planets from the sun, set them spinning, and then wandered away into space never to be seen again. We

Interstellar gas and dust—the birthplace of stars and planetary systems. Photographed by the 200-in. telescope. *(Photo courtesy of Hale Observatories.)*

no longer think such special creation to be true, but an even more intriguing picture has taken its place. We now think solar systems to be commonplace, arising in a natural way as a consequence of the formation of the stars themselves. The evidence for this is both observational, in that rotating young stars are always seen associated with complexes of gas and dust, and theoretical, in that calculations show that if a cloud of matter becomes sufficiently dense or massive, it must inevitably collapse and fragment into a cluster of stars. How planets form is somewhat of a mystery, one problem being that so far we have only one solar system to study. How life forms once the planets are there is even more obscure, but one of the fascinating discoveries of recent times is that some of the molecules necessary for the development of life may already have been present in the original dust cloud out of which the solar system formed. Indeed the process of creation may be universal; there may be many solar systems, and perhaps many life forms, scattered throughout the galaxy, with whom we might someday communicate or of whose existence we might at least learn.

We must uncover the initial conditions in the interstellar medium out of which the solar system formed. We know both gas and dust were present. Photographic studies of the sky long ago revealed many luminous clouds, which spectroscopic analysis later showed to be mainly hydrogen gas, ionized by the hot stars embedded within them. It was suspected that space far from stars also contained hydrogen, but in a neutral form too cold to be luminous. This was confirmed in 1951 when the 21-cm radio emission of hydrogen was first detected. Since then, radio observations have mapped hydrogen clouds in spiral arms stretching around the galaxy and detected such gas in other galaxies.

The solid particles in space, the cosmic dust, have also been long known, for even a casual glance at the Milky Way, let alone examination of deep-sky photographs, reveals numerous complexes of dark patches, which are nearby dust clouds obscuring and reddening the more distant stars. In general, the dust is where the gas is, except that when a radio telescope is pointed at the densest dust clouds, no emission from atomic hydrogen is found at all. The dust is presumed to have catalyzed the formation of molecular hydrogen, H_2, and shielded it from the destructive power of the ultraviolet radiation filling most of space. Because it is undetectable at optical and radio wavelengths, the hydrogen molecule was not detected in space until recently, when rockets carried stellar spectrographs above the atmosphere to obtain observations in the ultraviolet. Such observations are limited to clouds in the direction of bright stars, however, so we must await the detection of infrared emission lines of H_2 before we shall know the true distribution of this molecule.

Are there other molecules in space? Some, such as CH and CN, have strong absorption lines at optical wavelengths and were discovered early with the large spectrographs on conventional telescopes. New electronic detection techniques will doubtless lead to searches for weaker lines, allowing several more to be discovered in this way. However, it is in the radio spectrum, particularly the centimeter and millimeter regions, that the most dramatic molecular discoveries have been made.

The hydroxyl radical, OH, was the first sought and found. It too seems to be associated with dust clouds. Its existence is not surprising, for oxygen and hydrogen are among the most abundant elements. However, its radio emission, the 18-cm line, was found to be exceedingly strong in some small regions, and it soon became clear that an interstellar maser was involved. Understanding the details of the maser process is a challenge to the theoreticians. It involves finding some mechanism (which may involve collisions, infrared or ultraviolet radiation) that will over-populate a molecular energy level, causing it to amplify any incident 18-cm radiation. The full explication of this stimulated emission process will reveal new physical phenomena going on within the dust clouds.

Radio telescopes precise enough to work at millimeter wavelengths have led to the recent discovery of several other diatomic molecules, e.g., CO, CS, and SiO. Lines of water and ammonia have been found, and the water emission also has maser characteristics. The presence of these molecules is not surprising, for though they contain more than two atoms, they are the stable compounds of the abundant elements oxygen and nitrogen with hydrogen.

Do more complex molecules form in space? Only a few years ago, we would have said no, for the rate at which atoms collide and stick together in the near vacuum of space is so low that large complexes should not build up. In addition, most complex molecules are easily broken apart by ultraviolet light and cosmic rays—it is only because our atmosphere shields us from this radiation that life can exist on earth. Yet with the decade of the 1970's barely begun, an undreamed-of array of complex forms has been found in interstellar space—molecules like formaldehyde, methyl alcohol, cyanoacetylene, formic acid, and formamide. Every radio astronomer has an extensive search list, and the known species will probably be doubled in the next few years. We might speculate that even very complex species, such as amino acids and other prebiological molecules, may be found. Now the astronomer must call for help from the chemist to see if, together, they can explain how such a bewildering assortment of chemicals could be born. An important factor in constructing any theories of molecular formation will be the determination of the numbers of atoms, the building blocks of molecules, within the dust clouds. Here we must look to ultraviolet spectroscopy from space vehicles to provide the data, for few of the important elements have strong absorption lines in the visible part of the spectrum.

One current theory is that molecules are made on the surfaces of dust grains within the dark clouds. Much theoretical and laboratory work is needed to explore this idea, and astronomical work too, for we as yet do not even know what the dust is made of. The degree of reddening of starlight in the visible spectrum tells us that the particles must be small, a few tenths of a micron in diameter, but to determine the composition will require sensitive measurements in the infrared, where vibration–rotation bands of molecular solids occur, or in the ultraviolet, where we find the electronic absorption bands. Some clues are already at hand, however. Ultraviolet spectra of reddened stars obtained with the Orbiting Astronomical Observatory satellite are indicative of the presence of graphite. Infrared observations show emissions like those of silicate minerals. Perhaps a mixture of many kinds of substances is the true situation.

The dust clouds around stars are particularly interesting. In some cases the dust seems to be formed by the star itself and is being blown away into interstellar space. Perhaps such stars are the source of all the interstellar dust. Are they the source of molecules as well? CO and CS have been detected in one infrared source, and OH and H_2O in another. High-sensitivity infrared and millimeter studies of such objects should prove fruitful and may reveal the prime molecule factory.

Other circumstellar dust clouds appear to be the remnants of protostars that have just formed. Perhaps we are seeing preplanetary systems under conditions like those prevailing in our own solar system 5 billion years ago. The infrared spectra of comets show the same silicate emission feature found elsewhere in the galaxy. Here, too, in objects left over from the early days of our solar system, we look for molecular clues to our origins.

What does all this have to do with chemical and prebiological evolution on earth? The rapid discovery of ever more complex molecules within dust clouds suggests that they will, at least, lead us to a clearer delineation of the starting point from which evolutionary theories must proceed. It might, of course, be a false lead, if all the molecules evolved in interstellar clouds and protostars may be destroyed in the condensation or subsequent heating of the planet. Examination of the surface rocks of the moon and other planets will yield useful data on this point. Perhaps the destructive heating of planetary formation is circumvented by the temporary storage of the cosmic organic chemicals in natural "deep freezes." These could be the swarms of meteorites (some of which have complex hydrocarbons) that circulate in planetary systems or, even more likely, the comets (whose spectra are dominated by compounds of carbon, nitrogen, oxygen, and hydrogen). Here the material of life could be protected at low temperature while a planet develops the conditions suitable for life, eventually to rain onto the planet with much of it surviving unaltered. This implies that life everywhere is similar chemically, making biological similarity more likely.

Even if life is not constructed from molecules formed in interstellar space, study of the organic processes and chemistry taking place there will give insight into organic chemical evolution, wherever it took place. Whatever the case, it is clear that in the study of the interstellar medium, stars, and preplanetary systems, we are at a frontier of knowledge at which the techniques of optical, radio, infrared, and space astronomy, combined with the laboratory and theoretical skills of chemists and physicists as well as astronomers, are leading toward a better understanding of not only our ultimate beginnings but those of life everywhere in the universe.

THE SOLAR SYSTEM

We live on one small part of a giant ruin containing vital clues to the processes of star and planet formation and the origin of life. Are there other planetary systems like our own? Do they contain organized, self-replicating entities that we might call alive? What are the processes required for life to evolve, and how commonly do they occur? One method of attacking these questions is to investigate our own planet and our immediate neighbors—the other objects traveling through space in the company of the sun.

In the new era of solar-system investigations, direct probes are providing information in a quantity and quality impossible to achieve by other means. Nevertheless, important problems can still be studied with ground-based observations. This is especially true of the outer solar system, where journeys by sophisticated probes are not expected before the end of this decade, but it is also true for many kinds of studies of our neighbors Mars and Venus. Space missions are necessarily few in number; they often provide information only about a small region of the particular object to which they are sent. Furthermore, while the instrumentation may make irreplaceable observations, it must be miniaturized and limited, resulting in a loss of sensitivity compared with devices at ground-based observatories.

For example, the earlier spacecraft have failed to detect water on Mars or HCl and HF on Venus, even though ground-based measurements show these compounds to be present. The extremely low limits on possible constituents of the Martian atmosphere set by ultraviolet observations of that planet by OAO-A2 exceed the sensitivity of mass spectrometers currently planned for landing on Mars. In the atmospheric windows, much can be accomplished from ground-based and near-earth observations, especially as sensitivity and sophistication improve over the next few years.

Two areas of active research in physics indicate the diversity of solar-system studies. Tests of general relativity may be carried out by radar ranging of planets passing close to and behind the solar limb. The signal returned has predictable delays caused by gravitational deflection, which differ in Einstein and Brans-Dicke cosmologies, and accurate observations will permit a definitive choice between the two theories. A second subject is planetary magnetic fields, their interaction with the interplanetary medium, and the formation of belts of trapped high-energy particles. Jupiter is the only planet, other than the earth, known to have a magnetic field; it is surrounded by radiation belts similar to, but much more energetic than, those discovered around the earth by van Allen. The Jovian system is studied by the radio-frequency energy emitted by these charged particles. We do not yet understand their interaction with the planet's ionosphere and with its satellite Io. Increased understanding of the large-scale electrical and magnetohydrodynamical behavior of matter on an astronomical scale may come from such studies.

Within the last decade, it became apparent that the compositions of the atmospheres of earth, Mars, and Venus, while closely related, were significantly different. By analogy with the composition of our own atmosphere, the dominant atmospheric constituent for Mars and Venus was assumed to be nitrogen. Ground-based spectroscopic investigations (later confirmed by spacecraft) discovered that the major constituent of both atmospheres is carbon dioxide. Carbon dioxide is the second most abundant volatile gas released from the earth's crust over long geologic times. It does not dominate our atmosphere because in the presence of water it displaces SiO_2 in rocks to form carbonates. In the absence of water, we would expect the earth to be lifeless, with an atmosphere containing about as much carbon dioxide as the atmosphere of Venus, with nitrogen only a minor ($\sim$5 percent) constituent.

But water is the most abundant volatile outgassed by the earth. By analogy, we should expect large amounts of water on Mars and Venus also. But the surface of Venus is too hot to permit water to exist, either as a liquid or as ice or absorbed in rocks. This explains the presence of the enormous amount of carbon dioxide that has been observed: the equilibrium between silicate and carbonate rocks was established at a level that kept the outgassed CO_2 in the atmosphere. But what about the water? The amount of water presently in the atmosphere of Venus is uncertain, but it clearly is several orders of magnitude below that expected

Comet Mrkos, possibly preserving a record of the process that created the solar system. *(Photo courtesy of Hale Observatories.)*

by analogy with the earth. Either it was deficient in the planet originally, or it has subsequently escaped.

It is vital that we discover the reason for this anomaly if we are to assess properly the likelihood of the development of life elsewhere in the universe. Liquid water is generally conceded to be essential for life. How is it that we have so high an abundance on the earth and so little on Venus? Is distance from the sun the crucial factor? The answers should have equal applicability to other solar systems, even those with suns of differing luminosities and surface temperatures.

On Mars the problem is slightly different. Again we observe smaller amounts of water in its atmosphere than we can reconcile with the amount of CO_2. The total atmospheric-water pressure is too low for liquid water to exist. Our expectation on the basis of analogy with the earth is satisfied. To account for the absence of water, we could invoke again the possibility that it escaped during the life of the solar system. But we might also postulate it still present on the planet, as permafrost and adsorbed water buried beneath the surface..

It is obviously important to resolve such ambiguities if we are to develop our ideas about evolution of planetary atmospheres. We will have to resort to indirect methods, for example, studies of the abundance of noble gases and their isotopes. The amount of nitrogen present is also an important clue; spacecraft will probably be required to make these measurements.

Mars still holds the special fascination associated with the possibility that some form of life might exist on its surface. Each increase in our knowledge of the Martian environment has appeared to make this possibility less likely, but it is by no means excluded, and experiments to detect life are planned to be landed before the end of the decade.

The Jovian planets are drastically different from our nearer neighbors. The major planets are much closer in composition to the cosmic average found in stars and nebulae—they may represent the basic matter from which the sun and the planets formed. The composition and isotope ratios in these planets could provide information about conditions in the primitive solar nebula. Scientists commonly assume that, in order for life to begin on earth, it was necessary for the atmosphere of our planet to contain large amounts of methane and ammonia, the major constituents of the present atmospheres of the outer planets. The giant planets, especially Jupiter and Saturn, may thus provide enormous natural laboratories in which processes required for the origin of life are still being repeated. The level of complexity achieved under such conditions will affect theories of the origin of life on earth and elsewhere in the universe.

There is some doubt whether the earth's atmosphere was ever hydrogen-rich. It is more likely that after the crust solidified, free hydrogen was less abundant than the cosmic average. There are objects in the solar system with such atmospheres: Titan, the largest satellite of Saturn, and Uranus and Neptune appear to have hydrogen-deficient atmospheres.

Uranus and Neptune challenge our ideas about the formation and evolution of atmospheres. How was hydrogen lost from cold, massive bodies at such great distances from the sun? Are we seeing the results of composition differences in the original cloud of gas? Some models for collapsing gas clouds suggest that such effects might occur. We must interpret the "fossil record" correctly in order to assess such models, and this requires additional studies of the atmospheric composition and radiation balance of these distant planets.

The comets, planetary satellites, asteroids, and interplanetary dust must be encompassed by any comprehensive theory of origin and evolution. Before manned landings on the moon, meteorites provided the only extraterrestrial material for laboratory analysis. They are still the oldest undisturbed samples we have and provide invaluable information about physical and chemical conditions in the early stages of solar-system formation.

The rocky, metallic meteorites have received most attention and are probably associated with the asteroids. However, it has become apparent that friable meteorites, such as the type I carbonaceous chondrites, are much much more abundant than had been thought and are unique in containing relatively large amounts of complex organic compounds. A number of amino acids have been identified recently in two such meteorites, establishing that fundamental building blocks for living systems were formed abiogenically somewhere in space and lending support to theories that suggest that material for life may be common in the universe.

At this stage of understanding we can only speculate about connections between meteorites, organic molecules recently discovered in interstellar space, and development of life. Comets seem to be likely intermediaries; they appear to contain ices that outgas some organic compounds. Comets may have been the first objects to condense from the solar nebula and may contain some of the rich mixture of complex molecules now found in dense interstellar clouds. A tie between friable meteorites and comets has been established through the study of meteor orbits; but it remains to be proven whether the type I carbonaceous chondrites are fragments of extinct comet nuclei. One can see large questions about prelife organic chemistry and conditions in the solar nebula looming behind these studies.

With the opportunity for investigations of the widely differing environments provided by the inner and outer planets, their satellites, and the small bodies that move among them, studies of the solar system will continue to provide a foundation from which to assess the possibility that we are not the only form of life existing in the universe.

ASTRONOMY AND EXOBIOLOGY

There is perhaps nothing more tantalizing than the possibility of detecting the peoples of other worlds, a possibility made real by the development of powerful radio and optical telescopes in our era. Indeed, our current radio telescopes can detect the radiations of a civilization no more advanced than ours over distances of many hundreds of light-years, a range within which there are millions of stars. If more advanced civilizations exist with the capability of controlling the powers of the stars themselves, which is not inconceivable, the effects of such cosmic engineering can be discovered by our present optical and radio telescopes throughout our own and many other galaxies.

Our civilization is within reach of one of the greatest steps in its evolution: knowledge of the existence, nature, and activities of independent civilizations in space. At this instant, through this very document, are perhaps passing radio waves bearing the conversations of distant creatures—conversations that we could record if we but pointed a telescope in the right direction and turned to the proper frequency.

An assurance of rapid results cannot be made in a search for extraterrestrial civilizations. Such a search is akin to the one for the proverbial needle, but in this case the haystack contains three dimensions of space and two more of time and frequency, and there may be no needle. It is only our knowledge of the value of that needle, if it exists, that compels us to pursue such a difficult objective.

We can be helped greatly by any reasoning that will decrease the amount of space, time, and frequency that must be combed. But here, we are not only ignorant in many areas, but we are not even sure that we have recognized all the important considerations. We do not know the longevity of civilizations that are power-radiating, hence power-dissipating, systems. How quickly does a civilization, under the pressures of economy, become invisible—not as a result of inadequate technology but rather of superior technology? Are self-destroying wars a common destiny of civilizations? Despite the enormous times required for spacecraft to traverse interstellar distances, might other civilizations have circumvented this problem and launched large numbers of spacecraft rather than radio or light waves into space? Do civilizations indeed convert the material of their planetary system into the greatest possible living space and thus produce an unusual, perhaps easily detectable type of object in the sky? These questions must be answered if we are to conduct efficiently a search for other civilizations. The answers are also among the most interesting facts we could learn of life elsewhere. We find ourselves in that worst of fixes: to solve a problem, we need to know the answer in advance. There is no solution but the difficult one—to search a wide range of space, time, and electromagnetic frequency.

There are some arguments, none absolutely compelling, that can direct us to search modes possibly more likely to succeed. Most of these arguments apply if other civilizations are purposely trying to make their presence known. In this case, one argument is that the method that is most economical in energy is the most likely to be used. This leads to the deduction that electromagnetic radiation is by far the most favorable communications mode. This argument can be carried further by observing that the quantum nature of light causes lower electromagnetic frequencies to be more economical in transmitting information. However, the existence of cosmic radio noise renders the lowest frequencies less efficient. The frequency band of maximum economy is fairly well defined and is in the centimeter-wavelength region. Thus purposeful attempts by others to call attention to themselves are likely to be at such wavelengths, and it is sensible to emphasize searches at such wavelengths. It is a favorable circumstance that much conventional radio-astronomy research is done near these optimum wavelengths.

The 1000-ft radio telescope at Arecibo, Puerto Rico, is capable of communicating with a similar instrument at a distance of 1000 light-years. (*Photo courtesy of National Astronomy and Ionosphere Center.*)

It is clearly restrictive, however, to construct a search that aims to detect only signals generated to attract our attention. After all, we make no such transmissions, and perhaps few other civilizations do. It would be preferable to be able to detect the signals a civilization uses for its own purposes. If other civilizations exist, such signals are surely far more numerous, but unfortunately much weaker, and at completely unpredictable frequencies. Nevertheless, methods have been found that use the information from a telescope to determine if there is present an ensemble of signals typical of a civilization. Thus civilizations may be detected even though no individual signal rises above the noise of the telescope. Such methods call for observations of a large number of frequencies and extensive analyses of the recordings with high-speed computers; the technology is available.

Indeed there exist the know-how and instruments to search for extraterrestrial civilizations. The reach of our instruments is probably greatest in the radio region, which is one of the most promising wavelength regions in which to search. For example, the largest radio telescope, 1000 ft in diameter, could detect a civilization beaming signals with a similar telescope at a distance of some 1000 light-years. Some of the natural pulsar signals recorded by radio telescopes closely resemble signals that might be received from transmitters of other civilizations.

Despite the power and promise of our instruments for serious searches for other civilizations, no major search has taken place. The explanation lies in the intense pressure on major astronomical instruments to produce the astrophysical results that are the mainstream of astronomical research. Because we cannot accurately predict the effort needed to detect another civilization, quick results cannot be guaranteed. Indeed, the time estimated for a single radio telescope to yield a reasonable probability of success is a few decades, even with high-speed equipment and procedures. In today's rush such a time scale is usually considered unacceptable.

Nevertheless, each passing year has seen our estimates of the probability of life in space increase, along with our capabilities for detecting it. More and more scientists feel that contact with other civilizations is no longer something beyond our dreams but a natural event in the history of mankind that will perhaps occur in the lifetime of many of us. The promise is now too great, either to turn away from it or to wait much longer before devoting major resources to a search for other intelligent beings. For the time being, the discovery may come by chance, for many of the observations we now make of natural objects are done using methods that are suitable for detecting intelligent life—the studies of pulsars and of infrared sources are examples. In the relatively near future we foresee the construction of major facilities, such as a giant radio receiving array, and the operation of a project that will have as its goal the detection of intelligent life elsewhere.

In the long run this may be one of science's most important and most profound contribution to mankind and to our civilization.

CHAPTER FOUR

The Dimensions of American Astronomy and Astrophysics in the 1970's

INTRODUCTION

The predecessor to the present study, *Ground-Based Astronomy: A Ten-Year Program*, was prepared in 1964 by a Panel on Astronomical Facilities headed by A. E. Whitford. The reader can refer to the Whitford report for statistical data for trends before 1960. The panel noted the increase in number of graduate students enrolled in astronomy departments, which, if continued, would increase the astronomy PhD population growth to 19 or 20 percent from the 4 percent annual rate that held from 1920 to 1960. This growth was partly caused by the earlier surge in astronomical novelty and interest accompanying the development of radio and space astronomy. The manpower increase has continued to the present day at about the rate predicted by the Whitford report.

The postwar decade had been a period of generous federal support for astronomical research, principally from the Office of Naval Research and the National Science Foundation. In 1958, these two were joined by the new National Aeronautics and Space Administration, whose expenditures for space astronomical telescopes soon exceeded earlier combined programs. At about the same time, defense agencies, finding that advanced astronomical instrumentation such as radio and space telescopes were excellent testing grounds for technology of potential defense interest, supported advanced radio observatories, rockets, and earth-orbiting telescopes. Even the small fraction of such efforts devoted to basic scientific research represented a significant increase in astronomical resources. But as we shall see, these funding increases, unlike the manpower, have leveled off in recent years.

At the time of the Whitford report (1964), astronomy was in the midst of an unparalleled growth in people and financial support. There was also under way a qualitative change in observing techniques. Until the 1940's, astronomical technology was limited to photographic, simple photoelectric, bolometric, and low- or high-resolution spectroscopic measures of photons in one octave of the electromagnetic spectrum, the optical "window" in the earth's atmospheric transmission. In the 1950's, radio astronomy developed detectors and telescopes to exploit the radio "window." The technique of radio interferometry steadily increased the resolution and pointing accuracy of radio telescopes, until today they surpass optical telescopes. By 1960, telescopes above the earth's atmosphere opened new wavelength regimes as fast as detection methods could be pressed into service. Infrared observations from the ground, balloons, and airplanes revealed important new types of objects. Moreover, nonelectromagnetic astronomies were developed. Cosmic-ray physicists identified high-energy particle fluxes with astronomical sources like the sun and solar wind. Physicists built neutrino detecting devices in the difficult attempt to measure the solar flux of neutrinos; other physicists built gravitational-wave detectors to locate massive cosmic events such as supernovae explosions or gravitational collapse. Unlike manpower and funding, this increase in the range of observing techniques cannot be charted numerically on a graph. To gauge the extent of change, the reader should compare the Whitford report's two programs in optical and radio astronomy with the diversity of programs recommended in the present study.

An explosion in dramatic discoveries occurred. The current era of new ideas in astrophysics perhaps started with the ground-based optical measurement of the extremely large red shifts of the quasars in 1963. From that time to the present has been recognized by many as a new golden age of astronomy. Many discoveries, including the quasars with the associated question of their natures and distances, the cosmic fireball radiation, the pulsars or neutron stars, the complex interstellar molecules and interstellar masers, and the radar discovery that Venus rotates backward, result from our ability to view the universe in the radio parts of the spectrum. But others result from observations made possible only by the opening of the entire electromagnetic spectrum to view, from telescopes above the atmosphere in space, with rockets, balloons, and stratospheric airplanes. These include x-ray stars, the diffuse x-ray background (that may reveal vast new amounts of invisible matter between the galaxies), infrared galaxies (whose energy output exceeds even the quasars), cool infrared stars (that may include planetary systems in the process of formation), and the rocket ultraviolet discovery of hydrogen molecules in interstellar space. The net effect of such new discoveries placed an even greater load on the few large ground-based telescopes, operating through their limited window but necessary for backup and detailed study of the physics of these strange classes of objects.

The result has been to arouse the interest of nonastronomers, from laymen to other physical scientists, to an extent unparalleled in modern times. Thus, in its report to Congress for 1970 the National Science Board commented, "the rapid pace of discovery in astronomy and astrophysics during the last few years has given this field an excitement unsurpassed in any other area of the physical sciences."

This flood of discoveries and the high interest in astronomy and astrophysics come at a time when financial support has leveled off—as it has for almost all U.S. research and development. This combination of new discoveries at the frontier of physical science and a downtrend in real purchasing power of federal funding forebodes an imminent crisis in support *per scientist* trying to do research in astrophysics at a time when the quality, depth of education, and technological skills of those entering the profession are the highest that they have been. This is compounded by growth in another source of PhD researchers in astrophysics—physics departments. There are now about as many PhD's coming to work each year in astrophysics with a PhD physics background as with a PhD astronomy background. At the time of the Whitford report, about one quarter of the PhD researchers in astronomy had received their degree in physics.

In order to put our recommendations in context, we present in graphic and tabular form, a brief statistical survey of the resources—manpower, capital equipment, and financial support—now available to U.S. astronomers for astronomical research.

For the sake of brevity, only the most essential data will be provided. Most of the numbers given are based on surveys or fiscal computations, which, while done as carefully as possible, are necessarily subject to some incompleteness. The reader concerned with the precision and sources of these data is referred to the statistical sections and extensive appendixes to Volume 2 of this report. We believe the values of the numbers given here to be accurate in most cases to 10 percent.

TRAINED MANPOWER

Trends in the numbers of persons employed in astronomy are shown in Figure 1. The relatively steep increase in the numbers employed during the period 1963–1969 has leveled off in the last year. Not all the employed PhD's received their doctorates in astronomy. While in 1966, 26 percent of

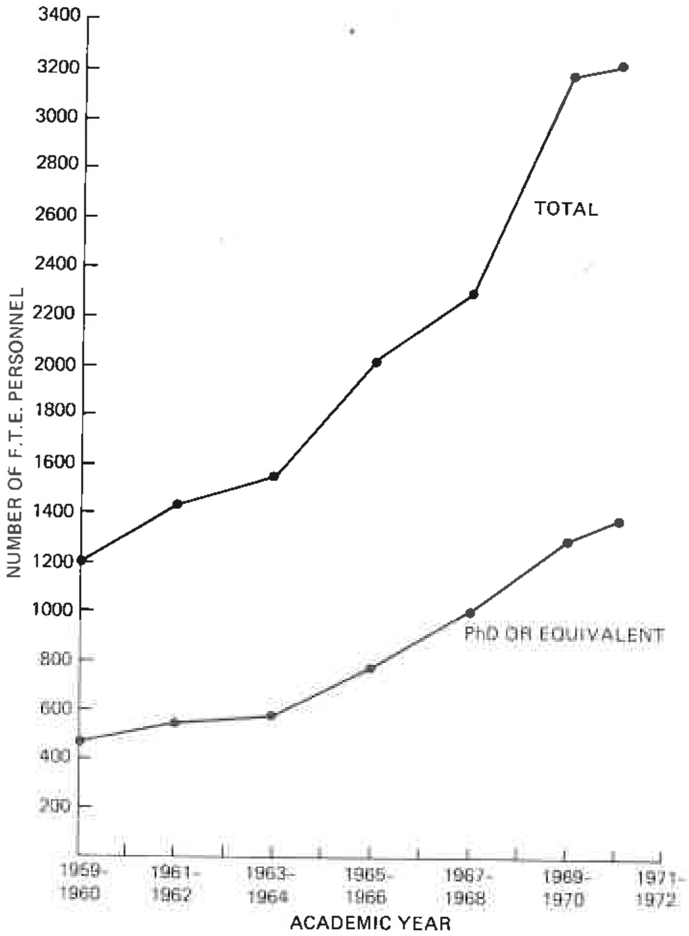


FIGURE 1 Number of scientific and technical personnel employed in astronomy in the United States.

them had received their doctorates in physics, this increased to 45 percent in 1970, reflecting the increasing interest that physicists are showing in both theoretical and experimental astronomy.

The trend in the annual rate of production of new PhD's and master's in astronomy in the United States is shown in Figure 2. There are signs that the rate of production of graduate degrees in astronomy will level off in the next year or two. This trend is suggested by data on the numbers of graduate students in astronomy, which increased by 5 percent per year between 1969–1970 and 1970–1971 after climbing more than twice as fast during the previous nine years. Preliminary data suggest that a *decrease* of about 5 percent may occur in 1971–1972.

In the light of current concern about job opportunities for new master's and doctoral degree holders in all the sciences, a survey of new PhD's in astronomy was carried out in the late spring of 1971; 289 individuals who had received their degrees in 1966–1970 were queried. The fraction of those who responded, who included information about their area of employment, was 64 percent. Of these at the time of the survey:

83% held jobs in astronomy,
6% held jobs in an astronomy-related area,
10% held jobs outside astronomy,
1% were unemployed.

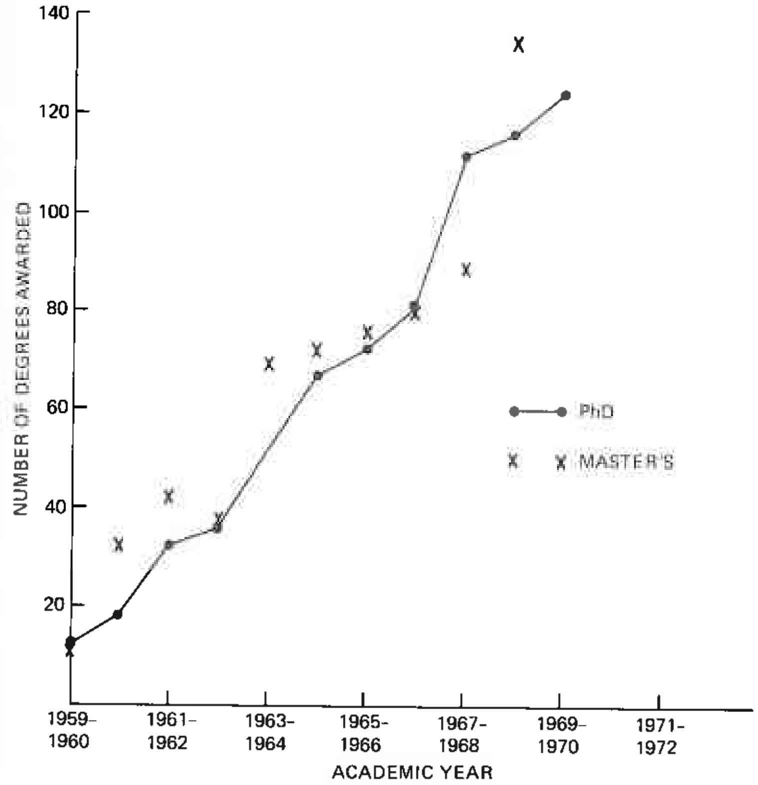


FIGURE 2 Number of advanced degrees awarded in astronomy in the United States.

Apart from uncertainty with regard to those who did not respond, this situation does not appear ominous. However, the responses to the survey also indicated that the 1970 PhD's had to work much harder than their predecessors to find positions. Cartter [*Science, 172,* 132 (1971)] has forecast a decrease in the availability of new faculty positions in all academic fields during the 1970's. This is reflected in the results of our survey, which show that whereas 80 percent of the 1969–1967 PhD's found jobs on campuses, less than 40 percent of the 1969–1970 PhD's found jobs of this type.

It seems that employment opportunities in the 1970's for holders of graduate degrees in astronomy will depend critically on the magnitude of federally supported research and development programs in astronomy during that period.* If the present rate of astronomy PhD production is maintained, it would be more than sufficient to meet foreseeable demand, given that a substantial fraction of those entering the profession obtain degrees in physics. If federal support is not substantially increased, then, at the present rate of PhD production, an increasing fraction of new PhD's will be obliged to seek employment in jobs that are not directly connected with astronomy.

It is difficult to make policy recommendations concerning the future production of PhD's in astronomy, since the demand will depend on the level of federal support of astronomy, which is uncertain. There is a clear implication, however, that it would be unwise to increase the present rate of production or to increase the number of institutions that are training graduate students in this area.

Table 1 provides data on the characteristics and research interests of a sample of scientific and technical personnel employed in 1968. The sample is estimated to be about 90 percent complete for PhD's and equivalents, and perhaps 75 percent complete for those with lesser training or experience. Table 2 includes information concerning the activities of astronomers at academic institutions.

The research interests of what we believe to be a better than 90 percent sample of PhD (or equivalent) individuals engaged in graduate instruction or research or both are displayed in Table 3. Also included is the distribution of thesis topics of a sample of recent PhD's.

It will be noted from Table 3 that a substantially larger percentage of

*If, for example, the magnitude of support for universities is doubled by the end of the next decade, approximately 880 new PhD's would be required to fill the new research positions that would be created [based on our survey, which shous that about this number of F.T.E. (full-time equivalent) PhD's are now engaged in research]. Another 100 might be needed to fill anticipated new undergraduate teaching positions (based on our count of about 200 F.T.E.'s now in such positions and anticipated growth in college enrollment). Perhaps 150 more might be needed to replace individuals lost through death and retirement.

TABLE 1 Characteristics of Astronomers, 1968 (Scientific and Technical Personnel)

Total Sample	1325	*Type of Employer*	
		Educational institutions	824
Age		Federal government	222
20–24	48	Nonprofit	48
25–29	411	Industry	115
30–34	290	Other, including military	31
35–39	198	Not employed and no report	85
40–44	135		
45–49	79	*Principal Work Activity*	
		Basic research	670
50–54	48	Applied research	124
55–59	40	Management or administration	
60–64	38	Research and development	102
65–69	24	Other	39
70 or over	8	Teaching	213
		Other	40
No report	6	Not employed	54

TABLE 2 Activities of Astronomers at Academic Institutions, 1970–1971

F.T.E.	employment (PhD or equivalent)	853
F.T.E.	engaged in graduate instruction and supervision of thesis research	35%
F.T.E.	engaged in research other than thesis research	43%
F.T.E.	engaged in undergraduate instruction	22%
	Number of undergraduate course enrollments in astronomy at institutions with astronomy departments (or the equivalent)	52,000

TABLE 3 Research Interests of Astronomers, 1969–1970

Type of Employer:	Academic	Nonacademic	
F.T.E. So Employed	622	511	
Interests	Academic (%)	Nonacademic (%)	Thesis Topic of PhD's 1967–71 (%)
Ground-based optical observations of objects outside the solar system	21	7	26
Ground-based radio observations of objects outside the solar system	10	8	6
Ground-based optical and radio observations of the solar system, but excluding the sun	6	8	2
Ground-based optical observations of the sun	3	7	3
Ground-based radio observations of the sun	2	1	<1
Space-based observations of all kinds, of all objects other than the sun	8	13	4
Spaced-based observations of the sun	2	7	2
Laboratory astrophysics	6	10	2
General-purpose instrument development	10	9	5
Astrometry and celestial mechanics	4	9	8
Theoretical astrophysics	25	15	36
Not classified elsewhere	3	6	6

astronomers is in ground-based observations than that involved in observations from space vehicles. This is undoubtedly due in part to the limited opportunities for making observations from space vehicles. It should be kept in mind, however, that about half of the total personnel are not directly engaged in observing, yet many of them are engaged in activities that support or complement activities of people making space-based observations.

FINANCIAL SUPPORT

Trends in federal obligations in support of basic research in astronomy are depicted in Figure 3. A large proportion of the total fiscal support for astronomy is funded through the National Aeronautics and Space Administration (NASA). This is evidently the case because of (1) the high cost of the instrument packages for spaceflights—these are of novel design, must be able to survive in the space environment, and must be extraordinarily

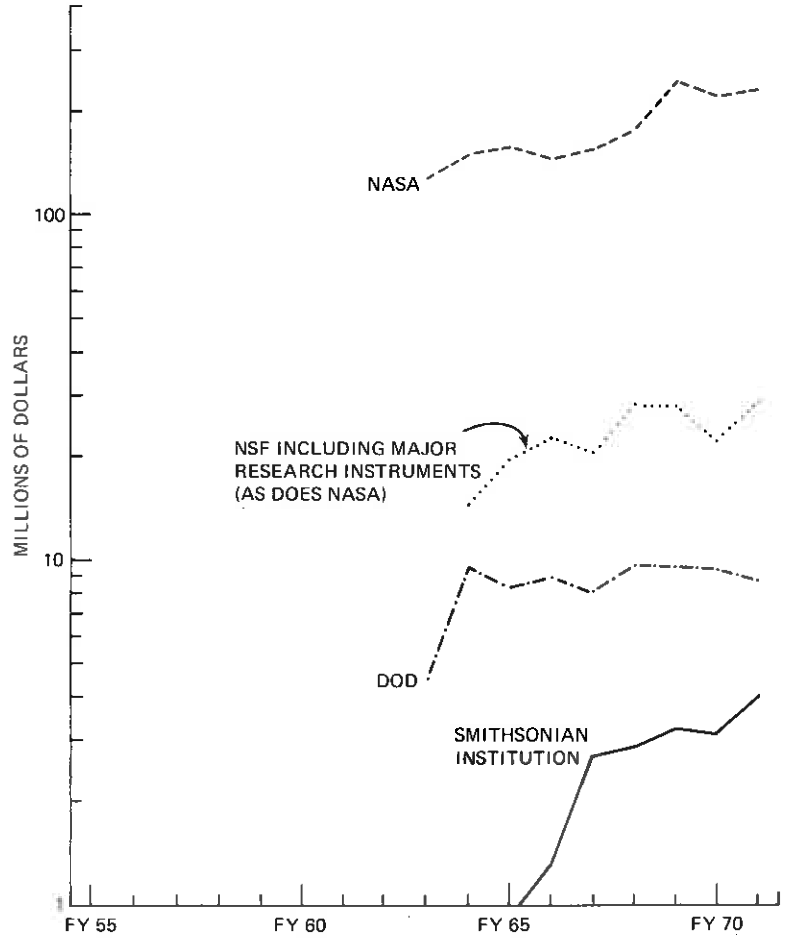


FIGURE 3 Federal obligations for basic research in astronomy.

reliable since they usually cannot be repaired after they have been launched; (2) sophisticated and expensive launching and guidance devices must be provided; (3) extensive facilities for launching, telemetry, and ground-based computing and control must be built, maintained, and manned. A large number of other nonscientific personnel, as well as aerospace engineers, are supported by the NASA funds, and the experiments have wider applications within astronomy than is indicated by the number of astronomers directly involved. The "direct" costs for some of NASA's major astronomical observing programs are shown in Figures 4 and 5.

In addition to the "direct" program costs shown in the two figures, it is estimated that NASA has obligated or will obligate between $10 million and $15 million annually (during fiscal years 1969–1971) on astronomical work that is part of, and budgeted through, its Lunar and Planetary Program. It is also important to note that (during fiscal years 1969–1971), out

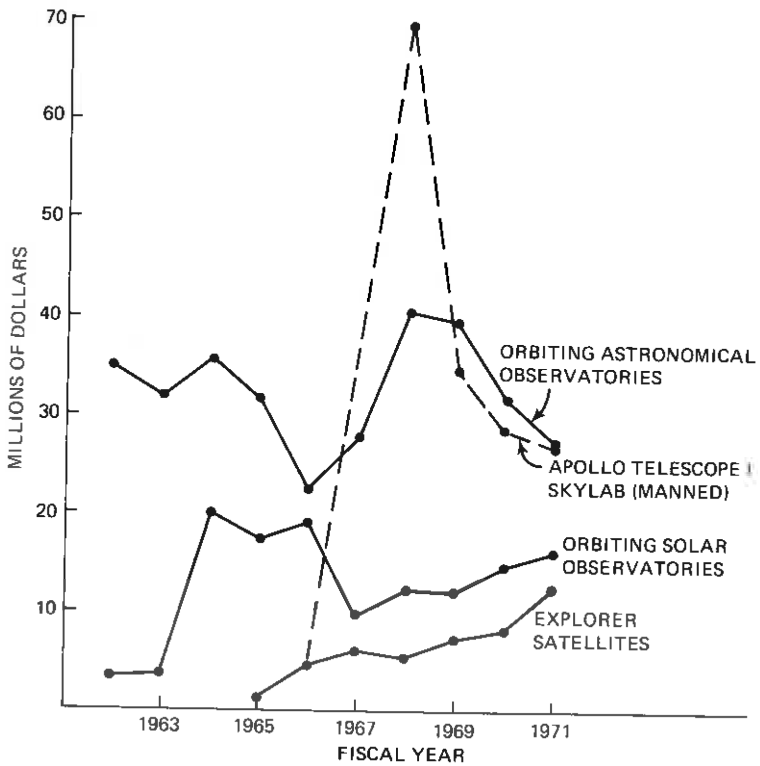


FIGURE 4 NASA budgets for specific astronomy programs that involve the use of satellite observatories.

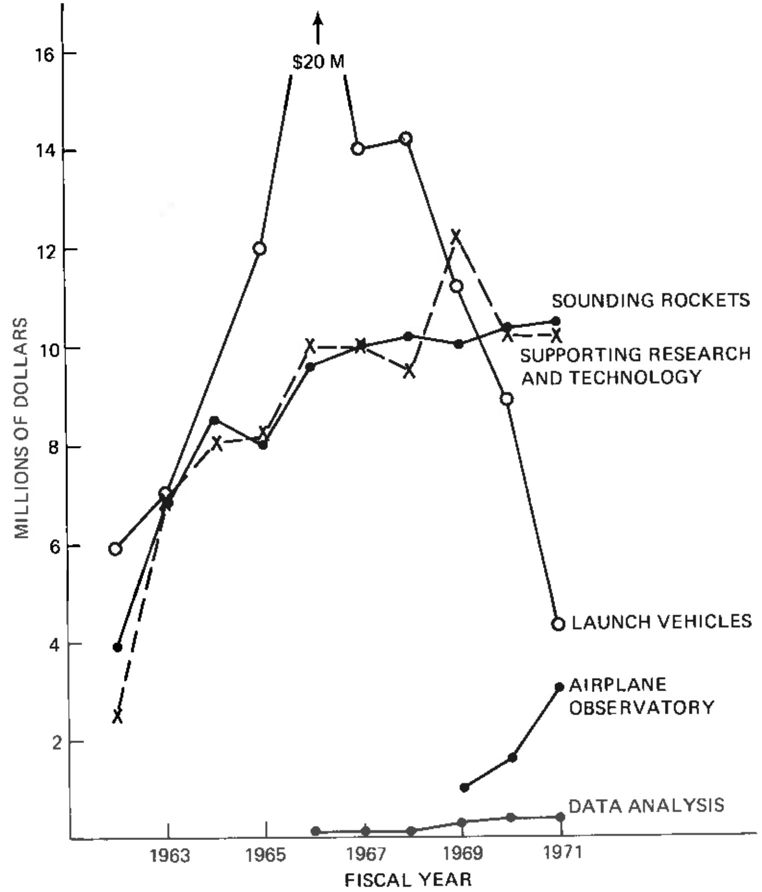


FIGURE 5 NASA budgets for specific astronomy programs that do not involve satellite observatories.

of its general costs for vehicle tracking, data acquisition, and administrative costs (including the costs of operating its research centers), NASA estimates that between $90 million and $100 million annually should be apportioned as the "indirect cost" of maintaining its astronomy programs.

Since launches of individual observing vehicles involve very high costs and intricate scheduling problems, it is natural that NASA budgeting should identify the costs of programs that employ specific types of vehicles. The National Science Foundation (NSF), however, is primarily engaged in the support of many-sided research programs at ground-based observatories and institutions. Except where major new observing facilities are to be constructed, it is more meaningful to describe NSF's support programs in terms of the kind of institution toward which the programs are directed or the kinds of activity within institutions for which they are primarily intended (e.g., education, research, capital equipment). Figure 6 provides this kind of breakdown. Virtually all that portion of the indirect costs that is paid by NSF is included. Where a large investment is made in a new piece of ground-based equipment, the expected useful life of that equipment usually ranges from a few years to many decades. This is in contrast to flight packages, which are usually designed to have an operating life ranging from a few minutes to perhaps one year, although some spacecraft experiments have much longer useful lifetimes. Thus investments in ground-based equipment can usually be regarded as additions to the available capital plant.

In fiscal year 1970 and fiscal year 1971 the budgets for the National Astronomical Observatories were approximately as shown in Table 4. The

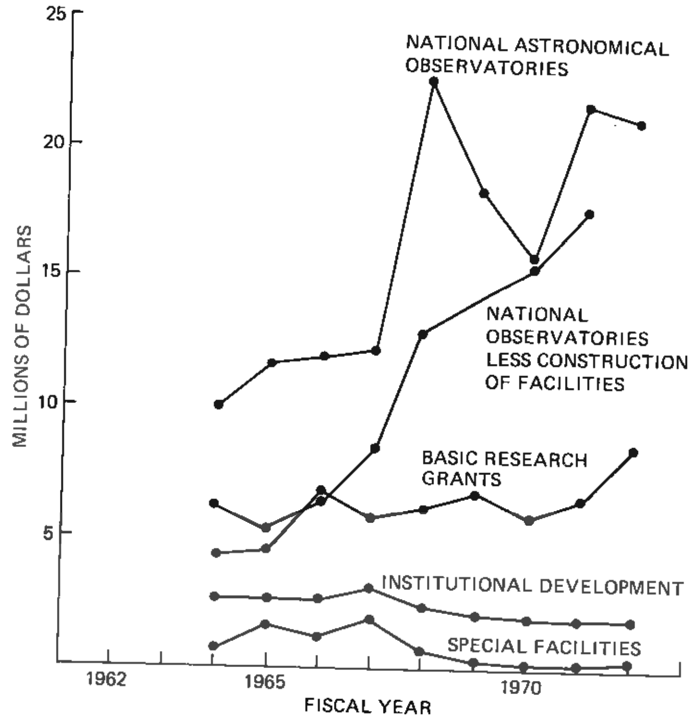


FIGURE 6 NSF obligations for astronomy.

TABLE 4 NSF National Astronomical Observatory Budgets ($millions)

	Total		Construction of Facilities	
	FY70	FY71	FY70	FY71
National Astronomy and Ionosphere Center (Arecibo)	1.6	6.1	0	3.8[a]
Cerro Tololo Inter-American Observatory	1.9	2.3	0.4	0.3
Kitt Peak National Observatory	6.5	7.2	0.1	0.1
National Radio Astronomy Observatory	5.9	6.9	0.7	0

[a]Partial cost of resurfacing the Arecibo dish.

completion of previously authorized facilities is included only where new money was obligated.

It should be noted that NSF Basic Research Grants are made in response to proposals submitted by individual investigators for the support of specific research programs. While some equipment may be provided under such grants, the bulk of the research done under these grants is performed with equipment already available to the investigator.

While the NSF is a primary supporter of ground-based astronomy, substantial support is provided by other federal agencies, as shown in Table 5. Between 20 and 30 percent of the total in this table (fiscal years 1966–1970) has been identified as ground-based radio astronomy. A good portion of the decrease in total support between fiscal year 1968 and fiscal year 1970 ($12.3 million) can be attributed to a $9.6 million decrease in the

TABLE 5 Federal Support of Ground-Based Astronomy, Including Facilities ($millions)

	FY66	FY67	FY68	FY69	FY70	FY71	FY72
National Science Foundation	22.9	22.7	30.4	26.4	23.3	30.3	29.9
National Aeronautics and Space Administration	9.4	10.0	10.0	10.0	9.0	8.0	9.0
Air Force	9.6	12.1	8.5	8.0	7.3	4.0	2.0
Navy	8.7	8.6	8.2	7.4	6.7	4.0	5.0
Advanced Research Projects Agency	2.6	2.8	2.7	1.7	1.3	0.0	—
Smithsonian Astrophysical Observatory	1.5	1.5	1.5	1.5	1.5	1.5	1.5
TOTAL	54.7	57.7	61.3	55.0	49.1	47.8	47.4

NSF budgets for construction for facilities at the national observatories.

We have endeavored to obtain estimates of the funds that institutions of higher education in the United States receive in support of research and education in astronomy. Difficulties are encountered because (1) in some cases it is not clear what fraction of a general-purpose grant should be attributed to astronomy and how much to other fields or purposes; (2) the returns from our survey were incomplete. Particularly in view of the latter fact, the numbers in Table 6 are probably low. Allowance for inflation is difficult in this context, but the pressures now felt at major private and state universities show that basic research is in jeopardy because of inflation.

The fiscal climate is markedly different now than when the Whitford panel formulated its ten-year program. From 1956 to 1964 the U.S. basic research budget had been climbing 20 percent annually. It has now slowed to 7.4 percent (in current dollars) and significantly less, 4.5 percent, in constant dollars. Even with astronomy holding its own, compared with other sciences, there has been a serious decline in the support per PhD, given their growth rate and transfers from other sciences. An additional pressure is the relative decrease in federal funds available outside the national centers, both for facilities and research, as shown in Figure 6. The NSF Basic Research Grant program (largely to universities) has fluctuated about a $6.5 million average annually, in current dollars, not responding to either inflation or the increase in manpower; the facilities program is now very small. Inspection of Table 6 and Figure 6 shows that the NSF's research grants covered only 10 percent of the support of astronomical research at academic institutions. In the period 1969–1971, NSF research and institutional development grants comprised 14 percent of the total. The Department of Defense (DOD) programs are level or decreasing and oriented toward applied research. The cessation of DOD support of Arecibo and Haystack has further increased the load on the NSF budget. We live in a quite critical time for the support of astronomical research.

TABLE 6 Support for Astronomy at Academic Institutions ($millions)

	For All Purposes		
Fiscal Year	Federal	Nonfederal (incl. indirect)	Total
1969	40	24	64
1970	41	26	67
1971	42	30	72

CAPITAL EQUIPMENT

In assembling data for this section, the Committee concentrated primarily on information regarding major telescopes. The buildings and laboratories in this country devoted to astronomy (other than the telescopes and associated domes) have a capital value of many tens of millions of dollars. Since astronomy is also being done with space vehicles, one could conclude that several millions of the dollars that have been expended on the design and construction of the Space Agency facilities represent a capital investment essential to this type of work.

Ground-Based Optical Telescopes

Table 7 contains a list of the largest U.S. optical telescopes, including those that are under construction or for which funding seems assured. All instruments of aperture greater than 70 in. were included. The reader is referred to Volume 2 for a more complete listing. Table 8 lists the largest U.S. solar optical telescopes.

It is difficult to make precise estimates of the costs of these telescopes in order to obtain the current cost of duplicating an instrument in the condition in which it exists today. The original design and engineering charges may be included in the first cost, but in other cases design, engineering,

TABLE 7 Largest U.S. Optical Astronomical Telescopes

Location	Aperture (in.)	Completion	Approximate Cost ($millions)
Palomar Mountain, Calif.	200	1948	25 in 1971[a]
Kitt Peak, Ariz.	158	Approx. 1972	10
Cerro Tololo, Chile	158	Approx. 1974	10
Mt. Hamilton, Calif.	120	1959	3
Ft. Davis, Tex.	107	1969	5.9
Las Campanas, Chile	100	Approx. 1975	5
Mt. Wilson, Calif.	100	1917	—
Kitt Peak, Ariz.	90	1969	2.5
Mauna Kea, Hawaii	88	1970	4.2[b]
Kitt Peak, Ariz.	84	1964	2.5
Ft. Davis, Tex.	82	1938	—

[a]Replacement cost with modern auxiliary instrumentation; initial cost $5.5 million.
[b]Cost was unusually high because, for technical reasons, a remote and very-high-altitude site was chosen.

TABLE 8 Largest U.S. Solar Optical Telescopes

Location	Aperture (in.)	Year of Completion and Cost ($millions)
Kitt Peak, Ariz.	82, 37, 33[a]	1962, $5
Sylmar, Calif.	24, 12[a]	1969, $1.0
Big Bear, Calif.	16, 10, 10, 9[a]	1969, $0.8
Sunspot, N.M.	30	1969, $4

[a]Several optical systems on the same mount.

and construction costs were contributed in such a way that they do not appear. Most large first-rate telescopes are steadily improved either in the structure itself or through addition of expensive auxiliary equipment, mostly from operating funds. It is not easy to know what price deflator to use when construction and improvement are extended over a number of years. Our estimates of costs of most instruments, except where noted, refer to the equipment as it existed at the first date of operation.

The ability of an optical telescope in a good location to produce observational results is proportional to the rate at which it can collect light from celestial objects. This rate is proportional to the area of its mirror. Using this simple criterion of the capability of a telescope, the combined capability of all U.S. telescopes can be measured in terms of the total collecting area of their mirrors. This total has grown over the last decade, as shown in Figure 7.

In 1971 we have already exceeded the Whitford report's 1976 goals for total collecting area of optical telescopes in the 36–59-in.- and 60–99-in.-diameter ranges. However, in the most significant 100–200-in. class, we are short of the 1976 goal, even when we include the two unfinished 150-in. telescopes under construction for the national observatories and the 100-in. southern hemisphere telescope of the Carnegie Institution of Washington. Two more 150-in. telescopes (17,700 sq. in. each) or one 200-in. telescope (31,400 sq. in.) are needed by 1976, even if we set no higher goal than did the Whitford panel.

The available collecting area has not increased during the past decade at as great a rate as the number of active astronomers or the demand for time to observe faint objects. The projected rise in collecting area of the largest telescopes (with apertures greater than 99 in.) will not be achieved until 1975, although all the post-1970 points shown on this curve represent instruments already funded, two of which have been under construction for a number of years. Telescopes in this class require from four to ten years for design and construction once funding is assured. Planning to meet anticipated need must be carried out at least this far in advance. While total collecting area is a useful criterion, it is the largest telescopes that must be used for observing the faintest celestial objects, such as those at the most distant parts of the universe. Any long-range national research program in astronomy will depend most critically on the available collecting area of the U.S. optical telescopes of the largest aperture.

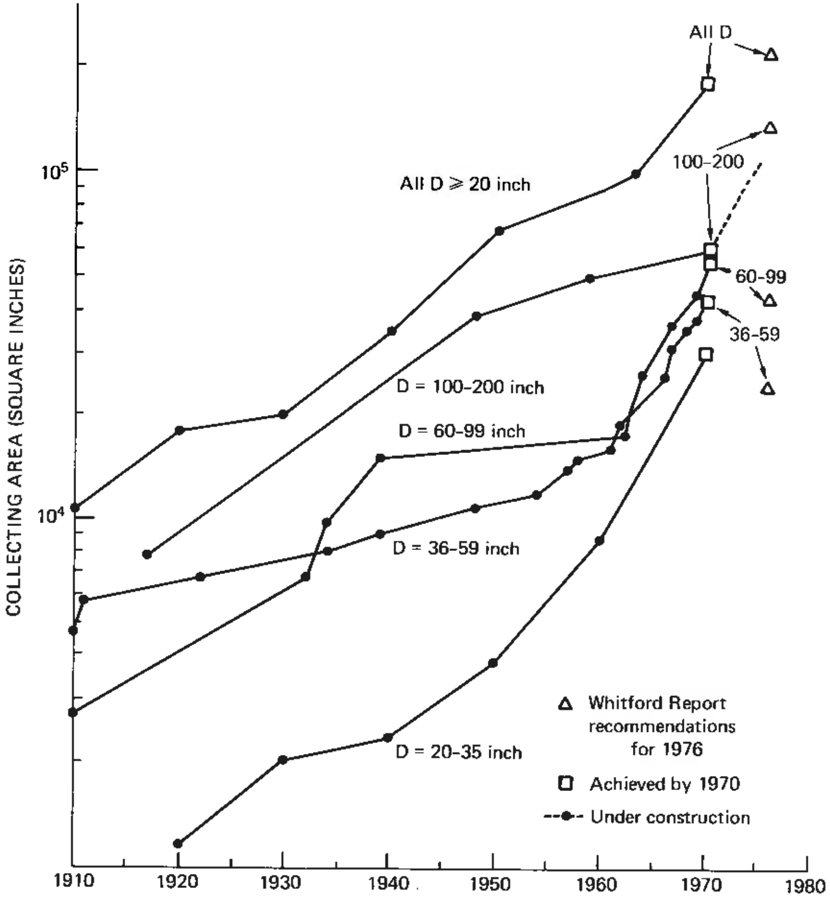


FIGURE 7 Cumulative collecting area of U.S. optical telescopes at astronomical institutions.

Ground-Based Radio Telescopes

Table 9 contains a list of the largest U.S. radio telescopes. Because many parameters are needed to define the over-all performance of such instruments, it is not feasible to set precise criteria for determining whether a particular instrument should be included in this table. In general, however, single steerable paraboloids of diameter less than 90 ft were excluded (with the exception of one with an extraordinarily high surface precision). The reader is again referred to Volume 2 of the report, where a more complete listing will be found. It should be pointed out that the largest, most elaborate, and best instrumented radio telescopes are critically important for the ultimate scientific problems of radio astronomy. Not only size but other performance criteria (surface accuracy, suitability for use in arrays, sophistication of receivers) must be taken into account.

The Whitford report made four important recommendations for new radio facilities: (1) a pencil-beam array of one hundred 85-ft steerable paraboloids, (2) an array of eight 130-ft steerable dishes, (3) two very large steerable paraboloids of about 300-ft diameter, and (4) a $3 million/year program of small and medium-size paraboloids and other instruments, including some with millimeter-wave surface accuracy, to be located at universities. It is hardly an overstatement to say that in 1972 essentially none of the Whitford program in radio astronomy had been implemented. Only the small-instrument category has shown progress, still without matching the 1971 goals. The steerable paraboloids designed and built for centimeter-wave research since 1964 include four 60-ft dishes, six 85-ft dishes, one 120-ft dish, one 130-ft dish, and one 210-ft dish (of the Deep Space Network). Of these, only one 60-ft, one 85-ft, one 120-ft, and one 130-ft telescopes are at university departments. Other telescopes were often built primarily for uses other than radio-astronomical research. For example, the 210-ft is NASA's major space communications telescope, two others are to be built for the Deep Space Network, and basic research must hold only a secondary place. In addition to the above centimeter-wave telescopes, four millimeter-wave dishes have been built since the Whitford report, one at an Air Force research station, one at the National Observatory, one by a university, and one at a NASA center. Of the large paraboloids, only the modest 120-ft University of Illinois telescope was planned and built since the Whitford report. None is under construction.

Space-Based Telescopes

Almost all of our experience with earth-orbiting satellite telescopes has been recent. Out of 13 astronomical missions attempted by NASA, two have been failures (both Orbiting Astronomical Observatories). The last seven successful missions, dating back to March 1967 and including four Orbiting Solar Observatories, the Radio Astronomy Explorer, the Orbiting Astronomical Observatory II, and the Small Astronomical Satellite *Uhuru*, are all still partially or fully operational. Table 10 contains some details. The impression that space instrumentation is extremely short-lived should change in the future. Other earth-orbiting experiments providing astronomical data are the Navy's series of Solar Radiation Satellites and the VELA nuclear-test-detection satellites of the Advanced Research Projects Agency, which provide significant x-ray data.

In addition to satellite missions, about three dozen astronomy experiments are performed from sounding rockets each year. Most of these are NASA missions, but the Kitt Peak Space Division (NSF), the Lawrence Radiation Laboratory (AEC), the Naval Research Laboratory (DOD), and the Air Force Cambridge Research Laboratories (DOD) also build and fly several rocket astronomy experiments each year.

The transparency of the atmosphere becomes sufficient at great altitudes to permit astronomical observations at gamma-ray and far-infrared wavelengths from stratospheric platforms such as balloons and airplanes. About 20 balloon flights per year are devoted to astronomical research by NASA. Two small jet airplanes capable of flying above the tropopause are available for infrared research, and a large NASA jet carrying a 36-in. infrared telescope will soon be in operation. Such high altitudes allow infrared telescopes to operate above much of the water vapor in the earth's atmosphere, complementing projected infrared satellites and the large space telescope. In addition, the Stratoscope series of balloon experiments has produced extremely high-resolution photographs of astronomical objects in visible light.

TABLE 9 Largest U.S. Radio Astronomical Telescopes

Location	Description	Highest Frequency Monitored	First Date of Operation; Approx. Cost	Remarks
Kitt Peak, Ariz.	36-ft steerable paraboloid	260 GHz	1967; $1M	High precision; mounted in radome
Clark Lake, Calif.	Sixteen log-periodic elements in 3300-m array	60 MHz	1968; $0.06M	
Goldstone, Calif.	210-ft steerable paraboloid	2300 MHz	1966; $12M	Primary use is for communication with space vehicles
Owens Valley, Calif.	Two 90-ft steerable paraboloids used as interferometer	11 GHz	1958; $2M	
Owens Valley, Calif.	130-ft steerable paraboloid	22 GHz	1966; $1.6M	
Stanford, Calif.	Thirty-two 3-ft steerable paraboloids in a cross array	3300 MHz	1960; $0.6M	Primarily for solar observations
Stanford, Calif.	Five 60-ft steerable paraboloids in linear array	10.7 GHz	Under construction; <2M	
Tyngsboro, Mass. (Haystack)	120-ft steerable paraboloid	38 GHz	1967; $6.5M	High precision; mounted in radome
Boulder, Colo.	Two corner reflectors, each 500 sq m in area	80 MHz	1959; $0.2M	Primarily for solar observations
Danville, Ill.	120-ft steerable paraboloid	2 GHz	1970; $0.5M	
Delaware, Ohio	340-ft × 70-ft fixed standing paraboloid with tiltable flat reflector	2.7 GHz	1961; $2.0M	Meridian transit type
Arecibo, Puerto Rico	1000-ft fixed spherical reflector with movable feeds	611 MHz	1963; $9M	
Greenbank, Va.	300-ft paraboloid, movable in N–S elevation	5 GHz	1962; $0.9M	Meridian transit type
Greenbank, Va.	140-ft steerable paraboloid	24 GHz	1965; $13.5M	
Greenbank, Va.	Three 85-ft steerable paraboloids used as interferometer	8 GHz	1965; $1.4M	

TABLE 10 U.S. Astronomical Telescopes Operating above the Earth's Atmosphere (NASA Missions) (August 1971)

Name	Date of Launch	Weight (lb)	Approx. Cost ($M)[a]	Mission/Remarks
OSO-III (OSO-E)[b]	Mar. 8, 1967	627	20	Solar physics: Similar to OSO-I and OSO-II; high-resolution spectral data within range of 8–1300 Å. Successfully completed second solar cycle. 7 of 9 experiments on
OSO-IV (OSO-D)[b]	Oct. 18, 1967	605	20	Solar physics: Continuation and expansion of data obtained by OSO program on high-resolution spectral data (within range of 1-1350 Å) from pointed solar experiments including raster scans of solar disk. 8 on 9 experiments on
Explorer XXXVIII (RAE-A)[c]	July 4, 1968	607	15	Radio astronomy: On Oct. 8, 1968, the four antennas were deployed to their full and final length of 750 ft (1500 tip-to-tip). On the same data, the damper boom was also extended to its full length of 315 ft (630 ft tip-to-tip). All antennas and booms are now fully deployed. 2 of 2 experiments on
OAO-II (A2)[b]	Dec. 7, 1968	4436	80	Astronomy: Carries eleven astronomical instruments developed by U. of Wisc. and the Smithsonian Astrophysical Obs. to investigate celestial objects in the ultraviolet region of the electromagnetic spectrum.
OSO-V (OSO-F)[b]	Jan. 22, 1969	620	25	Solar physics: The primary objective is to obtain high spectral resolution data (within the 1-1250-Å range) from on-board solar experiments pointed toward the sun. 6 of 8 experiments are fully operational, the remaining two are partially operative
OSO-VI (OSO-G)[c]	Aug. 9, 1969	638	25	Solar physics: The primary objective is to obtain high spectral resolution data (within the 10–20-keV and 1–1300-Å range). Seven experiment instruments on board are fully operational
Uhuru (SAS-I)[c]	Dec. 12, 1970	316	13	X-ray astronomy: Perform a sky survey of radiation sources between 0.1 and 60 Å to determine position, strength, spectral composition, time variations, and correlation with optical and radio celestial sources; discovered an x-ray pulsar

[a]Including cost of launch vehicle.
[b]Partially operational.
[c]Fully operational.

CHAPTER FIVE

The High-Priority Program

The first 11 Sections of this Chapter describe in detail the programs and facilities recommended as being of highest priority. Many more suggestions, which may be justified as of great urgency now or in the future, will be found in the individual panel reports of Volume 2. We describe here, in brief, scientific justifications and content of the programs we now recommend. The first four we view as of the very highest urgency and priority. The next seven are also essential to the health and balance of the total astronomical enterprise. The costs over a decade are approximately $600 million for the first four and $1200 million for the entire program. The rate of growth, as has been mentioned previously, is not large, and the manpower available or at present being trained is sufficient. In the final Section of this Chapter, we discuss a program of further new starts that we would have recommended if we had had only scientific goals in mind with no financial restrictions.

VERY LARGE ARRAY

The Committee recommends construction of a very large radio telescope array with the ability to observe the universe to great depth with unprecedented clarity. Such an instrument can break through existing observational barriers on a broad front and reveal important new lines of enquiry.

Radio telescopes have demonstrated their value by their involvement in an extraordinary number of discoveries in astronomy. These include the quasars, objects of unbelievable energy production and visibility at great distance; the pulsars; the universal blackbody radiation; and the detection of the vast ensemble of complex interstellar molecules. These discoveries owe much to the union of engineering and electronics, which has produced large radio telescopes capable of detecting incredibly faint signals. Indeed, all the radio-signal energy detected in our radio-astronomical history is little more than the energy released by the silent impact of a few snowflakes on the ground. Our telescopes can today detect easily the radiations of quasars to what we believe to be the edge of the observable universe. It is not surprising that there has been a flood of remarkable discoveries.

However, techniques that produce great signal sensitivity could not as readily give us an ability to see clearly. In fact, the limit on our ability to see has been the difficulty in distinguishing from one another the numerous objects that we can now detect in the sky; a blurred radio picture of the sky has been normal. Great effort has been invested in finding ways to see the sky clearly. Following the development of a new instrumental concept for high resolving power in Australia and England, several observatories in the United States have developed to a highly successful state a technique that can provide the resolving power so long sought after. This is the method called "aperture synthesis."

The basic technique of aperture synthesis involves the combining of signals received at two individual telescopes, retaining all the electrical characteristics of the signals, including the signal phase information. Such a pair of telescopes can resolve two point sources as well as can a single large telescope whose diameter is equal to the separation of the two antennas. Observations with a radio interferometer in which the separation of the antennas is increased from zero to some large dimension, perhaps miles, can produce as detailed a picture of the object as that produced by a single prohibitively expensive telescope of the same large dimension. A large number of geographical orientations of the line between the two antennas must be used for the method to succeed. Very high resolving powers can be achieved by this approach at relatively low cost. Indeed, several observatories have used this technique to achieve high-quality radio pictures of the sky with resolutions only ten times less than that achieved by optical telescopes. The method is, however, slow, and satisfactory progress requires simultaneous use of many antennas.

Many astronomical problems require a radio resolving power that approaches that of ground-based optical telescopes— ~1 sec of arc. The National Radio Astronomy Observatory has carried out extensive and detailed studies of aperture synthesis systems to achieve this goal. The result is a design that can achieve high-quality radio pictures of the required resolution at a rate of about two pictures of new regions per day. This ingenious design achieves this speed and resolution with a minimum cost by utilizing 27 antennas of 85-ft aperture, deployed in a carefully calculated pattern over an area 26 miles in diameter. The rotation of the earth over several hours causes the geometric separation of the antennas as seen from the sky to be altered to produce the required antenna orientations and separations. The antennas are controlled, and the information from them processed, by a central large computer system. This antenna system is called the Very Large Array (VLA) and will be by far the largest and most advanced radio-astronomical instrument ever constructed. It will produce the equivalent of a radio "eye" 20 miles in diameter. It is estimated that five years will be required to construct it at a cost of $62 million.

Although such a giant step in capability will certainly produce major discoveries and surprises that cannot now be predicted, there is an extensive ensemble of new results that can be foreseen. Particularly revealing will be the detailed pictures of radio galaxies and quasars, pictures that will show the distribution of high-energy particles and magnetic fields, allowing us to trace the evolution of these vast radiating regions as they are created by the violent explosive events in these objects. There will be high-resolution radio pictures of normal galaxies to compare with the radio galaxies and with our theories of the radio emission of normal galaxies and of the objects in them.

The VLA will be a major new tool for cosmology by virtue of its ability to distinguish large numbers of point sources one from another. A key cosmological problem is to plot a number-flux relation to very faint limiting fluxes, so one is sure to be including sources that are distant enough to distinguish different cosmological models. The VLA can count such sources because of its narrow beam and large collecting area. However, a more subtle problem is to eliminate from the count the numerous, but uninteresting, nearby sources that are intrinsically faint. At present, we are not sure how numerous such sources are. The VLA can determine this by observing all sources at a known distance, such as in a cluster of galaxies. The narrow beam will be decisive in distinguishing individual sources in such crowded regions.

There is some hope that spectral or other characteristics can be used to distinguish between intrinsically bright and faint sources; the multifrequency and polarization capabilities of the VLA will be important in this regard. Furthermore, if sources can be found which have a definite distribution of linear sizes, the high angular resolution of the VLA may be able to determine the angular sizes of such objects at large distances and therefore study the angular diameter-flux relation, which should be sensitive to cosmological effects. In summary, the VLA will be able to approach the solution to the cosmological problem by a variety of avenues.

The VLA will also open a new method for study of the stars—by providing information on the continuum radio emission of many normal stars. Just as radio telescopes have revealed important new information about high-energy envelopes of the sun, particularly about the solar corona, the VLA will give us our first opportunity to observe these phenomena in other stars, opening the door to important advances in stellar and plasma physics and perhaps providing clues to unsolved mysteries of the sun itself. Galactic novae have been observed with interferometers, and the VLA will give the detailed evolution of the clouds of plasma and gas ejected violently in the nova outburst. Perhaps emission from Wolf-Rayet, P Cygni, and magnetic stars will be detectable.

Prototypes of the VLA have measured the astonishing changes in the emission of x-ray stars in only hours. Nevertheless, the searches for x-ray star radio emission have been particularly frustrating, contributing little data toward the solution of the enigma of x-ray stars. The great improvement in sensitivity offered by the VLA may well remove a barrier to the understanding of these intriguing objects.

The VLA will give us for the first time a clear picture of the heart of our galaxy, where there is a complex ensemble of radio-emitting regions, concealed from optical telescopes by the dense dust clouds of the Milky Way. There is evidence that violent events in the nucleus of the galaxy have strongly influenced galatic evolution. Indeed, one object in the center may be the same type of structure that produces the quasar phenomenon.

By measuring the radiation of individual radio spectral lines, such as that of atomic hydrogen at 21-cm wavelength, the VLA will be able to give pictures of the gas clouds of our galaxy in such detail that we will see the processes taking place in them; the effects of heating, cooling, and supersonic collisions should all be discernible. The structure of the gas system of nearby galaxies will be sharply defined, testing theories of galactic dynamics and evolution.

The VLA will be able to distinguish detail in the radio emission of all the planets but Pluto, enabling the temperatures of the planets at various latitudes, seasons, and times of day to be established. The radiation belts of other planets could be measured in detail, and the atmospheric structure and nature of the planetary surface, be it rock, soil, or water-containing material, could be studied.

The VLA, and some other radio-astronomy facilities, will require a new site. It is possible that the large steerable dish or millimeter-wave dish could be located in the same area. Site development economies are possible in radio astronomy, since the major common requirement for all these instruments is a large area, free from industrial and radar electrical interference and direct aircraft routes. They all require highly developed technical support for receivers, computers, data analysis, and control. A dry, high-altitude site is preferable for the millimeter-wave dish, although not so important for the other devices.

With the program for the VLA, which will come into operation only near the end of this decade, we recommend expansion of research support and funding of moderate-sized instruments at university or consortium-operated radio observatories at a rate of $2.5 million per year. This will permit smaller groups to probe new areas of technology: new concepts in antenna and receiver design, ultra-high-frequency detectors, small millimeter-wave antennas and interferometers, centimeter-wave interferometers and receivers, adaptable to the new atomic and molecular lines discovered, and very-long-baseline interferometric terminals and arrays. A balanced program in radio astronomy requires a variety of less expensive facilities and innovative, flexible research projects, in addition to the large national facility described.

The costs over ten years for university facilities would be $25 million, and $62 million for the VLA. About $6 million per year (10 percent of the capital cost) will be required to operate the VLA. The full operating costs will not occur until the last half of the decade.

OPTICAL ASTRONOMY—ELECTRONIC TECHNOLOGY AND LIGHT-GATHERING POWER

We have witnessed a decade of remarkable discoveries in astronomy, including quasars, x-ray stars, and infrared galaxies. Most of these discoveries resulted from the expansion of astronomy into new regions of the electromagnetic spectrum, but observations at visual wavelengths have remained central in astronomy because they provide basic information about distance, mass, temperature, pressure, and chemical composition. Furthermore, through comparisons with well-established theories, optical astronomy is the basic tool for studying stellar evolution and nucleosynthesis, the ages of stars and clusters, the distances and stellar content of galaxies, and the scale of the universe. Moreover, optical astronomy has provided data that challenge established theories. For example, recent photographic advances have revealed puzzling phenomena in highly distorted galaxies.

For optical astronomy to fulfill all these roles, we must have telescopes to collect the photons and detectors to record them. Progress in astronomy has depended heavily on our ability to build larger telescopes and more efficient detectors. Introduction of refracting telescopes more than three centuries ago led gradually to a 500-fold improvement in angular resolution and permitted objects to be seen that are 10,000 times fainter than those that could be seen with the eye alone. These refractors were adequate for finding new planets and charting the stellar universe in the nearer parts of our Milky Way, but the astronomer was still left with only the memory of his personal visual perception.

Photography, beginning about a century ago, brought modern astronomy into being. Not only could each astronomer now share his vision with the world, but, equally important, he could extend it to objects a hundred times fainter, due to the ability of photographic emulsions to store light during long exposures. Photography unveiled the extragalactic universe, but the full appreciation of its size and grandeur depended on the parallel development of large reflecting telescopes through a progression culminating in the 200-in. reflecting telescope on Palomar Mountain, with its ability to study objects 10 million times fainter than can be seen with the unaided human eye. This great instrument, after nearly 25 years of use, still serves as the spearhead of world astronomy. It is worth noting that the 200-in. telescope was funded and designed during the presidency of Calvin Coolidge, before the space age and even before the first nuclear accelerators or radio telescopes. Some of the smaller telescopes still in active use in the country are nearly 100 years old.

Since there has been only modest improvement in the efficiency of photographic emulsions during the last 50 years, the building of ever-larger telescopes was aimed almost entirely toward collecting more light. The cost of conventional telescopes increases nearly with the cube of the aperture, making this an expensive, although necessary, pursuit. Consequently, astronomers began to investigate techniques that would detect photons more effectively than the photographic plate, which at best can record 1 out of every 100 photons collected by the telescope. The introduction of photomultipliers with quantum efficiencies up to 25 percent was a major improvement, but they were limited to view a single resolution element of an image at a time. Detectors were needed that would combine the high sensitivity of the photocathode with the ability of the photograph to record all parts of a large two-dimensional picture at the same time.

The first objective has been accomplished in the last few years by developments that include (1) image intensifiers in which photoelectrons from a cathode excite a phosphor screen that is then photographed, (2) electronographic cameras in which the photoelectrons strike a photographic emulsion directly, and (3) integrating television cameras in which the photoelectrons are stored in a target that can be read out with an electron beam. These techniques have in turn pointed to ultimate systems that will count individual photoelectrons focused onto a two-dimensional array of sensitive elements. In some of these systems, as the data are obtained, they can be read into a computer for immediate processing so that the astronomer can watch the image build and optimize the exposure.

The impact of these developments on astronomy has been enormous. In many situations they render present telescopes up to 25 times more effective than before. This is equivalent to scaling each existing 40-in. telescope into a 200-in. and the 200-in. into a 1000-in. If a 1000-in. telescope could be built, it would cost $2 billion; the replacement cost of the 200-in. is now near $25 million. The equivalent cost of such a fivefold transformation, assuming it could be done in the old way by actually rebuilding existing telescopes, would be at least $5 billion, whereas the cost of equipping all major American telescopes with such devices will be much less than 1 percent of this. These factors amply account for the unanimity of astronomers in giving high priority to the development of these electrooptical detectors and their installation on large telescopes.

Additional improvements can come from the more efficient use of telescope time through various controls for automatic setting and guiding and television cameras for finding and tracking objects too faint (or too red) for the eye alone. At present, work on invisible objects requires the time-consuming procedure of offsetting the telescope from objects that can be seen.

The major effect of the new detectors will not be to observe the same objects in shorter time but rather to study much fainter objects and to use higher spectral resolution. This will permit critical investigations not thought possible 10 years ago, such as analyzing individual stars in nearby galaxies for element abundances, studying the absorption lines in the faintest quasars, and measuring red shifts of the most distant galaxies. However, even with these impressive advances in detectors and controls, we still need more large telescopes. Some of our major reflectors are near growing urban areas whose lights make the sky too bright for work on the fainter objects, and even the Palomar telescopes are already threatened. While we make all possible efforts to improve the efficiency of present telescopes, we must also build new ones at safe dark sites where there is good seeing. The cost of a very large single-mirror instrument is so high that we recommend experiments with the concept of an optical telescope array. In order to achieve a large collecting area at a moderate cost, initial efforts should be directed toward developing a multiple-mirror telescope with either an array of mirrors on a common mount or a system of separate telescopes feeding the same detector. If prototype tests prove these concepts feasible, an operating telescope of high optical quality equivalent in area to a 150- or 200-in. should be built, followed by the design and construction of a much larger system in the 400- to 600-in. class, if experience with the smaller one indicates that the next step will succeed. However, if the multiple-mirror telescope does not fulfill expectations, another conventional reflector of the 200-in. class should be built as soon as possible.

While the multiple system is being designed and tested, we must proceed with the construction of at least one standard telescope 90 in. or larger, at a dark site, in order to begin to compensate for those instruments that no longer can be used on the faintest objects because of the lights from expanding cities.

Funding of at least $10 million will be needed for the development of the new electrooptical detectors and installation of the best systems on all major U.S. telescopes. There are at least nine existing telescopes large enough to use one or more of these detectors profitably, three more under construction, and three proposed. Outfitting these telescopes with television cameras and automatic controls for setting and guiding as well as with small computers for immediate data reduction will cost another $5 million.

An operating multimirror telescope equivalent to a 150- to 200-in. single mirror is estimated to cost about $5 million. Further funding up to $25 million should then be provided to build the largest possible telescope within that budget—either a multiple-mirror one with an effective aperture of 400 to 600 in. if the concept proves to be feasible or a conventional 200-in. telescope. An additional $5 million is for the urgently needed intermediate-sized telescope at a dark site.

The well-rounded program in optical astronomy requires (1) advanced sensors and controls—$15 million, (2) test of array concept—$5 million, (3) a 100-in.-class telescope—$5 million, (4) construction of a large optical array or another 200-in.-class telescope—$25 million. Operating costs for the new optical facilities will reach $3.5 million per year by the end of the decade.

INFRARED ASTRONOMY

Although Herschel detected infrared radiation from the sun with a thermometer more than 170 years ago, it is only in the past decade that infrared observations have become important to the mainstream of astronomical research. Only recently have solid-state and low-temperature technologies developed to the point where available infrared detectors are sensitive enough to study objects other than the sun in any detail. Low-temperature techniques are especially important, because the earth's atmosphere and the telescope are strong sources of background radiation in the infrared and are thus seen by the detector. Infrared detectors must be cooled, often to temperatures as low as 2 K. Ideally, the entire telescope should also be cooled and then lifted into space to avoid contamination by atmospheric radiation. Going high in the atmoshere or into space would also extend the available range of wavelengths, because water vapor makes the atmosphere opaque in large portions of the infrared region of the spectrum. Unlike ultraviolet or x-ray astronomy, which can be conducted *only* from space, some infrared astronomy can be carried out through the atmosphere by large ground-based telescopes. At other wavelengths, the absorption by water vapor, if not the background radiation, can be overcome by observing from an aircraft or balloon above the tropopause.

The infrared has great potential for astronomical research. This part of the spectrum begins at the long-wavelength end of the visible spectrum, at about 1μm, and stretches over a range of more than ten octaves to about 1 mm, where it overlaps the short-wavelength end of the radio region of the spectrum. Within this range lies the characteristic blackbody radiation of the moon and planets, cool stars, and prestellar clouds, as well as the background radiation of the expanding universe. The infrared is useful for observing any object with a temperature between 3 and 3000 K. The infrared is the realm of molecular spectroscopy, the range wherein lie the vibrational–rotational bands and lines of many cosmically important molecules. Theoretical studies of the interstellar medium also indicate that many of the important heating and cooling mechanisms involve infrared radiations from atoms and ions.

But as always, it is the unexpected and surprising that is the most interesting. Photometric studies aimed initially at improving temperature and luminosity determinations for cool stars led to the discovery of excess infrared radiation from circumstellar dust shells. A ground-based sky survey found some enormously luminous "infrared stars" that are barely detectable with optical telescopes. Exploratory observations of peculiar galaxies and quasars in the near infrared soon led to the realization that some of these objects emit more energy in the infrared than in all other wavelength regions combined, an unexpected and still unexplained result. Rocket observations of the cosmic background radiation, initiated mainly as a check on what had already been learned in the radio region of the spectrum, found a much greater flux than had been expected, and the resolution of the discrepancy may have profound implications for cosmology.

The new technology and the new exciting problems uncovered attract a large number of astronomers, particularly young experimenters, into the field.

We recommend expansion of support for this vigorous activity in all areas, including development programs for more sensitive detectors, exploration of new high-altitude dry sites for infrared telescopes, and exploitation of multiplex spectroscopic techniques, as well as increased funding of ongoing ground-based, airborne, and rocket programs. So much has been done with so little money (less than $2 million per year) that a large payoff is almost sure to follow from a doubling of this effort.

As part of this expansion, we recommend an immediate start on a program of surveying the sky for objects bright in the far infrared. This is extremely important for understanding the nature of exploding galaxies and may uncover new and unexpected phenomena. The first step, a balloon survey down to a relatively bright limit, can be done immediately for less than $200,000.

We also foresee the future need for a telescope with a large collecting area and high angular resolution in the far infrared. Such an instrument must of necessity operate in the stratosphere, and we recommend that a design study be initiated soon to determine the most suitable and economic platform.

The growth of infrared astronomy is creating large demands on existing telescopes, most of which are neither at the best sites nor optimally designed for infrared work. We therefore recommend as one item in the increased infrared program, construction of moderate-sized infrared telescopes, particularly in the southern hemisphere. We also recommend construction of a large (3 to 4 m) infrared telescope (at a cost of $5 million) at the best available high-altitude site in the northern hemisphere.

Such a combined program of ground-based, airborne, and rocket infrared astronomy is sure to lead to many exciting discoveries in this new and expanding field. The total budget is estimated to be $25 million.

HIGH-ENERGY ASTRONOMICAL PROGRAM

During the first half of the last decade, the total "observing time" in x-ray astronomy had accumulated only to about one hour, through many rocket flights. During that hour it had become apparent that the x-ray sky is extraordinarily rich in new phenomena, and that vast and vital aspects of many optical and radio objects had not been appreciated from observations in those wavelengths.

The Crab nebula is not only one of the brightest objects in the x-ray sky, but it is also extraordinarily complex. A steady x-ray glow is emitted by electrons spiraling in the magnetic fields of the nebula. Pulsed x rays are emitted from the pulsar created in the spectacular supernova explosion of A.D. 1054, one of only two radio pulsars known to emit x rays. The x-ray spectrum extends up into the gamma-ray region.

Scorpius X-1, the brightest x-ray object most of the time, is associated with a blue starlike object with strong optical emission lines. X rays are emitted from a hot plasma in the vicinity of the blue object whose nature remains a mystery. It appears likely that many of the celestial x-ray sources in our galaxy are generally similar to Sco X-1.

Occasionally, a new x-ray source appears in the sky, is more intense than Sco X-1 for a few months, then declines until it is no longer detectable. We do not have good enough position measurements of these sources to attempt to identify them with optical objects. One of the first major discoveries of the *Uhuru* x-ray satellite has been a new class of x-ray sources that undergo regular (pulsarlike) and irregular fluctuations on a time scale between 0.1 and 10 sec. No optical identifications are yet available.

Many unusual galaxies are x-ray sources. These include strong radio galaxies (M87), quasars (3C273), Seyfert galaxies, and ordinary galaxies (the Magellanic Clouds show a complex x-ray structure). Tremendous amounts of energy are released in the x-ray region in some of these sources, posing serious challenges to our understanding of high-energy astrophysics.

Underlying all these sources is a diffuse x-ray glow that appears to be featureless. Many astronomers believe that the background x rays were created far away and long ago in the early cosmological history of our universe.

This brief and incomplete list of important discoveries in x-ray astronomy is reminiscent of the early exciting years of radio astronomy. A wide range of new phenomena had been found, but understanding of these phenomena was minimal. The search for understanding required much larger instruments, new techniques, better detectors, better spectral coverage of the sources, polarization measurements, and the ability to repeat observations for variability, a common feature of "compact" objects.

A similar pattern of development is needed in x-ray astronomy. Much larger-area detectors than have been flown are required in order to find and study faint sources. For the lower-energy x rays, focusing optical techniques, involving grazing-incidence instruments, should be flown. These will allow detailed pictures with high angular resolution to be obtained. They will also act as photon collectors, concentrating x-ray photons from weak sources on Bragg crystal spectrometers and on polarimeters so that the detailed spectral properties of the sources can be measured. Because the detectors used with focusing optics can be made very small, the unwanted detector background counting rate can be greatly reduced, facilitating measurements of extended sources and of the apparently isotropic x-ray background.

With this major instrumentation, very large numbers of x-ray sources should be discovered. Many new examples of the various classes of x-ray sources in our galaxy should be found, so that the full range of properties of these sources can be studied. Positional determinations of these sources should be greatly improved, thus allowing large numbers of them to be identified with optical objects. With the resulting ability to study the sources in many different wavelength ranges, our theoretical understanding of the character and structure of the sources should improve rapidly.

Of great importance will be the ability to point at x-ray sources steadily for hours at a time. Not only will this allow a major improvement in the statistics of the spectral measurements, but it will also permit studies of the time variations of the total x-ray emission and of individual spectral features. One of the principal striking characteristics of the galactic x-ray sources that have so far been found has been the temporal variability of the x-ray flux, ranging from rapid fluctuations to long-term changes. This characteristic is more frequently found in x-ray sources than in optical and radio sources.

The major instrumentation should also have extreme importance for studies of extragalactic x-ray sources. It should permit detection of individual sources in nearby galaxies and of emission from active galaxies and quasars to very great depths in space. More definitive measurements of hot plasma concentrated in clusters of galaxies will be possible, allowing a determination of whether sufficient masses of such plasma exist in the clusters to bind the galaxies gravitationally. Much more definitive measurements of the spectrum and isotropy (or lack of isotropy) of the background x rays will improve our understanding of the cosmology and early history of our universe.

The National Aeronautics and Space Administration (NASA) has recognized the richness and promise of this field of research by requesting congressional authorization for two large rotating High Energy Astronomical Observatories (HEAO's). These are to be large spacecraft in orbit about the earth, slowly rotating so that the instruments scan across the sky. These will be survey spacecraft, with a large collecting area intended to discover new faint x-ray sources, to measure their positions accurately, and to measure spectral properties. Combined with the x-ray instrumentation would be gamma-ray and cosmic-ray instruments.

The spacecraft will play an essential role in the future of astronomy. X-ray astronomy will increasingly become a partner to optical and radio astronomy as more x-ray sources are identified and their properties are correlated with those in other wavelength bands. It is possible that some types of x-ray source may never be optically identified, in which case we will be entirely dependent on HEAO techniques to study them.

NASA planning also calls for two pointable HEAO 's. These will be even more important to the future of x-ray astronomy than the rotating HEAO 's. They will permit short-time-scale fluctuations in intensity to be followed continuously and to be correlated with simultaneous optical, radio, and perhaps infrared observations from the ground. They will take advantage of focusing x-ray optics to concentrate the x-ray photons onto small detectors, where background problems can be reduced and angular structural information and positions can be obtained with high accuracy. It is important that NASA also seek authorization for the pointable HEAO 's as soon as possible, in order that there not be too great a time delay between the discovery of new x-ray objects by the first rotating HEAO and the detailed study of them by the first pointable HEAO.

A measure of the importance attached to x-ray astronomy by astronomers is that they have scheduled large blocks of time on major optical instruments to exploit the discoveries and positional measurements of new x-ray sources by the *Uhuru* x-ray satellite. This reflects their expectation that a number of optical identifications will be possible of the newly discovered x-ray sources. If this is the case, the HEAO program will make large demands on optical astronomy and probably also on infrared astronomy. There should be an expansion in major optical facilities to satisfy the requirements of x-ray astronomy. Extragalactic objects in which a major portion of the energy emission is in the infrared are also proving to be x-ray objects; it is possible that a similar correlation may exist among some classes of galactic x-ray objects. Thus an expansion in infrared facilities may also be required for support of x-ray astronomy.

The high-energy astronomical program given extremely high priority by the Committee includes the four HEAO 's in the NASA planning program, two rotating and two pointed, together with an associated expansion in optical and infrared facilities to provide the ground support required for the development of x-ray astronomy.

The estimated cost of the four HEAO missions is $380 million. In addition, at least one intermediate-sized optical telescope to support the program should be constructed at a cost of $5 million.

MILLIMETER-WAVE ANTENNA

One of the dramatic discoveries of the recent past was the detection in the clouds of interstellar space of an astonishing variety of molecular species. These findings contradicted our expectations that the formation of such molecules was a rare event and that their destruction was rapid because of the flood of ultraviolet light in the galaxy.

The species found range from the small, diatomic molecules, such as CO, CS, and CN, to such complex substances as cyanoacetylene, methyl alcohol, formaldehyde, and formamide, containing as many as six atoms. Carbon monoxide is present in an abundance some thousand times greater than other molecules, probably reflecting its resistance to dissociation by ultraviolet light. The molecules of greatest abundance are those found in our laboratories to form the basic constituents of biochemical systems. For instance, formaldehyde is a precursor of both amino acids and sugars in experiments simulating conditions on the primitive earth. Thus the molecules observed seem to indicate that the chemistry of life on earth is closely paralleled in interstellar space.

The diatomic molecules are almost always best observed at relatively short radio wavelengths of a few millimeters. They form the basic building blocks for the larger molecules, and the physical interpretation of their spectra is much simpler than for the larger molecules. The larger molecules have great significance, however, since they often possess a rich spectrum, both at centimeter and millimeter wavelengths, and form a particularly powerful tool for probing the physical conditions in the interstellar medium. High resolution is necessary to define the distribution of the molecules from which the modes of their formation and destruction can be studied. High sensitivity is necessary to discover large molecules, which may have low abundances, and other low-abundance substances such as molecules containing rare isotopes.

High resolution and high sensitivity require a very large steerable telescope with a very precise reflecting surface. Such a telescope has many other important uses, particularly for the study of variations of quasar spectra and intensities and planetary emissions.

Such a telescope is not easy to build, because it must maintain its geometry to accuracies of tenths of millimeters under the influence of changing gravity forces, wind, and thermal stresses. A great deal of research has been carried out at the National Radio Astronomy Observatory on such precise and stable telescopes. A new approach to telescope design, called the "homology telescope" has been developed, which appears capable of attaining the desired performance. Indeed, some of the principles of this approach have been applied successfully in the new 100-m radio telescope of the Max Planck Institut für Radioastronomie in Germany.

The very large radio telescope recommended for observations at millimeter wavelengths would very likely be a fully steerable parabolic reflector with an aperture of 215 ft, performing satisfactorily at wavelengths of 3 mm and longer. The cost of this instrument is not as well determined as that of the VLA but is estimated to be $10 million.

The construction of this telescope will provide a major capability in a particularly promising area of astronomical research and will capitalize on our receiver technology, momentum, and design capabilities in a field developed in the United States and in which the country is pre-eminent.

AIRCRAFT, BALLOONS, AND ROCKETS

An essential part of space research is carried out using small vehicles—aircraft, balloons, and rockets. They are relatively inexpensive and ideally suited for programs of observation with specialized instrumentation where a few minutes or hours of data-taking will accomplish the research objective. They have also been essential for testing astronomical instrumentation for use in space. These vehicles have proved invaluable in the past; their utility in the future is assured by the steadily increasing requirements for their use.

At a time of severe fiscal constraints, the reduction of the number and variety of large astronomical missions in space can, in part, be balanced by the initiation of much less costly programs utilizing small vehicles. These may be able to carry out some of the research contemplated in the abandoned missions, thus maintaining a degree of flexibility and vitality in the affected field of research. The scientifically sensible course of action is to increase funding for aircraft, balloons, and rockets when fewer major satellite experiments are planned. If satellite programs are increased, an accompanying increase in rocket research, with smaller but innovative goals, will lead to optimum satellite design and therefore be of high value.

Until recently, x-ray astronomy depended entirely upon rocket research. The x-ray sources were discovered by rockets, and quite accurate positions were measured for some of them with ingenious rocket instrumentation. Rocket measurements made during a lunar eclipse of the Crab nebula revealed that the x rays were not a point source. At the present time, rockets are proving essential to the further study of some x-ray phenomena discovered by the *Uhuru* x-ray satellite. Unexpectedly rapid x-ray fluctuations of the Cyg X-1 source were discovered utilizing the satellite, but since the satellite rotates, it is not suitable for following the fluctuations. Rockets are capable of pointing at a source like this for several minutes at a time, and missions can now be instrumented to provide the data essential to a better understanding of Cyg X-1. Since this source appears to be but one of several classes of strange x-ray objects, it is clear that there will be a pressing need for more x-ray astronomy rockets for the next several years—and most certainly through the era of the High Energy Astronomical Observatories.

Ultraviolet astronomy also began with rockets, first for studies of the sun and then for studies of the stars. Differences were found between theoretically calculated ultraviolet stellar spectra and the rocket observations. Rapid rates of mass loss from hot supergiant stars were discovered by spectroscopic observation in the ultraviolet. Perhaps one of the most important of the ultraviolet astronomical discoveries was that of molecular hydrogen in interstellar space. Today the bulk of the ultraviolet astronomical observations are carried out with an Orbiting Astronomical Observatory, but the instrumentation in this vehicle is relatively inflexible, even though it returns a great amount of data. It is necessary to supplement and enrich these data with selective rocket measurements using a wider range of instrumentation.

The loss of OAO-B has been a severe setback for ultraviolet astronomy. The authorized program will conclude with the launching of OAO-C in fiscal year 1973. For many years, the program of ultraviolet astronomy from spacecraft is likely to be modest even if new satellites such as the proposed SAS-D are authorized. In these circumstances, it will be all the more important that a supplementary program of rocket observations in the ultraviolet be provided to maintain vigor in this field of research. The instruments carried in these rockets may provide some of the measurements that would have been made by OAO-B. They also will provide an opportunity to exploit the discoveries made by OAO-A and OAO-C and will provide an important survey of certain classes of ultraviolet phenomena. There will undoubtedly be many celestial objects found in these ultraviolet studies that will turn out to pose important scientific puzzles, many of which can be further studied and elucidated by research using rockets.

Infrared astronomy now relies heavily upon aircraft and balloons. While a few infrared windows can be exploited from the ground, most of the wavelength region, and especially the far infrared, requires an observing platform above the bulk of the atmospheric water vapor. Observations from balloons and aircraft have given important new spectroscopic information in the infrared about the sun and planetary atmospheres. Observations from aircraft have detected high fluxes of radiation in the infrared from the cores of active galaxies and quasars. Large numbers of strong infrared sources near the center of the galaxy have been discovered during surveys made from aircraft and balloons.

NASA is providing an aircraft platform for a 36-in. infrared telescope, which should produce important new results. The Committee recommends that a first, crude, long-wavelength infrared sky survey be carried out from balloons in the near future. In the longer-range future, a deep-sky survey in the infrared will probably require satellite techniques, but these will require a prior rocket development program. Hence, infrared astronomy will be a major user of aircraft, balloons, and rockets in the next few years.

Solar research has been heavily dependent on rockets as well as on satellites in the Orbiting Solar Observatory series. These have produced detailed ultraviolet spectra and x-ray pictures. They have been flown on command at times of solar flares. There is a continuing need to supplement the satellite coverage of the sun with special, flexible, quick-response rocket instrumentation.

Thus essentially all the major areas of space astronomy have an expanding need for small research vehicles: aircraft, balloons, or rockets. The expenditure on these research vehicles for astronomical research presently amounts to $12 million to $13 million per year. The Committee strongly recommends that the expenditure for this type of research be doubled as rapidly as possible, certainly within the next three years.

SOLAR PROGRAM

The opening up of the extreme ultraviolet and x-ray region of the solar spectrum by rocket and satellite observations has provided many important new advances in solar research in the last decade. In this region of the spectrum occur the dominant emissions from the solar corona, where mechanical energy, generated in the solar outer convection zone, is deposited both in the form of steady heating and in violent events such as solar flares. Apart from teaching us more about coronal heating and the origin of flares and cosmic rays, EUV and x-ray observations of the sun, as the brightest astronomical object, also play a role in leading the way to the understanding of similar observations elsewhere in the universe.

The Orbiting Solar Observatory (OSO) program was started in the beginning of the last decade. The OSO's provide a platform for studying both rapid events and slow variations of radiation over time intervals up to one year. There has been steady improvement in the capabilities of these satellites. Early OSO's had virtually no spatial resolution and carried only small payloads. Rapid technological development will make it possible for the eighth OSO, to be flown in 1973, to carry instruments that attain a spatial resolution of ~ 1 sec of arc, comparable with that obtained with the better ground-based telescopes.

This program of continuous development and gradual improvement has made the OSO program among the most successful and productive of all astronomical satellite programs. We recommend the continuation of this program beyond the present OSO series, through OSO-L, -M, and -N (at a cost of $30 million each), to be flown during the next solar maximum (1977–1981). These OSO's will probably provide for the first time a spatial resolution equal to or better than that of the very best observations obtained from the ground or balloons. This improved spatial resolution is of utmost importance, since we know from ground-based observations that the energy transfer to the chromosphere, to flares and cosmic rays, and perhaps to the corona, occurs on scales probably less than or equal to 1 sec of arc.

OSO-L, -M, and -N will fly during the next period of maximum solar activity, with a spatial resolution 10 to 50 times better than was possible in the last period. They will carry instruments capable of analyzing the properties of flares and active regions in the spectral region from 3000 Å down to the very energetic x rays below 0.1 Å. It is entirely reasonable to expect that these observations will result in a significant increase of our understanding of the layers of the sun above the photosphere, of solar activity, and of solar flares.

We envisage this continued OSO program, together with the expanded solar rocket program discussed in the space astronomy recommendation, as the backbone of the solar space program. It is of the greatest importance, however, that improved observations from space go hand in hand with the improvement and extension of observations from the ground. The solar photosphere, best observed in visible and near infrared radiation, reveals most of the sources of the energy input in the chromosphere and corona in the form of granulation, magnetic structures, and mechanical motions. Coronagraphs, eclipse experiments, anticipated observations of far infrared recombination lines, and radio observations provide relatively inexpensive ways to observe other aspects of the sun's upper atmosphere. We therefore recommend the continuous updating of existing ground-based and aircraft facilities and the construction of small specialized telescopes for the visible and infrared spectral regions (at a cost of approximately $1.0 million per year). This updating includes improved image detection, storage, and analysis, as well as improvement of image quality by telescope refinement and site selection. For the study of the interaction of solar-flare plasma with the magnetic field and plasma of the outer solar corona, we suggest the construction of a relatively inexpensive multifrequency metric and decametric radioheliograph with moderate (1–5 min of arc) spatial resolution (at a cost of approximately $1.5 million). The cost of the program over the next decade will be $90 million for OSO-L, -M, and -N and $10.0 million for ground-based facilities.

THEORETICAL ASTROPHYSICS AND COMPUTING REQUIREMENTS

Physical theory has always played a crucial role in astronomy—from the period when Newton's theory of gravitation provided the explanation of planetary orbits to the present time, when nuclear reaction theory promises to explain the synthesis of chemical elements in supernova explosions. Any balanced program for progress in astronomy will necessarily contain a vital, if relatively inexpensive, program of theoretical research.

Much theoretical astrophysics today is concerned with model building. In this type of activity, physical principles substantiated in the laboratory, including those of quantum theory, nuclear physics, and plasma physics, are used to construct a mathematical model of an observable astronomical object, such as a star, a galaxy, or even the whole universe. The relevant equations are usually complex and nonlinear and must be solved on a computer. The resulting models are then compared with observations to fix parameters of the model, such as the mass of the star or the random velocities of stars in a galaxy, and to show how the model should be improved to attain agreement with observations. Model building is essentially the only way known to convert the stream of photons entering a telescope into a physical picture of what is going on.

The theoretical astrophysicist thus stands astride physics and astronomy. Close contact with physicists is essential if current developments there are to be properly included in the model. Constant interaction with observers is essential if theoretical work is to be aimed in the most productive directions for interpreting nature and if observational work is to be focused on the most theoretically significant questions.

In the recent past there has been increasing exploration of dynamic states. The theory of stellar evolution can be largely constructed from a sequence of static stellar models, but in the final stage of a star's life—in some ways the most interesting one—events occur very rapidly, with gravitational collapse and outgoing shock waves playing a vital role. To reconstruct these phenomena, it is vital to simulate the dynamics in a computer. Dynamical modeling is playing an ever-increasing role, from stellar explosions to interstellar shock waves to the spiral structure of galaxies. Such modeling is orders of magnitude more time-consuming than static modeling, so faster computers with larger memories are required. A prime example of the success of this approach is the modeling of a supernova explosion, in which the progress of a shock wave is followed in detail, and a network of about 100 nuclear reactions is followed at each time step. The result is a prediction of the abundances of the chemical elements, which seems to agree remarkably well with observation.

A related activity is theoretical work in dynamical astronomy—the application of Newton's equations of motion (with small relativistic corrections) to the positions of planets and satellites of the solar system. Here the problem is to compute the orbits using interactions between all bodies to extract precise values for the parameters of the system, including the masses of the bodies involved. Recently, such work has demonstrated its vitality by providing extremely accurate motions of the earth for use in reduction of optical observations of pulsars. Without these precise positions (about 10^{-8} of the distance to the sun), it would have been impossible to utilize the precise optical timing measurements, which require correction for light-travel time within the solar system. It would thus have been impossible to infer the existence of abrupt changes in the period of the Crab pulsar, which have been interpreted as due to starquakes in the crust of a neutron star. Such is the unity of astronomy, of the old and the new.

We believe that increasing the effort in the universities, where there is strong interaction of theoretical astrophysicists with both observers and physicists, is the best way to optimize results in theoretical research. We suggest particular emphasis on relativistic astrophysics, stellar evolution (particularly early and late phases), derivation of physical data needed to construct precise stellar models (including opacity sources, nuclear-energy generation rates, convection theory, and equations of state), and theoretical interstellar physics and chemistry (including the solid-state theory of grains, molecular and atomic cross sections and transition probabilities, the theory of masers, and the plasma physics of interstellar gas and magnetic fields).

Interaction between relatively isolated theoretical groups should be increased wherever possible, for example, between groups working on stellar interiors, stellar atmospheres, and observational stellar spectroscopy, between plasma theorists and astrophysicists working on stellar and interstellar plasma processes, and between chemists and astronomers working on molecular astronomy.

Support should be increased for both theoretical and experimental study of atomic and nuclear collision cross sections and transition probabilities, taking care to locate this work in several independent groups to increase the effectiveness of cross checking. By and large, this can be accomplished by supporting physicists in universities where there is an active astrophysics group that can be helpful in establishing priorities for experimentation and calculation. We recommend that in the specific areas of beam-foil spectroscopy and low-energy nuclear cross sections, the U.S. Atomic Energy Commission (AEC) consider support of groups utilizing existing facilities for this work.

Funds are needed for individual university investigators to increase their efforts using such university computers as are available. The funds available for computation generally need to be increased. Theoretical astrophysicists and dynamical astronomers are moving into an era when the maximum speed and storage capacity available will be needed to solve dynamical problems, but many university and national center computers are not equal to this task; selected ones should be upgraded. In addition, state-of-the-art computers in mission-oriented agencies such as the AEC and NASA would be extremely useful if means for using them part-time can be worked out. The additional funds needed for first-rate activity in this area are not trivial—perhaps $5 million per year.

The theoretical effort at the national observatories needs to be fostered. Research output would be optimized by increasing the availability of theoreticians at the national centers. To succeed, it is essential to find highly qualified versatile individuals as visitors or on the staff. Such a goal involves enhancing the computer facilities, as required, to make the observatory attractive both to resident and visiting theorists.

Joint activities between physics and astronomy programs in universities should be encouraged. Because of the close relationship of theoretical astrophysics to both physics and observational astronomy, productivity is served by every possible mode of cooperation, including, in some cases, merged departments, joint academic programs, and shared facilities. It is most important that astronomy PhD students receive as thorough training as possible in physics, and to this end, special seminars should be designed.

A National Institute of Theoretical Astrophysics has been suggested, to provide a focus for theoretical research, to promote interchange between astrophysicists from different subfields and between astrophysicists and other scientists, and to provide a stimulating atmosphere for postdoctoral fellows before they accept permanent appointments. A proposal by the Panel on Theoretical Astronomy would fund an institute at an annual rate of approximately $750,000 for a fixed period of seven years. The institute would have some six permanent staff members, with an outstanding scientist as director, and would be located in an attractive place close to a research university and close to a group of observational astronomers. There would be particular emphasis on postdoctoral and visiting appointments, and in keeping with the need to keep administrative and other expenses low, the support staff and computational facilities would be strictly limited.

The Committee concurs with the panel in the thrust of its recommendation for an institute. Nevertheless, it believes that for both pragmatic and historical reasons, the main strength of theoretical astrophysics is likely to remain in the universities. There it can have the greatest impact on the educational process and on young men from a wide diversity of backgrounds and fields of interest. The institute, if it is set up, should strengthen, not compete with, university groups. Emphasis on interaction between groups, on funding of young people, and on a moderate budget, which will suffice if the staff and computer facilities are limited, is consistent with this goal. We recommend, to this end, that if the institute postdoctoral fellowship program is established, it be used also for purposes not immediately related to attendance at the institute, including travel funds for visits to other institutions and the cost of computing at home institutions or other facilities.

While there are advantages in such a permanent institute, we recommend that, as a first step, consideration be given to smaller funding for a summer institute. Such an institute would have no permanent staff beyond the director and would occupy rented space at one of a number of possible sites that may prove attractive. No computation facilities would be provided; the entire funds beyond rental and minimal administrative expenses would be expended on travel and subsistence for a few senior and a larger number of junior people. We believe that the final plans for a possible permanent institute would be beneficially affected by one or two years' experience with such a summer institute.

Both the Theoretical Astrophysics Panel and the Committee wrestled at length with a problem that theoretical astrophysicists, along with others in all areas of theory, now face in their needs for a very large computer. Our conclusion may be viewed as suggesting something for everyone. We are probably in a state of transition from a stage in which large general-purpose university centers were optimum to a stage when the needs of many different research groups will share much larger computers through sophisticated data-communication links. We understand that quantum chemists have considered a national center with high-power computers, comprehensive software library, and staff of computer-oriented theoretical chemists, able to do large-scale service-type calculations for others. The needs of the Global Atmospheric Research Program suggest that an international network of large computers would be desirable. It will ultimately be necessary for scientists to assess these requirements and discuss the problems of a national computing system, making maximum use of facilities already in place, or needed, for calculations in industry, the space program, weather forecasting, and reactor design, among others. The needs of astronomy should be considered when such an overall national computing system is discussed.

Theoretical astrophysics is a growing field that attracts young astronomers and physicists with a broad range of interests. The speed of modern computers makes it possible to construct models of atoms, stars, and galaxies and to study the dynamics of the solar system or the universe. The tools of the theoretician, except for the large computers, are inexpensive. The pattern for the best range of computing facilities, national and local, must still be worked out. We recommend an increased program of about $3 million a year. For the theoretician, travel, to make new contacts and to attend summer institutes, performs a special function. Interdisciplinary research is particularly effective and not expensive. Theoreticians can work at small institutions, often at colleges or universities without large facilities.

OPTICAL SPACE ASTRONOMY—LEADING TO THE LARGE SPACE TELESCOPE

Some of the most far-reaching additions to our knowledge of the universe occurred during the first half of this century with the development of astronomical spectroscopy and its utilization with large telescopes. During this time, spectroscopic analysis of planetary atmospheres, the sun, the stars, and the interstellar medium brought about clarifications in our understanding of these objects. Of equal significance was the spectroscopic study of external galaxies, leading to the discovery of the increase of spectroscopic red shift with distance and the realization that we live in an expanding universe. Throughout this development, astronomers have been acutely conscious of the fact that their analyses were incomplete and tentative, since much of the information that they would have liked to have obtained was in the inaccessible ultraviolet range of wavelengths.

The missing spectroscopic information consists of two classes: one is the spectral lines in the ultraviolet due to elements and stages of ionization of elements that do not have lines in the visible region of the spectrum; the other is the general shape of the spectrum in the ultraviolet and the relation of this to the distribution of emitted energy in the visible and infrared wavelength regions.

Ultraviolet observations can be made only above the atmosphere. During the last 15 years, the technological barriers against such observations have progressively been broken. Rocket observations of the sun and the stars have resulted in a number of important discoveries concerning the ultraviolet spectrum of the brightest objects visible in space. At the same time, the discovery of quasars, some of them with large spectroscopic red shifts, has provided a means whereby the ultraviolet emission from a limited class of objects can be studied from the ground, because the light originally emitted in the ultraviolet has been red-shifted into the visible region of the spectrum.

Because objects emitting ultraviolet light are also likely to emit visible light, it has not been expected that completely new classes of objects would be discovered. Nevertheless, there have been a number of important discoveries made concerning the properties in the ultraviolet of some of the objects that had previously been studied in the visible:

1. The ultraviolet resonance lines in certain early-type stellar giants have shown that matter is streaming out from these stars with velocities of the order of 1000 km per sec, with total mass loss rates of the order of 10^{-7} solar mass per year.
2. The extinction of ultraviolet light by the interstellar medium has turned out to be different from that predicted on the basis of observations made in the visual region. There is a prominent absorption feature near 2200 Å and a gradual increase in the extinction toward shorter wavelengths. These results are leading to extensive revisions of our ideas concerning the character of interstellar grains, and the presence of considerable variations of these features in different parts of the interstellar medium indicates that individual stars can modify their interstellar environments.
3. Most galaxies have been found to emit more radiation in the shorter ultraviolet wavelengths than would have been expected on the basis of their apparent color temperatures in the visible region.
4. Large hydrogen clouds have been found surrounding the recent bright comets Tago-Sato-Kosaka and Bennett. Such large clouds appear to constitute a fourth major structural component of the comet.
5. A broad absorption feature at λ 2550 has been discovered in the spectrum of Mars, possibly due to ozone.

The Orbiting Astronomical Observatory program is becoming a true national facility for astronomers. On the first OAO, about ten groups of astronomers have been observing approximately 100 objects. The OAO-C is expected to have a considerably greater observing capability, and consequently it should be of great service to the astronomical community through the guest-observer program.

The Orbiting Astronomical Observatory program has, unfortunately, been marked by tragedy. The first and third launches were failures, the first through troubles with the battery, and the third through a failure in the launch vehicle. After the launch of OAO-C, there are no further authorized programs in space ultraviolet astronomy. At the present time, no satellite capable of carrying on intermediate spectral and spatial observations in the ultraviolet is funded.

The ultimate objective of the ultraviolet astronomy program should be the development of a National Space Observatory containing a large diffraction-limited telescope capable of operating in the near-infrared and visual regions as well as in the ultraviolet. The exciting role that such a large space telescope (LST) could play in astronomy during the decades to come is discussed in the final Section of this Chapter. The nominal aperture that has been utilized in studies of the LST is 120 in. Such an instrument could attack problems that are of the most fundamental astronomical significance and that are unlikely ever to be solved using ground-based instruments. Perhaps of even greater importance than its ultraviolet capability would be the high angular resolution of such a telescope. Turbulence in the atmosphere limits the angular resolution obtainable with large telescopes to the equivalent of that obtainable with a 12-in.-aperture telescope, although the light-gathering power of a larger instrument is superior. In the visible region, the LST would have an angular resolution better by a factor of 10, which means that one resolution element observed with a ground-based telescope could be divided into 100 resolution elements with the LST. The angular resolution in the ultraviolet would be still better by a factor near 2. One result of this high angular resolution should be the capability of observing stars and stellar-appearing objects at nearly ten times the distance at which such objects can now be studied with the 200-in. telescope. The LST should lead to a much improved understanding of the most fundamental problems in cosmology, as well as of the broad range of astronomical problems presently being investigated by ground-based astronomers.

A great deal of technological development will be required before such an LST can be launched. It will be desirable to test the new technology, not only through rocket instrumentation for ultraviolet studies but also through the construction and flight of intermediate instruments. For example, a diffraction-limited space telescope of about 60 in. would have a tremendously useful versatility and capability beginning to approach that of the LST itself. It is now technically feasible to build such an instrument, and it would be useful to incorporate into its design the results of new technological developments intended for the LST. Yet no high-quality large telescope is in the current planning stage.

The Committee recommends very strongly that a vigorous program be maintained in ultraviolet astronomy. This program should be directed toward the ultimate use of an LST. One or more intermediate instruments, designed to test the technology of the LST and to return large amounts of data of immense value to the astronomical community, should be launched. If there is to be an extended delay between the launch of OAO-C and the first of these intermediate instruments, then it is most desirable that an interim ultraviolet telescope be launched, perhaps a replacement for the OAO-B or a smaller instrument in a Small Astronomy Satellite.

The program for ultraviolet astronomy that we have outlined is a large one, leading, as it eventually should, toward a large space telescope as a major program for the next two decades of astronomy. Within it there is enough flexibility to provide ample trade-off possibilities between small-scale activities and larger instruments. If we cannot afford the largest diffraction-limited instrument soon, then a much more vigorous rocket and intermediate-size ultraviolet and infrared telescope program is needed to avoid losing all opportunities in this area. If, as appears likely, the 120-in. must be delayed to the mid-1980's, the 60-in. diffraction-limited telescope is an important prototype, giving both valuable experience and important scientific results. The cost of continuing the ultraviolet satellite program throughout the next decade at approximately the current level of expenditure ($35 million per year) is $350 million.

LARGE CENTIMETER-WAVE PARABOLOID

Large steerable paraboloids have been the basic instrument of radio astronomy. Within minutes, a modern radio dish can be converted from one frequency band to another, and its mode of operation can change from polarimetry to spectroscopy at the flick of a switch. Even major changes in receiving equipment, such as the installation of masers and other refrigerated amplifiers or the installation of radar transmitters, take only a few hours. This versatility has paid rich scientific dividends, especially in the study of time variation of radio sources, in spectrographic studies of the interstellar medium, and in studying the polarization of radio sources. Large steerable paraboloids have been essential elements in the recent developments of very-long-baseline interferometry (VLBI), in which the study of radio-source structure to angular resolutions of better than 0.001 sec of arc has been possible. They have geodetic applications.

Each larger instrument has, in its first few years of operation, produced new discoveries. Even a modest increase in size gives a surprising advantage, because the effective sensitivity, for observation in a given period of time, varies as the fourth power of the diameter. An additional advantage is the freedom, with a flexible instrument, to pursue occasional speculative programs. The recent explosive growth of discovery of new molecules in the interstellar medium provides an excellent example, as a new subbranch of astronomy—the chemistry of space—has started to grow.

The choice of instrument size, and of its wavelength capability (determined by the precision of its construction), has been carefully considered. An instrument whose diameter is approximately 440 ft would represent a significant step beyond any existing or planned steerable paraboloid, and it appears that a dish that performs well at 2 cm and is usable with somewhat reduced efficiency to 1-cm wavelength is well within present engineering practice. The largest comparable antenna, the 100-m telescope of Germany's Max Planck Institut, is actually only an 85-m telescope at wavelengths shorter than 6 cm. Thus the projected instrument has three times greater observing capability at all wavelengths, and at wavelengths of 6 cm and smaller over six times greater observing capability.

An especially attractive feature of the new paraboloid is its complementary role with our proposed millimeter-wave telescope. The simple basic molecules such as CO, CN, and CS have spectra that lie in the millimeter-wave region, while the larger, quasi-organic compounds such as methyl alcohol, formaldehyde, cyanoacetylene, and formic acid have spectral lines in the band from 2 to 30 cm. Many of the larger molecules, and ammonia, possess lines that could be observed with either system, although the greater angular resolving power of the 440-ft telescope would give it an advantage for certain problems.

The large centimeter-wave paraboloid would certainly serve as the hub of many VLBI observing programs, and its large area would increase enormously the classes of object accessible to study. In conjunction with the other large paraboloids of the world, stretching from Australia to the Soviet Union, the present observations of the closer, bright objects would be extended to quasars and radio galaxies that are far more distant and faint.

The radar capability of the new instrument would also be impressive. With the exception of Pluto, all the planets and the larger moons of Jupiter and Saturn would be within range of its 6-cm radar, while the greatly enhanced signal-to-noise ratio would enable the radar astronomers to study the surfaces of Venus and Mars in great detail, enhancing the effectiveness of space missions to those planets.

The estimated cost of such an installation, including the telescope, land acquisition, site development, controls, computers, radiometers, and radar, would be approximately $35 million. Some economies could be effected by sharing common support facilities with other instruments such as the very large array or the large millimeter-wave telescope. Operating costs would be $3.5 million per year following its completion.

ASTROMETRY

The establishment of a system of star positions based on an absolute inertial system is essential, and the system of proper motions should be determined with respect to such an inertial frame.

The mean proper motions of faint stars are of fundamental importance to the study of unusual stars found in the galactic halo. Many interesting objects in the halo are between 1 and 5 kpc from the galactic plane, and even with the rapid space motions of extreme halo stars, their angular proper motions are small—approximately 0.25 sec of arc per year. The motions must be determined with high individual accuracy. This requires that the inertial frame be determined to an accuracy of at least 0.005 sec of arc per year; ideally, the accuracy should be several times higher.

One type of fundamental data that astronomers must have is the distance to the object studied. Interesting objects are at great distances, which can be calibrated in successive steps if nearby objects of similar characteristics have accurate distance measurements. The most fundamental method uses accurate trigonometric parallax—the angular displacement of a star caused by the earth's motion about the sun. These parallaxes are the backbone of the stellar distance scale. They are needed for faint stars near the sun and for bright stars at greater distances. An insufficient number of trigonometric parallaxes in the southern hemisphere will reduce the benefits of the larger facilities built there by the United States and European countries.

Stars moving parallel in space appear to converge, because of perspective effects; this method provides individual distances for nearby star clusters. Cluster parallaxes should be extended to the southern hemisphere and to fainter clusters in the northern hemisphere. For other distant types of stars, we must take advantage of the accumulated drift provided by the motion of the sun through space, which causes the stars to drift backward at angular speeds proportional to their parallax. Such group or secular parallaxes are often the only possible distance measure for the most interesting stars of high luminosity. They depend directly on the accuracy of the fundamental system of proper motions.

Theories of stellar interiors would have a sounder basis if a sufficient number of parallaxes and masses of nearby stars and clusters could be provided. These should include interesting and important objects like rapid variables, highly luminous B stars, planetary nebulae, hot subdwarfs, bright white dwarfs, and cool red degenerate stars.

The establishment of the actual luminosity–temperature diagram for stars like the sun and fainter is essential for the determination of the distances to the globular clusters and the luminosities of the RR Lyrae stars. For these important determinations, a combination of trigonometric, cluster, secular parallaxes, and all other possible methods must be used.

Recently, the possibility has appeared of detecting companions of low mass by the nonlinearity of the motion of a nearby star through space. Several companions have been announced that have masses like that of

Jupiter—or even lower values. These astrometric binaries have been studied essentially in very few institutions, take a long time to give results, and yet will provide us with our only direct proof of the existence of other planetary systems until radio communication from some of these may eventually be received.

The changes of period detected in pulsars are fundamental to the theory of neutron stars. Yet the first observations of these changes were compromised by uncertainties in such supposedly well-known subjects as the orbits of the planets around the sun and the masses of the planets. The motion of the earth around the center of gravity of the earth–moon system is detectable in the accurate observations of the radio pulsars. Improved planetary orbits are necessary to take full advantage of this technique. Similarly, the very-long-baseline-interferometry technique requires accurate geodesy and accurate timekeeping.

The improvement and extension of astrometric measurements necessary to interpret the problems mentioned above rests ultimately on observations by small astrometric instruments. We therefore recommend construction of two automatic transit circles, three photographic zenith tubes, three astrolabes, and three automatic measuring engines, as well as modernization of several existing long-focus telescopes, the equipment to be located geographically so as to provide systematic observations in both the northern and southern hemispheres. The precision attained by these fundamental astrometric instruments has hardly been affected by modern electronic technology (except for the timekeeping function). However, the modern technology of automatic measurement is in fact successful, and we recommend it, together with some of the classical smaller telescopes mentioned above as part of our fundamental program.

The estimated cost of these small instruments is $6.4 million.

BEYOND THE RECOMMENDATIONS

After concluding a detailed study of the state of our science and making our recommendations within the framework of recent available funding, we feel that it is important to discuss, in certain areas, what additional programs our science requires to meet fully the scientific challenges that we face. We have therefore re-examined the manpower resources that will be available in the decade and the technologically feasible and desirable projects studied by the panels. What areas have we omitted, discarded, or reduced in size mostly because of financial constraints? How much have we failed to recommend of the urgent needs pressed by our technical panels?

Large Space Telescope

Without any doubt, the largest and most exciting area is the construction and launch of a large space telescope (LST), for high-resolution studies in the normal and ultraviolet spectral regions, possibly with manned resupply and maintenance (e.g., by the space shuttle). This development can be undertaken in a vigorous way only at budget levels for astronomy and physics that represent considerable growth over the next decade.

The LST concept is based on two major exploitations of the orbital environment. First, the mirror—from 60 to 120 in. in diameter, depending on available funds—will cover completely the wavelength interval from 1000 Å (the cutoff imposed by interstellar attenuation) to 10,000 Å (or 1 μm), with considerable utility out to 1 mm, thereby covering the entire ultraviolet and infrared range not accessible from the ground, as well as the optical window. The large collecting area and high angular resolution over this entire range would provide unmatched versatility.

But a more important dimension of the LST is the precision of its image in the ultraviolet and optical ranges. On the ground, the deleterious effects of atmospheric seeing smear the image to one or more seconds of arc even at an excellent site. This means that the observer is in effect comparing the image of the target object with that of the night sky (including background galactic light, zodiacal light, and airglow) in a comparable solid angle. If a 120-in. telescope can be designed to achieve diffraction limitation at 5000 A, an image as small as 0.04 sec of arc in diameter would result. If an image of 0.1 sec of arc can be achieved in practice, the night-sky radiation, which tends to obscure the image of a faint object, is effectively reduced by a factor of 100—a five-magnitude gain in sensitivity over ground-based instruments of comparable aperture. There is an additional gain from the fact that the telescope operates above the airglow layer and, of course, does not suffer from atmospheric attenuation. It should be possible to observe to apparent magnitude 29 in several hours of integration. The implications of such a capability for all branches of astronomy are great.

The Committee feels that the LST has extraordinary potential for a wide variety of astronomical uses and believes that it should be a major goal in any well-planned program of ground- and space-based astronomy.

The Committee recognizes that the large cost involved can be accommodated only within a vigorously growing program. Therefore, it has adopted the view that, within the main program, the emphasis on the LST is at a moderate level of some $35 million per year, enough to fund technological development of smaller-aperture telescopes and an LST in the following decade.

A much more expensive program is required if the LST is to become a reality in the 1980–1985 period. This Committee sees the LST as a natural program goal to follow the High Energy Astronomical Observatories (HEAO) mission. To achieve this will require budgets for diffraction-limited missions that grow from a level of the order of $20 million per year in 1970 to the order of $200 million per year in 1980, with launch scheduled for the early 1980's. Total cost of the program leading to the final fabrication of a 120-in. telescope will be of the order of $1 billion over 10 years.

A program of this magnitude requires the highest quality scientific leadership and the most advanced space engineering available. The highest quality scientific leadership in this field can be found in the academic community, and the highest degree of space engineering talent exists in the centers of the National Aeronautics and Space Administration. Therefore, the best chance for success lies in a merging of academic talent with that in the NASA centers.

We suggest that NASA select one or more centers to carry out the engineering phases of the program and that the National Academy of Sciences encourage the formation of a new corporate entity representing universities with strong programs in space astronomy. The latter should be limited to less than eight members in the interests of efficiency. This corporation would be responsible for establishing a National Ultraviolet Space Observatory (NUSO) — a working scientific laboratory under contract to NASA and the National Science Foundation. The Director of the NUSO should be a scientist of top rank in space astronomy.

The NUSO would be responsible for the planning and utilization of a series of satellite ultraviolet observatories, including the LST, and for administering them on behalf of the entire scientific community, as is done for the ground-based national observatories. To achieve this mission, the NUSO would work closely with the responsible NASA centers. Effective control of the engineering task of the NUSO would be exercised by NASA; effective control of the scientific direction would rest in the Director and in the Board to which he would report.

Optical- and Radio-Astronomy Instruments

Certain major facilities in optical and radio astronomy were omitted from our program, for reasons of economy. Optical astronomers could make effective use of two more telescopes in the 200-in. class, with modern electronic auxiliaries. The pressures generated by space and radio astronomy have so overcrowded the few large instruments that even the two 150-in. telescopes under construction fail to match the present needs. In addition to our recommended optical program, it would be desirable to double the effective collecting area of existing large telescopes. To accomplish this, at least two additional 200-in. telescopes or two equivalent-cost larger arrays or possibly one even larger array would have to be built in addition to those that we have recommended. Such a program would cost $50 million (with site development) plus the modern instrumentation described earlier in this chapter under Optical Astronomy—Electronic Technology and Light-Gathering Power.

Of the radio telescope systems planned, studied, and repeatedly recommended, one major item is omitted from our list of new starts. It is the only large, university-based plan that goes back to the Whitford report—the completion of the Owens Valley aperture-synthesis interferometer of 130-ft radio telescopes. The original plan required five additional antennas, tracks, receiver, and computer. The high quality of the mechanical design makes the present 130-ft good at 2 cm and possibly usable at 1 cm. An aperture-synthesis array working at high frequencies, usable for molecular and atomic lines, can be constructed for $15 million. Its beam, at 2 cm, will give 2 to 4 sec of arc resolution; its collecting area and sensitivity is about half that of the VLA. One advantage of the relatively small number of 130-ft antennas is the flexibility, ability to change rapidly, and reduced cost of the receivers needed to permit aperture synthesis, at high resolution, in molecular and atomic emission and absorption lines. In addition, the interferometer could be used for extragalactic astronomy at higher frequencies, providing data on the time-variable radio sources with flat or rising spectra.

Very-Long-Baseline Interferometry

A tremendous breakthrough in our ability to perceive fine details in radio sources has come in the last four years as the result of the development of very-long-baseline interferometry (VLBI). By using highly stable atomic clocks, high-speed magnetic recording, and modern computing techniques, antennas distributed over the entire world can now be used as elements of a single radio telescope. If we were to extrapolate from our present pioneering observations of the brightest sources and construct a vision of future developments, we could confidently sketch a technically feasible system that could construct complete maps of the details of quasars and interstellar masers. The present network of large antennas gives a sketchy view because there is a lack of intermediate spacings and north–south baselines. The situation could be remedied by the development of a mobile VLBI terminal, consisting of two dishes, one large and one small, plus the necessary atomic clocks and recording apparatus. The large dish would be designed to permit rapid assembly and disassembly, so that it could be transported to new locations. The small antenna would constantly monitor one of the stronger sources, to provide constant updating of the station clock. Several terminals would be needed, certainly at least two on each continent, although the best disposition would have to be determined by a careful study.

The resulting network, if operated at 1-cm wavelength (which recent observation of H_2O masers at 1.35 cm have shown to be feasible) could give us a complete picture of the radio structure of quasars, with 0.0001 sec of arc resolution. If our ideas of the distances of quasars are correct, we could see structures approximately 1 light-year in size and could follow the development of dynamic events from year to year, seeing the details of these enormously energetic events. There are other, more speculative areas that one can also foresee—the study of the coronas of other stars, the observation of their sunspots and flares, the study of supernova shell developments in other galaxies, and the analysis of the mysterious nuclei of Seyfert galaxies.

In addition to the VLBI program at radio wavelengths, we foresee the development of interferometer techniques at both infrared and optical wavelengths. Because the angular resolving power of an interferometer varies inversely with the wavelength, one can anticipate remarkable discoveries by such systems, rivaling the recent radio VLBI demonstration of motions apparently faster than light in a quasar explosion.

The ultimate instrument would be a 10-μm VLBI having a global baseline (10^4 km). Such a device would have a resolution of 10^{-7} sec of arc, permitting one to peer deep into a quasar, perhaps to see explosive events on the surface of a supermassive star, which, some say, powers a quasar. The surface features of exotic stars that sporadically shoot dust and molecules into interstellar space could also be studied. The choice of 10-μm wavelength is dictated partly by the fact that atmospheric phase shifts are small there, permitting the use of large apertures, and partly by the fact that quasars and red giants—key objects in relativistic astrophysics and molecular astronomy—radiate a major fraction of their energy there. The 10-μm VLBI might use a superheterodyne system, which mixes the incoming infrared signal with a stabilized CO_2 laser to produce a microwave signal that can be recorded at each telescope. The bandwidth of available tape recorders (10^8 Hz) should be sufficient to detect at least the brighter sources.

A forerunner of this device is now under construction, using line-of-sight transmission of a 10^9-Hz bandwidth microwave signal to a common point to form an interference pattern. Following tests of the system with a 0.1-km baseline (10^{-2} sec of arc), it will be expanded to 10 km (10^{-4} sec of arc). It will be sensitive enough to study nearby Seyfert galaxies and bright galactic objects, but a version sensitive enough to study quasars (where the resolution will be 1 light-year) will require larger telescopes and better detectors.

Of course, most astronomical objects emit more powerfully with visible light so that there also is need for devices that can work in that spectral range. Fundamental studies of angular sizes are possible with both the intensity interferometer, which correlates the intensities in the two signals, and the Michelson interferometer, which brings together the raw signals to form fringes. A large-intensity interferometer could be built immediately with a 1-km baseline to give 10^{-4} sec of arc resolution, but perfection of the Michelson system requires development of an optical delay line and techniques of fringe detection. The Optical Facilities Panel believes that both the delay line and fringe detection should be studied immediately with funding up to $200,000.

Beyond these preliminary investigations, worthy goals of a ten-year program include a sensitive 10-km infrared interferometer, and perhaps a 10^4-km infrared VLBI, and for visible wavelengths a 1- or 2-km intensity interferometer and a Michelson interferometer with a similar baseline. The sensitive 10-km infrared interferometer is estimated to cost $10 million over the decade, and the large intensity interferometer $4 million. Further studies are needed before the cost of the infrared VLBI or the Michelson interferometer can be estimated.

Infrared Astronomy

The growth of infrared technology resulted in discovery of quite unexpected objects that radiated most of their energy in the infrared. The energy maximum at 1500 K is at 2 μm and is observable from the ground. A survey with a 62-in. light collector discovered 20,000 cool stellar and prestellar objects. Observations in the far infrared are needed to study objects near 500 K, most of whose radiation falls in regions of high atmospheric absorption; to study objects at 50 K, observations above the atmosphere are needed. The Infrared Panel put highest priority on a large stratospheric telescope, about 120-in. in diameter, in a large, high-flying aircraft or possibly supported by balloons, gliders, or kites. We recommended funds only for study of the most economical mode of operating such a large infrared telescope, but the scientific goals of the large stratospheric telescope are extremely important. No realistic financial estimate can yet be made; both the study and experience with the NASA C-141 airplane (with a 36-in. telescope) will determine the best course of action. The infrared groups are small at many universities, in both astronomy and physics. The changes in technology, the availability of new detectors, and the revelations of new types of objects make this an unpredictable but challenging field. Interdisciplinary grants to physics and astrophysics departments will enlist the aid of low-temperature physicists for astrophysical applications.

Solar Physics

Solar physics has benefited enormously from the OSO series of solar observations. OSO's are rapidly becoming more sophisticated and more reliable. However, a large diffraction-limited solar telescope (about 40-in. diameter) is needed, carrying a heavy payload (over 1000 lb) and capable of accurate pointing and 0.1 sec of arc guiding. This will provide high spectral resolution in the optical and near ultraviolet and will permit very fine-scale study of the rapidly fluctuating solar plasma, its excitation temperature, velocity, and magnetic field. This is a large project, of the order of $200 million, but it is one that will both provide experience useful for the LST and be a nearly ultimate solar space telescope.

High-resolution observations of the radio sun provide information on the energetic particle acceleration process, as revealed by the gyrosynchrotron radiation. The relativistic electrons are studied near the site of the acceleration of solar cosmic-ray baryons. This study requires a high-resolution radio telescope with about 5 sec of arc resolution, which works on a short time scale and essentially gives a radio picture. A radio spectroheliograph in Australia has already demonstrated its usefulness in the study of the interaction of fast particles and the hot solar plasma and has shown that flares are triggered across the sun as disturbances run out through the corona or return to other active centers on the disk.

Theoretical Astrophysics

Facilities should not monopolize our attention. The present and planned facilities, the space astronomy program, and the importance of the field for itself justify a strong case for theoretical astrophysics, over the widest possible range of topics—study of neutron stars, the quieter phases of stellar evolution, planetary dynamics, galactic structure, supernovae, collapse nucleosynthesis, explosions in galaxies, black holes, relativity, and cosmology. A test of the concept and viability of an Institute of Theoretical Astrophysics is an inexpensive recommendation. Also linked to theoretical needs is a fourth- or fifth-generation computer at a single National Computing Center. The total cost of an Institute and Computer Center for 10 years might be $40 million. About 30 percent of our recent PhD's in astronomy have their degrees in, and wish to work in, theoretical astrophysics or dynamical astronomy. The issue of a National Computing Center is not clear-cut, since the efficiency and costs of high-speed long-distance lines are not yet known, but the very large computer is at the heart of much theoretical model building in astrophysics. To take advantage of the presently available theoretical talent among young astronomers and physicists, we also urge that an expanded postdoctoral and senior postdoctoral program be considered. The goal would be to provide a number of theoreticians with at least a summer's or, preferably, a year's visit to other universities, national, DOD, or NASA centers, by direct fellowship grants, with freedom to travel, or by small research grants covering their salaries and expenses.

CHAPTER SIX

Implementation of the Recommendations

THOUGHTS ON PLANNING

Introduction

We are at the end of a decade of outstanding success, marked by the revelations of such new and unexpected phenomena that we must be reminded of the great age of Galileo and Newton. What history led to the pattern of American astronomy today, with its record of achievement? How can this history guide us either in improving the system or, indeed, in finding any developing weaknesses in the pattern? Technical capability, funds, scientific competence, and careful planning are not in themselves creative, nor do they lead to great achievements in science; they provide the background against which the human intellectual drama is enacted.

The conditions that made the past decade so successful are complex; they include the ripeness of modern technology and especially electronics and computers, the development of reliable space vehicles, and the development of new types of detectors for all wavelengths. These factors have made it possible to turn astronomy from an observational into an experimental science. We may now ask questions, and hope for significant answers, about a celestial object at essentially all wavelengths. This all-wavelength capability is provided in part by the successes of space, radio, and infrared astronomy. Combined with the rich tradition of ground-based optical astronomy, it has produced an explosion of experimental knowledge. Novel experiments are enriched by the traditional, and the theoretical tools of atomic and nuclear physics, plasma physics, and solid-state physics are applied to objects of intrinsic beauty and strangeness, to

analyze and to attempt to understand. In addition, we have developed a critical testing ground and stimulus for advanced technology, which must eventually find important practical applications.

The all-wavelength capability provided by space astronomy is relatively expensive; yet the discoveries can be made no other way. Combined with observations of the same objects at other wavelengths from the ground, the usefulness of space observations is greatly enhanced.

A Lesson of the Decade—The Need for Balance

In Chapter 4 we noted that the available funds per astronomer have dropped during this decade. But in spite of the leveling off of federal funding, we are in an enormously productive period. What historical background led to our present favorable condition?

Optical astronomy has a long tradition in universities and observatories, founded by the states and individual philanthropic largess. After World War II, and especially following the completion of the privately endowed, 200-in. Hale telescope, it was obvious that we needed to develop large facilities available to a wide section of the growing astronomical community. A consortium of universities, Associated Universities for Research in Astronomy (AURA), established the Kitt Peak National Observatory (KPNO), funded by the National Science Foundation and open to all qualified users. It has two main functions: first, it provides facilities for those who do not have such equipment, and, second, it provides major instruments for work on forefront problems. In 1971, the Kitt Peak telescopes were used by astronomers from 33 small institutions, which presumably lacked any modern large instrument; astronomers from 24 larger university or observatory groups also used the telescopes. The observatory has served a large fraction of the astronomical centers of the country, with a broad geographic distribution. Few university or private facilities rival in size and diversity the KPNO operation, and none can match it in terms of manpower, capital, and operating funds.

For the last few years, the AURA group has been actively engaged in construction of two large optical telescopes, 150 in. in aperture, one in Arizona and the other in Chile, together with sophisticated instrumentation. The Chilean development provides an excellent site also suitable for infrared observations and a needed U.S. facility in the southern hemisphere.

Radio astronomy, too, had its origins in the universities, with a history beginning after World War II. A government research laboratory (the Naval Research Laboratory) and several university-based installations largely funded by the Department of Defense provided the backbone. The need was felt for more generally available equipment, but in addition, new major instruments were desperately needed. The result was the founding of the National Radio Astronomy Observatory (NRAO) by a university consortium, Associated Universities, Inc. (AUI). A national, rather than a single university-based, facility was required to serve all astronomers. Its large instruments were considered to involve capital investments and support groups too large to be handled by a single university. NRAO has unrivaled equipment in addition to its large permanent scientific and technical staff. Recently, a second large radio facility, the Arecibo Observatory, became a National Science Foundation center, with the same goals as NRAO of serving permanent staff and outside visitors.

But if all of this is provided at national centers, why should there be any university-based observatories? The reasons are compelling; the vitality of the U.S. astronomical effort originated in the universities. Future progress would be restricted if insufficient support were given to such already developed centers. The majority of the talented astronomers are located in the universities. They need adequate support if the flow of young scientists, new scientific ideas, and technical innovations is to continue.

For space projects, the National Aeronautics and Space Administration (NASA) centers are essential to the engineering of much of the flight hardware. Few complete satellite systems can be handled by university groups, and even rocket flights normally require specialized support for launch operations, pointing controls, and telemetry. Nevertheless, rocket programs do provide an excellent opportunity for university scientists to obtain experience in space astronomy, to test new instruments, and to train graduate students. For the larger satellite projects, it is usually appropriate to have the engineers at a NASA center responsible for the basic spacecraft. In principle, all experiment packages are open on a competitive basis to the entire scientific community, including industrial laboratories, other government laboratories, NASA centers, and university-centered research groups.

The statistical material in Chapter 4 shows the diversity of sources of funding for various branches of astronomy, involving a considerable number of federal agencies. The National Science Foundation is designated as the lead agency for support of ground-based astronomy, although its funds have been limited. Certain parts of astronomical research were close enough to problems of interest to the national defense that the Department of Defense has been traditionally involved as a funding agency. NASA expends part of its basic research funding on astronomy-related topics. The extraordinarily good health of astronomy in the last decade depended on a well-balanced mix between space and ground-based techniques. It involved transfer of the sophisticated experimental

techniques of the physics laboratory into laboratories in space and on the ground. It has involved a growing young segment of the astronomical population in experiments using a remarkably wide range of techniques. It was funded in part by the state universities, by private universities and foundations, and by a considerable number of federal agencies. Balance is the essential ingredient.

Planning a Balanced Program for the 1970's

We believe that a similar broad base of funding and a wide range of modes of operation holds the greatest promise for future accomplishments. One difficult question is the best balance between expenditures at national centers, or national observatories, as compared with those at smaller government laboratories, industrial laboratories and companies, or universities and their observatories. Productivity has been high under very different management auspices, but there are important differences in management style and research style, and no one pattern is likely to be successful.

We recommend that for flight experiments, sponsored by NASA, the choice of payloads be based on scientific merit, technical competence, cost and compatibility with other experiments, without regard to the nature of the proposing institution. NASA should continue to seek the advice of outside scientists in choosing flight packages; these scientists should be further consulted if a final payload is to differ significantly from that originally recommended by them. An institution or center awarded a flight experiment should be responsible for the design, fabrication, testing operation, and data analysis for that experiment. We urge that sufficient funds for data analysis be included in original planning, which has not always been the case. On all programmable observatories in space, significant amounts of observing time should be available to outside investigators with worthwhile proposals.

For some years, NASA supported a small number of research groups at individual universities, which either had experiment packages in orbit, were analyzing data, or were preparing new equipment for future flights. The support for such university centers has drastically decreased in recent years. The NASA traineeship program has been terminated. Since costs of experiment packages are high, university-centered groups in space astronomy depend almost completely on federal funding. High competence in design, operation, and interpretation of space experiments now exists in universities and organizations outside of NASA centers. It is critical that NASA recognize its responsibility to these outside institutions as well as to its own centers. It should direct its available resources to maintain the best groups in each field, including ground-based astronomy in support of space missions.

Turning to ground-based astronomy, largely funded by the National Science Foundation (NSF), recent trends and our newly recommended program raise questions about the possibility of maintaining the properly balanced effort, which we feel to be the most efficient mode of operation. The very large new research facilities funded by the NSF have been based at the national optical and radio observatories, and recently at the Arecibo radio observatory; recent large optical telescopes at universities are the 107-in. (Texas), 90-in. (Arizona), and 88-in. (Hawaii). (The Texas and Hawaii instruments were largely supported by NASA.) Obligations for major research facilities and equipment at universities have averaged only 1 percent of the NSF astronomy budget in the last four fiscal years, as compared with 6 percent in the previous four years (see Chapter 4). Basic research grants to astronomy groups in universities, which are the essential life blood of university-centered research, have been only about 25 percent of the budgets of the national observatories. Astronomers at smaller institutions, without their own facilities, are dependent on, and grateful to, the national observatories for providing generally utilizable facilities; but, without local support by research grants, the health of the wide community is threatened and the usefulness of the centers reduced.

Of the large facilities recommended in our new program, some, but not all, are likely to be considered as part of the program for the national observatories. The national observatories can remain a central part of the national effort in ground-based astronomy, without automatically processing each new instrument. The policy questions that must be considered in allocating resources are (1) What size or cost must a facility reach to be viewed as the responsibility of the national observatories? (2) Where can new auxiliary instruments (sensors, electronic devices, TV, and data-handling systems) and unique, even if expensive, types of new instruments best be developed? (3) How can we best enhance interaction between all groups to stimulate each other to the highest level of efficiency and economy? These questions may be answered differently in individual cases. There is no obvious cutoff in size or cost beyond which a single institution might not compete efficiently in design, construction, and use of a new device. The largest optical telescope is privately owned and operated, but, clearly, any major instrument largely funded by public monies must be available for a proper fraction of its time to all qualified users, whatever their institutional affiliation. "Nationally available," however, is not identical with "nationally designed and operated," just as "privately owned" telescopes are, in part, nationally supported and available to qualified outside users.

Differences in style have existed between the universities and private optical observatories and the national centers. At the former, long-duration programs have been carried out involving extended periods for observation of objects difficult to observe. Many of the scientific goals for the coming decade will involve very faint objects and require extended observational programs. This style of operation may thus become more common (for at least part of the available time) at the national observatories. Another question of style concerns the reliability and simplicity of use of auxiliary equipment supplied to the often less-experienced users visiting Kitt Peak and Cerro Tololo. This has placed different requirements on electronic equipment developed there from those imposed on the one-of-a-kind, state-of-the-art equipment used or planned for at Lick, McDonald, Mount Wilson, and Palomar. Yet the abundant resources of the national observatories have permitted individual experimentation with advanced devices by their permanent staff, and their telescopes have been sufficiently flexible to allow use by visitors who bring their own advanced equipment. During a period when funding does not increase rapidly, readjustments in the program for basic, current support and construction of facilities may be needed for the most efficient use of available funds. A fruitful interplay between the different styles of operation is possible only if healthy operating budgets exist at a variety of places.

Our studies emphasize large facilities and the support of good existing large groups, because of the difficulty of observing from space and the ground the interesting objects, which are most often the faintest. In this context, it is inefficient for the total number of small groups to grow at the expense of the large facilities required by the scientific problems. Existing large centers can make available nationally, in a democratic manner, a proper fraction of their time to outside visitors. We also believe that larger instruments than have hitherto been built at universities with federal funds could well be maintained at universities in the future. The national centers and observatories provide opportunities for long programs by their permanent staff, principal investigators, and meritorious visitors.

In addition, relatively small sums spent on travel grants, summer institutes, and visiting appointments at large centers are efficient, encouraging scientific creativity.

It is easy to advocate diversity in styles of scientific research and management, although hard to implement in detail. Funds and facilities should be put at places where there is the best possible management and scientific talent, with a policy that accommodates the diversity in styles of both the best-established scientists and the talented outsiders. All should have ample opportunity to display their talents and to use instruments as efficiently as possible.

One question proved difficult for the Committee to answer. It involved the possibility of international cost-sharing in major facilities construction. Several of us explored this privately with radio-astronomy observatory directors and science planners from other countries. Since the world of astronomical research divides fundamentally into two hemispheres, north and south, a fruitful opportunity for international cooperation arises where large facilities are to be built for both hemispheres. Our private conversations show that astronomers from other countries naturally favor international cooperation where U.S. technology is outstanding. One particular area for special exploration is a millimeter-wave radio dish in the southern hemisphere; several countries seemed strongly interested, and the British have funding for such a device. From Europe, where large centimeter-wave telescopes are built or planned, suggestions were made for exchange of observing time for use of the millimeter-wave radio telescope in the United States, when built. The high U.S. capability in receiver technology led to the recommendation, from our foreign colleagues, that the NRAO help by building receivers over the wide range of radio frequencies. A suggested long-range plan was an international radio-astronomy facility with European cooperation, similar to the CERN program for high-energy physics, for the 1980's.

A number of European countries are building 150-in. telescopes in the southern hemisphere; unless a very-large-collecting-area, optical-array telescope becomes feasible, there is little prospect of more than temporary exchanges of facilities. An interesting possibility is international collaboration for the development of a new, dark sky, dry location in the northern hemisphere. Few possibilities remain within the continental United States; cooperative efforts with Mexican astronomers may be realistic and attractive.

Some cooperation in space experiments exists currently; the lead held by the United States has permitted other countries to use our spacecraft technology without facing independent development costs. Such tendencies can be encouraged for the future, although we have not explored them specifically. Programs for ultraviolet astronomy leading toward the large space telescope, however, are so large that international cooperation, and possibly cost-sharing, would seem highly desirable, even at an early stage.

PHILOSOPHY OF PRIORITIES

Within our field of study, which was large, we considered a program substantially supported by the federal government, which we discuss in some detail in Chapter 4. The approximate figures for research and facilities are near $230 million per year from NASA (of which nearly half is support overhead for NASA facilities), $30 million per year from the NSF, and $10 million per year from the DOD. The nonfederal budget (largely universities) lies near $30 million per year. The accumulated nonfederal capitalization is effectively larger than one would derive from the numbers we give, because it was provided at lower prices and involves university buildings and facilities of high replacement cost.

The program outlined in Chapters 2 and 5 is in addition to current base level of support. We are not specific about individual rocket flights, research projects, or small facilities, and we hope that the level of support for these efforts will increase. In these competitive and quickly changing areas resides the current health of most of the astronomical community.

The total spent on individual research projects from federal grant or contracts within the current program has been about level, maintaining a balance between terminations and new starts. New research areas and opportunities develop, and shifts of funding emphasis follow the excitement and promise of the science. Our recommendations do not consider in detail shifts of capital or research grants within the present funding level. Here, and in the panel studies in Volume 2, we emphasize certain areas as promising for the next decade, thereby providing some guidance to funding agencies. What effect will the recommended program have on the current level of expenditure?

Space

One large effect will be on the NASA astronomy program, where the High Energy Astronomical Observatory (Recommendation 4) will represent a virtually new effort and involve an estimated total cost of $380 million. Doubling the aircraft, balloon, and rocket program (Recommendation 6) would add $13 million per year to the NASA program; the solar satellites (Recommendation 7) and ultraviolet program (Recommendation 9) are essentially continuations (or enlargements) of ongoing efforts. The net effect of the Committee's recommendations, ignoring year-to-year fluctuations, would be to increase the direct NASA astronomy program from $130 million to $180 million per year and the total NASA contribution (including internal overhead) from $230 million to $280 million per year.

Radio

The Committee's recommendations in the area of radio astronomy call for an expenditure for capital equipment of $62 million for the Very Large Array (Recommendation 1), $10 million for a Large Millimeter-Wave Dish

(Recommendation 5), and $35 million for a Large Centimeter-Wave Dish (Recommendation 10). To this must be added for operating expenses 10 percent per year of the capital costs, i.e., $11 million per year at the end of the decade when the three major facilities would be completed. The Committee also recommends an additional $2.5 million per year for the construction of new university radio facilities. The magnitude of the program—$110 million in capital expenditure ($11 million per year over the coming decade) and a growth in support reaching $13.5 million per year—is a reflection of the fact that virtually none of the programs recommended for radio astronomy by the Whitford panel in 1964 has yet been implemented.

Optical, Infrared, Solar, and Theoretical

In the area of optical astronomy, we strongly urge that $1.5 million per year be expended for development and installation of electrooptical detectors and automation of the largest existing telescopes. We call for a capital expenditure of $5 million for an optical array, $25 million for a large optical array or single-mirror telescope, and $15 million for the construction of three intermediate-sized telescopes—one at a dark site, one for infrared observations (see below), and one to support the High Energy Astronomical Observatory (discussed above). The program thus involves $1.5 million per year for upgrading existing instruments and a capital expenditure of $45 million, i.e., an average of $4.5 million per year over the decade, together with operating expenses growing to $4.5 million per year at the end of the decade. The Committee's infrared program, in addition to the intermediate-sized telescope mentioned above, involves a number of programs entailing an additional $2 million per year for support of this discipline. The solar program calls for $1 million per year for improved ground-based facilities. The Committee recommends increased support for theoretical astrophysics amounting to an additional $3 million per year; it also calls for capital expenditures in dynamical astronomy amounting to $6.5 million.

Thus the Committee's recommendations in the area of ground-based astronomy involve capital expenditures of $160 million, or $16 million per year over the decade, and increased support and operating funds for new facilities growing to $25 million per year by the end of the decade.

Priorities and Alternatives

The above program calls for a growth in the federal support for basic research in astronomy from $270 million per year to average $355 million per year over the course of the next decade, a program that could be accomplished with a growth rate for funding averaging 5½ percent per year. During a time when funding levels tend to be declining in real dollars, even so modest a growth rate might seem unrealistically high. What alternatives exist if funding does not grow this rapidly?

The Committee has attempted to deal with part of this question by establishing priorities among the programs it recommends. The first four programs, recommended for immediate funding, are ranked in order of importance. They comprise a group of higher-priority recommendations than the remaining seven, within which the order of listing is not significant. If funding is not immediately available for the initiation of large capital programs, the Committee would recommend implementation of its lower-cost programs of highest priority—increased support for university radio facilities together with possible initiation of the large millimeter-wave dish (which may be the lowest-cost major radio facility); detector development and implementation for optical astronomy; increased support for infrared astronomy; and increased support for aircraft, rocket, and balloon astronomy.

The further question might well arise as to the extent to which the program might be funded out of existing resources, by omitting or curtailing portions of the current effort. Let us examine a bit more closely how current resources are utilized. Of the $230 million per year that NASA devotes to basic research in astronomy, $100 million involves indirect support for NASA centers and management. This cannot be reprogrammed to carry out new flight efforts. The remaining $130 million maintains the flight programs in solar and ultraviolet astronomy; airplanes, rockets, and balloons; astronomical experiments on certain interplanetary missions; and some ground-based programs. Since the High Energy Astronomical Observatory (HEAO) program is essentially a new effort in this extremely important wavelength region, any attempt to support this program out of funds already authorized for current programs would entail the virtual abandonment of entire areas of space science such as the solar or ultraviolet programs—programs that the Committee recommends be continued at or above the present level of effort. The Committee places the highest priority on the HEAO program but hopes it could be carried out without eliminating the other important space-astronomy programs. Since the recommendation for increased support for airplane, rocket, and balloon astronomy involves only 10 percent of the present effort, it could possibly be implemented without a significant increase in funding.

It is clear that funding for the major new ground-based radio facilities recommended by the Committee, such as the Very Large Array (VLA), also cannot come from within the current budget. The total NSF budget for astronomy facilities and equipment amounted to only $4.4 million in fiscal year 1971; this amounts to $44 million over the next decade—considerably less than the $62 million estimate for the VLA alone. The NSF support for operations at the National Radio Astronomy Observatory and the Arecibo Observatory amounted to only $8.5 million in fiscal year 1971; clearly, even drastic reprogramming would not produce significant funds for new programs. The situation is even more critical for current support of the national optical observatories. Support for the new national facilities (Kitt Peak and Cerro Tololo) is just beginning, with two major telescopes being built. Support for auxiliary instrumentation will have to be increased within that current program.

What about additional areas in the budget? The only other significant line item in the NSF astronomy budget is the Basic Research Grants, amounting to $6.5 million in fiscal year 1971. This is one of the most important budget items—the life blood of university research in astronomy. It is already too small to support current research needs; it makes no sense to reduce research support in order to construct new facilities, which could then not be used.

The Committee believes that, in order to implement to any significant degree its recommended program, a modest but nevertheless real growth in the federal support of astronomy amounting to some 5½ percent per year is a fundamental necessity. Without such growth it will be virtually impossible to carry out the program of exploration on whose threshold astronomy now stands.

Why do we feel now such an urgency in the need for major facilities and expanded research support? Astronomy has been in an explosive and happy situation of rapid discovery of unexpected objects that have required re-examination of our concepts of the nature of the universe and the forces within it. These discoveries have been made, however, with instruments designed, and largely constructed, before the 1964 Whitford report. The past few years have not seen a level of investment in new facilities of the magnitude necessary to maintain this momentum. We fear that this lack of support will be reflected in a diminishing scientific return during the coming years; if, on the other hand, new facilities are constructed, the expectations for science are correspondingly great.

We must stress that we live in a particularly fortunate time and place; modern technology and the all-wavelength capability of space, radio, infrared, and optical techniques together provide us with powerful tools, within a science where major discoveries are still commonplace. The importance of radio astronomy for the detection of prebiological molecules and ultimately, perhaps, intelligent life, suggests the need for new funds for what may become a new science. Auxiliary electronic, television, and data-handling equipment for large telescopes bring us to an unprecedentedly high level of electrooptic technology. These large telescopes are needed to support the results found in space, by radio telescopes and by infrared. Without them, a much lower total scientific productivity can be expected in these newer fields.

A number of our recommendations urge increase in the level of support of certain research areas. We concur with other studies recommending a doubling of the effort in space research from aircraft, balloons, and rockets. The new discipline of infrared astronomy has proved very fruitful; its facilities and research support must be enhanced to take advantage of both modern technology and the important scientific link the infrared provides between physical processes in the radio and optical regions. Theoretical astrophysics, whose tool is the large computer, is a relatively inexpensive subject that needs increased funds. Finally, we face the very expensive long-range program that would lead toward the large space telescope (LST). Ultraviolet astronomy prior to the LST may be supported within NASA's current budget with suitable reorientation. But the LST, the ultimate tool of ultraviolet and optical space astronomy, with man involved, may become a major component of the manned space program. Astronomy has provided leadership for technology by providing extraordinary demands, which technology has met. We have, here, tried to foresee what technology can provide, and what the next decade of astronomy requires to maintain its present rate of advance.

APPENDIX:

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Index